Proceedings of the Institution of Electrical Engineers

Institution of Electrical Engineers

No. 217 MAR 25 1913 FEBRUARY, 1913. VOL. 50.

JOURNAL

OF

THE INSTITUTION OF ELECTRICAL ENGINEERS,

ORIGINALLY

THE SOCIETY OF TELEGRAPH ENGINEERS.

FOUNDED 1871. INCORPORATED 1883.

EDITED BY P. F. ROWELL, SECRETARY.

CONTENTS

E. AND F. N. SPON, LIMITED, 57, HAYMARKET, LONDON, S.W.

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CORRIGENDUM.

Vol. 49, p. 884, lines 29–30: *delete* "of telephone working."

The Institution is not, as a body, responsible for the opinions expressed by individual authors or speakers.

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JOURNAL

OF

The Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. 50. 1913. NO. 217.

Proceedings of the Five Hundred and Forty-third Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 14th November, 1912—Dr. S. Z. DE FERRANTI, Past-President, and Mr. W. DUDDELL, F.R.S., President, successively in the chair.

The Minutes of the Annual General Meeting, held on 16th May, 1912, were taken as read, and confirmed.

The following list of transfers was presented as having been approved by the Council :—

TRANSFERS.

From the class of Associate Members to that of Members :—

Albert Arthur Blackburn.
Frederick Thomas Hall.
Laurence Joseph Kettle.
William Noble.
Henry Villiers Pegg.
Thomas Fortune Purves.
Sidney Arthur Simon, B.A.

From the class of Associates to that of Members :—

Gordon Layton.
Arthur Edward Loos.
Anthony Clark McWhirter.

From the class of Associates to that of Associate Members :—

Captain Basil Condon Battye, R.E.
Francis John Chapple.
Wilford Henry Taylor.

From the class of Students to that of Associate Members:—

Charles Gordon Barker.
Francis William Bissett.
Thomas Alfred Brown.
William Richard Brown.
William Anthony A. Burgess.
John Lowry H. Cooper.
Edric Collingwood Creagh.
Richard Hill A. Deane.
Richard Francies.
George Yalden Fraser.
Cecil Hugh Hayward.
Charles Wells Hirst.
Walter Daniel C. Juritz.
Norman Mitchell.
Ernest Vincent Pannell.
Henry Edward Parry.
Charles Joseph Polden.
James Francis Riley.
Sidney Simpson.
John Bertram Sparks.

From the class of Students to that of Associates:—

Arthur Liddiard Annison.

The list of candidates for election and transfer approved by the Council for ballot was taken as read, and it was ordered that it should be suspended in the Hall.

Donations to the *Library* were announced as having been received since the last meeting from The American Society of Mechanical Engineers, G. E. Bairsto, W. H. Booth, J. H. Bowden, C. Bright, F.R.S.E., H. H. Broughton, The Canadian Department of Mines, E. C. Clement, Professor W. C. Clinton, Congresso Internazionale delle Applicazione Elettriche, Dansk Elektroteknisk Forening, Baron A. Danvers, W. Dubilier, The Electrician Printing and Publishing Company, Ltd., The Engineering Standards Committee, The Faraday Society, B. Gáti, Gauthier-Villars, J. M. Gledhill, J. Grosselin, Professor A. Hay, A. Hermann et Fils, E. G. Hillier, T. Hirobe, The International Electrotechnical Commission, Dr. G. Kapp, Professor A. E. Kennelly, Laboratoire Centrale d'Électricité, Professor C. G. Lamb, Liverpool University, Macmillan & Co., Ltd., H. Matsumoto, W. P. Maycock, The Metallic Composition Co., R. E. Neale, The Hon. Sir C. A. Parsons, K.C.B., H.M. Patent Office, The Royal Society, J. Rymer-Jones, W. O. Schumann, Schweizerische Elektrotechnische Verein, Siemens Brothers & Co., Ltd., Sveriges Allmänna Exportförening, A. A. C. Swinton, F. Tait, The Underfeed Stoker Company, Ltd., The United States Bureau of Standards, University College (London), Verein deutscher Ingeniere, W. J. White, Dr. H. Wilde, and Professor E. Wilson; to the *Museum* from R. Hammond and K. Hedges; to the *Building Fund* from W. A. Del Mar, F. H. Nicholson, A. von Siemens, N. Tesla, and A. H. Unwin; and to the *Benevolent Fund* from S. Evershed, The Electrical Engineers' Ball Committee, The Foster Engineering Company, Ltd., J. G. Lorrain, F. H. Nicholson, F. C. Raphael, C. P. Sparks, W. C. P. Tapper, The "Z" Electric Lamp Manufacturing Company, Ltd., and The "25 Club," to whom the thanks of the meeting were duly accorded.

The CHAIRMAN (Dr. S. Z. de Ferranti): I have very much pleasure in announcing that Professor Silvanus Thompson has presented to the Institution a medallion of Dr. William Gilbert.

I now propose to read to you the names of the members of the Industrial Committee which has been appointed by the Council :—

INDUSTRIAL COMMITTEE.

W. Duddell, F.R.S. (*President*),
H. Hirst (*Chairman of the Committee*),

A. B. Anderson,	S. Morse,
G. Balfour,	A. H. Preece,
G. H. Bowden,	H. Faraday Proctor,
D. N. Dunlop,	W. Rutherford,
S. Z. de Ferranti,	A. H. Seabrook,
E. Garcke,	A. Siemens,
F. Gill,	Dane Sinclair,
Godfrey Isaacs,	C. P. Sparks,
H. C. Levis,	A. H. Stanley,
C. H. Merz,	A. de Turckheim,
R. K. Morcom,	F. J. Walker,
W. M. Morrison,	H. E. Wimperis, M.A.

The Chairman then presented the premiums and scholarships referred to in the Annual Report* for the year 1911–12.

The CHAIRMAN: I now come in a sense with very great regret to the last act that I have to perform as President of this Institution. The Presidency has involved a great deal of work and a great deal of time, and I feel that to cease to be President is a great relief, and in my case an absolutely necessary relief. And yet I vacate the position of President with the greatest possible regret, because I have found the work so interesting and altogether delightful. My regret, however, is almost entirely nullified and turned into a pleasure when I remember whom I have to introduce to you as the new President. Everybody knows Mr. Duddell as a most distinguished scientist; but few know what a wonderful worker he has been in the interests of the Institution. The last thing that I say to you in my capacity of President is to express the opinion, if I may be allowed to do so without impertinence, that I have no doubt whatever he will be a most complete and unqualified success in his conduct of the Institution's business and affairs. I introduce Mr. Duddell to you as your President.

The chair was then vacated by Dr. Ferranti, and taken, amid very hearty cheering, by the new President, Mr. W. Duddell, F.R.S.

Mr. ALEXANDER SIEMENS: As the oldest past-president here, it is my very pleasant duty to give expression to the thanks of the Institution to our past-president, Dr. Ferranti. I think I can claim that I have known Dr. Ferranti as an electrical engineer a great deal longer than anybody else; because in the dark ages—I mean in 1879—the Electrical Engineer of the British Museum came to me and said, "I have a young man named Ferranti; will you take him as your pupil?" I said,

* *Journal of the Institution of Electrical Engineers*, vol. 49, p. 659, 1912.

"Well, we will try him!" He came to us at Woolwich entirely as a learner, if I may say so, but he very soon managed to get into the good graces of Sir William Siemens and helped him in his research work, more particularly, I think, at the Fisheries Exhibition in showing the electric furnace which Sir William Siemens at that time tried to develop. I will not weary you with all I could tell you about Dr. Ferranti; his last words to you to-night indicate what a great influence he has had during his Presidential career. We certainly shall feel the influence of his Presidency for a long time to come. I have much pleasure in moving: "That the best thanks of the members of the Institution of Electrical Engineers be given to Dr. S. Z. de Ferranti for the very able manner in which he has filled the office of President during the past twenty-four months."

Dr. SILVANUS P. THOMPSON: I have the honour of seconding the motion which Mr. Alexander Siemens has put before the meeting. Once a year the past-presidents have an opportunity of returning to their scenes of activity, and of moving and seconding votes of thanks to the retiring president. Never on any previous occasion—and I have been present at a good many—has the vote of thanks been proposed and seconded with better goodwill than that with which it is proposed and seconded to-night. We have had an unusual term of office in Dr. Ferranti's Presidency, and I venture to say that the obvious prosperity that at the present moment is almost overwhelming the officials of the Institution is very largely due to his activities during the two years of his office. The Institution for several years to come will owe a very great debt of gratitude to its retiring president; and as he now takes his place among us, the extinct volcanoes, you will have the opportunity of hearing him from time to time proposing or seconding votes of thanks to the future retiring presidents of this ever-enlarging Institution. I beg to second the motion.

The PRESIDENT: I think I need not put the motion to the meeting after the very cordial and hearty way in which you have responded to it by acclamation. Dr. Ferranti has been our excellent president for the last two years, and I am sure we are all very sorry indeed that we are losing him as our figure-head.

The motion was then carried by acclamation.

Dr. S. Z. DE FERRANTI: I thank you very much indeed for your kind expression of thanks.

The PRESIDENT then delivered the following Inaugural Address:

INAUGURAL ADDRESS.

BY WILLIAM DUDDELL, F.R.S., President.

(Address delivered 14th November, 1912.)

GENTLEMEN,—In the first place, I wish to express my thanks to you for the great honour you have done me in electing me to be your President for the coming year. Year by year, owing to the growth of the Institution both in numbers and in prestige, the position of your President is becoming a more important one, and I think I may say that the responsibilities of the office were never greater than they are at the present moment. The Institution is installed in its new home. It has revised its Articles of Association, and it is now prepared, having got these matters out of the way, to undertake its proper functions with regard to the electrical industry. There is a wide field of work to be undertaken, on the one hand in encouraging the publication of scientific technical knowledge, and on the other in looking after the interests of the electrical industry.

The fourth ten-year index to our Journal, which has just appeared, reminds one that the Society is in the fortieth year of its life and work. For the Society of Telegraph Engineers held its first meeting in February, 1872, though the actual inception of the Society may be looked upon as having taken place at the preliminary meeting of Council held in May, 1871, when the first 73 members were elected.

In passing, I may mention that the Institution does not appear to possess a copy of the first list of its members, although the original proposal forms are still preserved.

It may not be known to many members that the Institution was not the first electrical society. As far back as 1837 there existed a society which, judging from its proceedings, was a very active one, called the London Electrical Society. It did good solid work, but in the light of our present knowledge the titles of some of the papers appear rather curious, such as "On the Use of Black Paint in averting the Effects of Lightning on Ships." I wonder how many of the present papers will appear equally curious in seventy years' time.

The Institution, through the generosity of the late Sir Francis Ronalds, possesses in the Library the original documents dealing with this Society, and the actual manuscripts of many of the papers read before it.

LIBRARY.

This brings me to a subject which I have very much at heart, namely, the Library of the Institution. I think that a large number

of the members do not realise what a wonderful library is at their disposal. By the gift of Sir Francis Ronalds, the Institution has vested in it the use of what was, in that day, the most perfect collection of works dealing with electrical matters.

For a time the Institution added books and papers as they appeared, then owing to lack of space and to the small use members appeared to have made of the library the number of accessions decreased. Eight years ago, when it became evident that the finances of the Institution would shortly allow us to have a building of our own in which a proper library could be fitted up, the Institution started to bring the library up to date, a work which is now nearing completion.

The combination of the older works which were collected by the late Sir Francis Ronalds, and modern works which the Institution has obtained either by gift or purchase, forms a library which I can safely say outrivals any other library of electrical literature, certainly in this country, and possibly in the world. It contains many original documents unobtainable elsewhere, especially in connection with the early days of the telegraph, both land and submarine, as well as most of the early rare books on magnetism and electricity. The classified section of modern technical books is very complete. Nearly all electrical works of importance are being added to it as they appear either by gift or purchase, and I look forward to the time when the few works which are still missing will be safely placed on our shelves, so that we shall possess, without possibility of doubt, the finest electrical library in the world. I also hope that at a not distant date we may acquire a sufficient duplication of modern works to enable the Institution to lend them to those members who cannot conveniently visit the library.

MUSEUM.

Another subject which I wish to invite the members to support is the Institution Museum. We as engineers are rather inclined to live in the present and the immediate future, sometimes looking forward to more distant times. Looking back along the lines of progress has also its use, and nothing brings to one's mind more clearly the steady development of many of our electrical machines than to look at the actual machines that were previously in use. We are luckily in possession of some of the early machines that form stepping-stones in the development of modern electrical generators, though our set of machines is far from complete. We possess a wonderful collection illustrating the development of the modern telephone and telegraph, and quite good collections of early meters and lamps. One of the great difficulties in making a museum is that when things are in common use every one thinks that they are of no importance to a museum, and directly they go out of common use and begin to become historically interesting, one finds that all the examples have been destroyed and that they are no longer obtainable. This has unfortunately been the case with a large amount of electrical machinery which, owing to its size and the value of the materials, has been broken

up and destroyed. I should like, therefore, personally to appeal to the members of the Institution to present to the museum examples of present-day apparatus as they become obsolete, so that they may be preserved for future generations rather than that they should be melted down as scrap.

Presidential addresses generally follow along two main lines: one is a review of the position of the industry which the Society represents, or some special branch of it; and the other is a review of the progress of the Society itself. With regard to the progress of the Institution, latterly so much has been said and written, and my predecessor in office has so fully explained to you our aims, that I think I cannot do better than endorse his views without adding anything.

We have had many able reviews of the position of the electrical industry from our Presidents, and we have had put before us by Dr. Ferranti a vision of a time when the price of electricity will be very low and when everything will be operated electrically. I want to take this opportunity of voicing a warning against this Institution becoming narrow in its ideas, and limiting itself too much to one branch of the electrical industry.

The Institution of Electrical Engineers should, according to my view, embrace in its discussions and papers all matters dealing with electricity. If an engineer is properly defined as one who directs the great sources of power in Nature for the use and convenience of mankind, the term electrical engineer should include any one who applies electricity in any way so that it may ultimately be of benefit to the human race.

I think I have noticed a tendency of late years to limit more and more to one class of subject the papers read before the Institution. It is difficult to find a satisfactory word which expresses just the class I mean, but you will understand when I say that the tendency is to bring before the Institution papers mainly dealing with generation, distribution, and the cost of electrical energy, and in many cases only dealing with these as large general questions.

Of the 6,600 members the proportion must be very small who have the actual scheming or designing of large things. The great bulk of the profession must naturally be occupied in working in their own groove in life. These members have very little chance at the present time of discussing their own special subjects; the reason for this is, I think, that members are more and more beginning to think that the Institution will not consider detail subjects. This is a totally erroneous idea. Details—small things—are very often of the greatest importance. In many cases the success of a big scheme or a big plant mainly depends upon the attention that has been bestowed on its detail. The importance of this matter was emphasised, with many practical examples, by Mr. J. H. Rider in his address to the students in 1910. I trust that during the coming session members will bring forward for discussion many subjects which might be looked upon under the old

ideas as not being of sufficient importance to discuss at a general meeting of the Institution, but which can be discussed with advantage at the informal meetings that it is proposed to hold.

The tendency to restrict the subjects brought before the Institution is not good for the profession or for the Institution. To attain that much-desired and talked-of progress in the application of electricity, and with it the advancement of the industry, it is necessary for all engineers to take as wide a view as possible of everything that concerns their profession.

I am far from advocating that an engineer needs to try to know everything about all the many branches of his profession; the man who thinks he knows everything is usually useless, and generally knows very little. What I advocate is that an engineer should take a broad view of his work, and not live in a water-tight compartment bounded by the limits of his own special subject. He will find that the broader outlook not only gives him a more balanced view of the importance of the various aspects of his work, a keener interest in his profession, but also brings, at times, pecuniary advantages. But I am digressing from the real point, namely, that the Institution should set a good example by encouraging the dissemination of knowledge on as wide a range of subjects as possible.

There are applications of electricity that give work to many men, applications which employ much plant and apparatus, and on which large sums of money are spent, about which we have heard very little or nothing in the Institution. Again, we hear little, if anything, about what is occurring on what I may term the borderland between electricity and the other sciences. In this borderland or fringe a large number of scientific workers are quietly at work, and what is to-day a laboratory experiment may to-morrow form the basis of a large industry. Finally, we should have an opportunity of discussing the many details in the design and operation of electrical plant and apparatus to which I have already referred, the importance of which cannot be over-estimated.

Let us take a few examples to show what I mean.

TELEGRAPHY AND TELEPHONY.

Probably from the point of view of the amount of money invested and number of men employed, the original work of this Institution occupies the first rank. I refer to telegraph engineering. It is curious, considering the importance of the subject, how few papers dealing with telegraphy have been read before us. In fact, excluding Sir John Gavey's very able review in his Presidential Address,* we have had, during the last ten years, only twelve papers dealing with this subject, in a total of 400 papers and addresses published in the Journal.

I do not think that this can be said to be due to the fact that there is nothing new to describe. A great deal of work has been going on in telegraphy and telephony, and a steady and real progress has been

* *Journal of the Institution of Electrical Engineers*, vol. 36, p. 4, 1906.

made. The great underground trunk lines for the telegraphs have come into existence, and new problems have arisen in connection with high speed working through these cables. Machine telegraphs have been developed, and are being largely tested by the Post Office. The central battery system, so well known in connection with telephone exchanges, has been applied to the telegraph, and the Post Office has installed a large power station on the south side of the river.

Telephone exchanges have increased in size and number, and many ingenious devices have been applied to render the working more and more automatic and less dependent on the human element. This has led up to the latest development of the automatic exchange, in which the whole of the operations are carried out by machines. The distance over which telephone communication has been accomplished steadily increases, and Major O'Meara spoke hopefully in his paper of the possibility of telephoning to Astrakhan. In fact it seems possible that telephony may reach to places now only connected by submarine cables.

A "telephonic current" is an expression often thought to be a synonym for a small current, but the currents used in the large central battery exchanges are far from small, and may reach many hundreds of amperes. The generation and handling of these large currents so that no sound from the commutator segments is audible to the subscriber nor any cross-talk takes place through the battery has required very careful consideration.

The expense of the lines both for telephones and telegraphs has led to the development of many ingenious systems of superimposing different messages on the wires grouped together in various ways.

It is difficult to obtain figures of the amount of money invested in the telegraphs and telephones of this country, but if we take the original sum of money paid by the State when it purchased the telegraphs, and the amount of money spent on its extension, together with the money spent by the Post Office and the National Telephone Company for the telephones, the sum is probably well over £100,000,000 and the number of employees probably exceeds 100,000, though it is difficult to state any figure because many employees carry out both postal and telegraph duties.

Electric signalling on railways may be looked upon as a form of telegraphy in which most striking advances have been made in the last few years.

I cannot leave the subject of telegraphy without mentioning the progress that is being made in the art of wireless telegraphy. I need not recall to your minds the great interest that was awakened when in 1899 Signor Marconi read his paper on wireless telegraphy before this Institution.

Since that date we have had a number of papers dealing with different parts of the apparatus, but we have not had a general review of the subject. The principles are well known. At the transmitting end we require to produce in the aerial conductor high-frequency currents of considerable power and of one definite frequency, and to

radiate the energy. At the receiver we wish to absorb the radiant energy and to convert the high-frequency current in the aerial into audible or visible signals.

The exact mechanism of the means of transmission between the two aerials is at the present moment under discussion, and opinions differ as to how far waves through the air, waves through the earth, and waves on the surface of the earth take part in the transmission. The matter has been very ably expounded in a paper by Professor Fleming, which was discussed at the last meeting of the British Association.* The general conclusion seems to be that Hertzian waves propagated through the atmosphere are sufficient to explain transmission over short distances, but when we come to consider the observed bending of the radiation around the curvature of the earth it is necessary to take into account the fact that the earth is far from being a perfect conductor, that the upper layers of the atmosphere are far from being insulating, and also the effect of sunlight on the conductivity of these upper layers. It must be remembered that the waves are being regularly transmitted round one-eighth of the earth's surface, and that Mr. Marconi has received signals over 6000 miles—that is, round one earth quadrant.

There seems no doubt that long waves are more suitable for long-distance transmission, but the question is far from settled as to what is the best wave-length for long-distance work, or even whether there is such a thing as a best wave-length for any given distance. It may well be that the wave-length will have to be chosen to suit each individual pair of stations.

What is a suitable wave-length, or in other words what is a suitable frequency to use in the antenna? This most fundamental question is, I believe, as yet unanswered. If we knew the most suitable frequency for transmission, what then is the most suitable design of antenna, radiator? Again, how are we to generate the current? Can we generate it in a dynamo, or must we use some form of spark or arc?

Naturally the above questions are mutually dependent, that is to say, the frequency to be used depends upon the design of the antenna, and the design of the plant also depends upon the frequency, and all three naturally depend on how much money one can afford to spend on the different parts.

The engineering problems which confront us at the transmitting end are the design and support of the aerial conductor, the machinery to produce in it high-frequency currents of one definite frequency and of considerable power, and the mechanism for cutting up the high-frequency currents to form the dots and dashes of the Morse code. The changes that have taken place in the design of the aerial have been mainly in two directions, the one to make it more suitable for the longer wave-lengths now in use, and the other to give the aerial such a shape that the radiation may be much stronger in one direction than in any other. The first desideratum is obtained by extending the

* *Electrician*, vol. 69, p. 934, 1912.

height of the aerial and by increasing the capacity of the condenser formed by the upper part and the earth. The Eiffel Tower aerial, which consists of an inverted fan of six wires 300 metres high, and the Nauen aerial (destroyed by a gale this year) in the form of an umbrella and 200 metres high, are examples.

Of the directive aerials the Marconi type, consisting of a comparatively short vertical part and a much longer horizontal part, is the only one that has come into use for long distances. As an example, the aerials proposed for the Imperial Wireless Scheme are to be 300 feet high and apparently some two or three thousand feet long, supported by ten steel masts. The design of an inexpensive form of mast to carry the aerial is a difficult engineering problem.

The forms favoured at the moment are sectional tubes and lattice-girder constructions supported by stays.

The type of generating plant depends upon the system employed, and here it is necessary to distinguish between the two forms which the high-frequency currents in the aerial may take.

In the first form the amplitude of the high-frequency current in the aerial starts from zero, rises rapidly to a maximum value, and then dies away again. In the second form the current is of constant amplitude exactly similar to that produced by an alternator. Considering the first and older form the oscillations are produced by the sudden discharge of a condenser through a small self-induction. If the condenser be charged by means of an alternator, in general one discharge will be obtained across the spark-gap with the corresponding train of oscillations during each half-period, thus the number of trains of oscillation per second is equal to twice* the alternator frequency.

If we have a given amount of energy to transform per second, the higher the spark frequency the less the energy to be dealt with at each spark. Further, for a given size of condenser, the less the energy the less the voltage to which it will have to be charged. Hence, there are certain advantages in the high spark frequency and a further advantage will appear when we consider the receiver.

The spark frequency has gradually increased from 10 to 20 per second with the induction coil to 100–200 per second, corresponding to commercial alternating-supply currents, up to 1,000-1,200 obtained from special designs of alternators.

A method much in use for obtaining a high spark frequency from an ordinary, say 50-frequency supply is to employ a rotating spark-gap or discharger. The apparatus consists essentially of a number of revolving electrodes, which in turn come between the fixed electrodes of the spark-gap and cause discharges to take place by shortening the sparking distance. The number and speed of the revolving electrodes is so chosen as to give the required spark frequency. If such a discharger is used in connection with a continuous current, as is done at Clifden, one discharge is obtained each time the electrode or stud

* This number can be varied by suitable adjustment, and several sparks may be obtained per half-period, or conversely a number of half-periods may elapse between the successive sparks.

passes between the fixed electrodes of the gap. In the case, however, of an alternating-current supply the matter is a little more complicated because the potential of the condenser varies according to the different parts of the wave-form at which the discharger causes the spark to take place.

One great advantage of the revolving type of discharger over the older fixed gap is that the windage on the electrodes keeps them cool and blows away the conducting gases formed at each discharge, so that the gap rapidly passes from the conducting into the insulating condition. This is a matter of great importance, for if the gap did not rapidly recover its insulating properties an arc would be formed which would prevent the condenser being fully charged again. A number of methods of cooling the electrodes and gases of the spark-gap have been devised. One, which is being largely used by the Telefunken Company, consists of making the spark-gap of a number of flat metal discs, separated by extremely small air-spaces of the order of a tenth of a millimetre, so that the cooling action of the discs on the gases between them is very great. This cooling action causes a rapid extinction of the discharge called "quenching." Quenching is of great value in reducing the reaction between the oscillations in the aerial and those in the condenser circuit, thus enabling tighter coupling to be used and a pure radiation to be obtained. This system, due to Professor Wien, is spoken of as the Wien or shock excitation system.

A number of ingenious methods have been proposed in which advantage is taken of the fact that a condenser takes a certain time to charge through a resistance to a given voltage.

If a high-voltage continuous-current supply is available, and a condenser be connected to it through a resistance, the condenser will charge to any voltage less than the supply voltage in a definite time depending on the value of the resistance. If the condenser be also connected through a self-induction to a discharger in the ordinary way, then discharges can be obtained at regular intervals, depending upon the time the condenser takes to charge. It is quite easy to vary this time over a very wide range, say, from one discharge per second to 100,000 per second, provided the spark-gap becomes insulating sufficiently quickly after each discharge. Mr. Galletti has coupled together a number of circuits on the above principle to enable large powers to be dealt with, and he informs me that he is now erecting near Lyons, in France, a transmitting station on a large scale. The results of the tests of this station will be extremely interesting.

Turning next to the continuous oscillations, the arc method has been longest in the field. The principle is well known to you, as it was described before this Institution in 1900. It consists in shunting an unstable arc with a self-induction in series with a condenser when, if the conditions are suitably adjusted, high-frequency oscillations take place in the condenser circuit of practically constant amplitude. Since then Poulsen and his colleagues have founded a practical system on this principle, and considerable progress has been made, although the

system has not come into commercial use in this country. The engineering difficulties of the system appear to consist in maintaining the arcs, by means of which large powers are being transformed, so perfectly regulated that the output of high-frequency current is constant both in strength and in wave-length.

High speeds, over 100 words per minute, have been attained in tests of this system between Copenhagen and Cullercoats in England. I am informed that a number of commercial stations have been recently opened on the west coast of the United States, and that good communication is now being obtained by night between San Francisco and Honolulu, a distance of 2360 miles, with only 25 kw. The stations have masts rather over 400 ft. high, and they are now being equipped with arcs to transform much larger powers.

Another development of great engineering interest is the construction of alternators of sufficiently high frequency for direct connection to the aerial. In this case the wireless station becomes very much like an ordinary generating station, only that, instead of producing alternating current at 50 frequency, alternating current of, say, 50,000 frequency is generated and is supplied straight to the aerial. This, of course, does away with all questions of sparks and arcs, and I must say that, from an engineer's point of view, is a very attractive solution. Alternator designers will, however, readily appreciate the difficulties in designing alternators for these frequencies. In order to get space for the poles the peripheral speed of the rotor must be made as high as possible and the details of the mechanical design must be first class. In spite of the difficulties a number of machines have been made by the General Electric Company of America, which will produce 1 kw. at a 100,000 frequency. They are of the inductor type, and details of their design have recently been published by Mr. Alexanderson. Another most ingenious alternator is that designed by Dr. Goldschmidt, in which advantage is taken of the reaction of the stator current on the rotor and the rotor current on the stator to step up the frequency of the machine. By this method Dr. Goldschmidt has reached frequencies of 50,000 with several kilowatts output, and he is now engaged in building some very much larger machines to give over 100 kw. each.

The present position is that we have in the field three systems actually at work—the loosely coupled spark system, the shock excitation system, and the arc system. Within a very short time we shall, no doubt, have high-frequency alternators at work, and it is probable that at least one other system will be under practical test.

Whatever system is employed it is necessary to install some form of prime mover, the type chosen depending upon the local conditions and the amount of power required, which may be anything between the $\frac{1}{2}$ kw. for a small ship station and the 2,000 h.p. proposed for the imperial wireless stations. In the case of the larger powers the problem is similar to the design of a generating station for a small town, and presents the same necessity for good work and ample duplication to prevent the possibility of an interruption of the supply.

The engineering difficulties which confront us in keying are really questions of switching on and off considerable powers a great many times per second with perfect certainty and definiteness in the contacts. In the early stations for a kilowatt or so the ordinary Morse key connected in the primary circuit of the transformer sufficed; but as the power got larger different types of relays were introduced. Some of these relays operated by interrupting the primary current, some the secondary current of the step-up transformer. In continuous wave systems on the arc method keying is sometimes carried out by altering the frequency, by switching in and out self-induction or capacity in one of the circuits. This has the advantage that the whole power is not interrupted, and it also keeps the load on the arc generator more constant, which tends to improve its steadiness. With alternators it is obvious that keying can be done in the field circuit, but with these machines another difficulty arises. When the load is thrown on and off the alternator there is naturally a tendency for the speed to fall and rise. If the speed varies the frequency and hence the wave-length of the radiation alters, this interferes with the tuning.

Turning to the receiver the changes that have been made are not so striking. The coherer has become practically obsolete. The magnetic detector, which replaced the coherer about ten years ago, still holds its own, although it is not very sensitive. Its reliability in action and the fact that it is almost fool-proof are for many purposes convincing arguments in its favour.

The electrolytic detector has been but little used in this country. The Fleming valve and the crystal detector have come into considerable use. They are both highly sensitive and depend upon the curious properties, in the one case of the residual gas of the electric lamp bulb, and in the other case of the contact between two minerals. The property which makes them useful as detectors is in each case their unilateral conductivity. The high-frequency currents induced in the receiving aerial by the incoming waves are not generally sufficiently strong to affect any of our ordinary alternating-current measuring instruments. If, however, they can be rectified and converted into continuous currents, then it is easy to detect them, for it is common knowledge that the direct-current measuring instruments are in general hundreds, if not thousands, of times more sensitive than alternating-current ones.

Corresponding to each spark at the transmitter a train of oscillations is received, and these trains of oscillations are rectified by the detector, and in general are passed through a telephone as an indicator. At each spark a click is heard in the telephone, so that with 600 sparks a second the diaphragm is attracted 600 times, producing a somewhat musical note.

Herein lies one of the great advantages of high-spark frequency. There seems no doubt that the combination of the human ear and a telephone is much more sensitive for high-frequency notes than for low ones. In some tests I have made, using an alternating current to

determine the minimum power required to produce an audible signal in a telephone receiver at different frequencies, I found in one case that the power was reduced from 430 micro-microwatts at 300 frequency to 7·7 micro-microwatts at 900 frequency. At higher frequencies it increased again.

Due to atmospheric causes, there are generally audible in the telephone receiver clicks and noises commonly spoken of as atmospherics or strays. With high-spark frequencies the human ear easily distinguishes the musical note from these atmospherics; this enables the operators to read through a large amount of extraneous interference. The elimination or compensation of these atmospherics is one of the most important outstanding problems in wireless telegraphy.

When operating with continuous waves practically no note is heard in the receiver telephone unless the currents are chopped up into rapidly recurring groups of waves either at the transmitter (tone sender) or at the receiving end (ticker).

In order to make a permanent record of the signals, and to allow of high-speed working, the rectified current from the detector may be passed through a galvanometer or a relay, and here we come to one of the difficult problems which requires solution, namely, the construction of a relay or recording instrument which will make a record of the very small received currents at high speeds. The Einthoven or string galvanometer, which is at present used for this purpose, is delicate and gives a photographic record.

Although the difficulties may be minimized, I do not feel at this moment that the photographic method of recording, with the attendant chemicals, and the necessity of handling moist slip, can be looked upon as the final solution from the point of view of commercial telegraphy. The problem of constructing a relay for this purpose is a very difficult one. The mean current strength of the signals, after rectification by a high-resistance detector, is of the order of $\frac{1}{10}$ to $\frac{1}{100}$ of a microampere, and the amount of power available to work the instrument is only of the order of a few micro-microwatts. For high-speed reception the number of contacts to be made and broken per second may be anything up to 50. The problem before our instrument-makers is to construct a relay or recorder which will operate with a power not exceeding a few micro-microwatts at the rate of 50 signals per second.

Recent experiments by Kiebitz have drawn attention to the fact that quite good reception of the long waves from distant stations can be obtained without high aerials. A single wire supported on insulators near the ground, or even resting on it, gives quite good results. Whether similar antennæ can be efficiently employed for transmission is a matter for further experiment.

I have for some time felt that it would be of very great interest if some authority like the Post Office, which has available many miles of overhead wires, would make some experiments at comparatively low frequencies. We know that Mr. Marconi, in his transatlantic work, is

working with frequencies as low as 50,000; high from the station engineer's point of view, but low from the wireless point of view. Now an alternator for ten or fifteen thousand frequency is not at all impracticable. Supposing an ordinary telegraph land line on fairly high poles four or five miles long was used as an antenna, and fed from a 10,000-frequency alternator, how would it behave as a wireless transmitter? There are reasons which make one think it might be satisfactory.

The radiation in wireless telegraphy is similar to light, therefore it is of interest to compare the human receiver of this radiation, namely, the eye, with the receiver of a wireless station.

According to some recent experiments by Messrs. Paterson and Dudding, a light of $\frac{1}{10}$ of a candle at a distance of a kilometre is near the limit of visibility. Assuming the square law, this corresponds to a candle-power of 2,560 at 100 miles. With our present incandescent lamps this would require $2\frac{1}{2}$ kw. In wireless, to cover the same distance, it is usual to install what is nominally a $1\frac{1}{2}$ kw. station.

Taking, however, Drysdale's figures for the rate of radiation of energy in the visible spectrum (about 0·1 watt per candle-power), our source of light is radiating energy at the rate of about 250 watts. Very accurate figures for the rate of radiation for a wireless antenna are not available, but assuming our overall efficiency at 20 per cent, the radiation of the wireless antenna is 300 watts. It would seem, therefore, that there is a remarkable similarity between the sensibility of our eye to radiation of the short wave-length which constitutes light, and the sensibility of our wireless receiving apparatus for the long wave-length radiation used in telegraphy.

According to Lord Rayleigh's experiments, if a tuning-fork is producing sound at the rate of 42 ergs per second it will just be audible at a distance of 30 yards. This corresponds to a source of sound giving out 0·0056 watt being audible at 1 km., or, assuming no absorption or bending of the sound waves, 143 watts at 100 miles. We therefore have the curious result that if it were possible to radiate energy at a given rate, either as sound-waves or as light-waves or as long Hertzian waves, we should be able to detect them at approximately the same distance by means of our ear or our eye or the receiving apparatus of a wireless station.

Wireless time signals are regularly sent out each day from the Eiffel Tower and other stations for the use of the ships at sea. In view of the fact that wireless signals are received practically simultaneously everywhere on the globe, they form in conjunction with transit observations a ready means for the determination of differences of longitude, to a high degree of accuracy. It is claimed that in recent tests, using the signals sent out by the Eiffel Tower, differences of time were determined to an accuracy of $\frac{1}{100}$ of a second, which corresponds to an uncertainty in position of only 5 yards at the Equator. The surveying of difficult country may be expected to be greatly facilitated by this new means of determining the difference of time.

Of the sister science, namely, wireless telephony, there is not so much to relate. A certain amount of progress has been made, but the details of the methods used have not been made public. The principle is simple. Given continuous oscillations or a spark frequency above the limits of audibility you may vary the antenna current, and hence the radiation by means of a microphone, in the same way as a continuous current is varied by the microphone in ordinary telephony. As the radiation varies according to the modulation of the current by the voice the received current will be varied in the same manner and the voice will be reproduced. The difficulties are mainly in the transmitter. First, we require a perfectly steady source of continuous oscillations, and secondly, a microphone capable of modulating the large powers required to transmit any distance. Over short distances of a few miles there are no difficulties. It is only when we come to distances of 50 to 100 miles that the engineering problems become troublesome. In view of the progress that is being made in the high-frequency alternator, and of how much more easy it is to modify the power given out by an alternator, it will not be surprising if as soon as high-frequency alternators are in use wireless telephony over comparatively long distance becomes a working possibility.

ELECTROCHEMISTRY AND ELECTROMETALLURGY.

I hardly think it is necessary for me to say very much to convince you of the great importance of electrochemistry and electrometallurgy, a subject but rarely mentioned here. The amount of power installed for chemical and metallurgical purposes is very large indeed. Exact data are wanting, but it seems probable that the power employed in these processes in Norway and at Niagara may already reach 1,000,000 kw. One of the necessities of our industry, namely, copper, is largely purified by electrical means. Aluminium, calcium carbide, carborundum, sodium, and potassium are wholly prepared electrically. The only hydroelectric stations of any size that have been built in this country are used for electrochemical purposes. The production of aluminium alone at Loch Leven absorbs some 30,000 kw.

The application of electric heating either in the arc furnace or in the induction furnace is making considerable progress in the smelting and refining of iron and in the manufacture of special steels and alloys, and it seems as if we are on the verge of extensive application of the electric furnace.

My predecessor in office, Dr. Ferranti, in his Presidential Address two years ago, pointed out that if very cheap electrical power were available all these industries might flourish in this country, and he outlined a scheme for power production on such a large scale as to make very cheap power available.

Although we have not yet attained Dr. Ferranti's ideal, the ever-increasing demands for electrical power for all purposes is causing the price per unit slowly but surely to decrease in a most satisfactory manner.

The production of disinfectants electrolytically is being worked on a small scale. In Poplar the formation of a solution of chlorine of water by means of electrolysis is in practical use. Although one cannot anticipate very large powers being required for this purpose, yet if the demand for electrolytic disinfectants all over the country were the same as in Poplar, it would require about 2,000,000 units per annum, all of which could be supplied at such times as would help to level up the load curve.

ELECTROMEDICAL APPARATUS.

I know that I shall be touching on a somewhat delicate question if I refer to the medical properties of electricity. The terms "electric" and "magnetic" when applied to certain articles are often intended to mislead the public into thinking that they have a somewhat miraculous property. It is not unusual to find advertised as electric, ordinary articles of clothing which are alleged to cure almost every known disease. It is very unfortunate that this use of the terms should have grown up, because there are without doubt many diseases in which electricity can be applied beneficially, and, further, it leads the public to disbelieve in results which are attained by genuinely electrical means.

Now, as we are not a medical society, the nature of the diseases and their treatment do not come within our sphere, but as electrical engineers we should endeavour to supply the medical profession with the very best apparatus for their purpose. It seems to me that this would be facilitated if the two professions could be brought a little more closely into touch with one another. One difficulty which no doubt exists is the want of a common scientific language. The medical profession do not, I think, understand many of the terms used by electrical engineers, and I am quite certain that electrical engineers do not understand many of the medical electrical terms. For example, I wonder how many present know what a medical man means by Galvanic, Faradic, or Leduc currents, or by d'Arsonvalization. I have often wondered why so much of the apparatus used for medical purposes comes from abroad, and I feel confident that not only is the apparatus capable of being made in this country, but that a much better class of apparatus could be made here if only electrical engineers and the medical profession would co-operate. The present apparatus is extremely ingenious, but there is little doubt that much of it might be greatly improved in its electrical and mechanical design. The problem of devising suitable apparatus is quite an interesting one, for it must be remembered that owing to the want of standardization in voltage and system of supply in this country, it is necessary for the apparatus to be able to convert all these different supplies into each and all of the peculiar kinds of currents required by the medical man and not only convert them with certainty but also with safety to the patient. It may be said that apparatus of this kind is of no importance to the electrical engineering industry, but when I mention that the value of the exports of electro-

medical and electrodental apparatus from Germany in the year 1910 was returned at the sum of about £132,000, of which just under 10 per cent was purchased by this country, you will see that it is not quite negligible.

The question of X-rays, a subject which I think has never been mentioned before this Society, is a very important one from the point of view of diagnosis. In this connection I should like to point out that the design of induction coils for the production of X-rays has advanced a long way of late years, and that some of the latest pieces of apparatus for the production of the discharge through the X-ray tube involve considerable ingenuity and engineering design. The discharge must be unidirectional and at a high pressure, say 50,000 volts or more. One method to obtain this is to step up by means of an e.h.t. transformer and to rectify the secondary current. Another method of working to obtain practically instantaneous photographs consists in switching the primary of the transformer straight on to the direct current mains, when the current rush instantly blows the fuses. This interruption of the current produces one powerful discharge on the secondary, which passing through the X-ray tube suffices for the photograph. I do not know how the supply companies view this method of operation, because the rush of current must be pretty considerable, as the apparatus is not constructed on a particularly small scale. The transformer weighs about half a ton.

Household Apparatus.

I believe that we have had no papers dealing with the design and construction of apparatus for household use, such as fittings, heating and cooking apparatus.

We have discussed frequently the great advantages that will be obtained by a more extended use of electricity for domestic purposes. To extend domestic uses it is most important that the apparatus should be really appropriate to its purpose. When we come to deal with fittings and other apparatus for use in our homes, we cannot wholly look upon the matter from the strictly engineering point of view. Many of our household articles are things with which we have been familiar since childhood. We have grown up with them, and we have become so used to them that we have a tendency to think that they are the only possible things to serve their purpose, so that when a proposal for a change comes it comes almost as a shock. We in this country have grown used to the open fire, and the acceptance of the suggestion that any other form of heating is as good involves a complete change in our ideas. The substitution of gas for candles involved quite a considerable change in our methods of lighting, and although this change was started almost exactly one hundred years ago, it is not really complete yet, and there still lingers in the minds of many an affection for the older form of illuminants, namely, the candle and the oil-lamp. The great superiority of the electric light over all other forms of illumination has made the change from gas to electricity easier, but the designs of our electric

light fittings are still largely reminiscent of gas and candles. I think that from the artistic point of view there is no doubt that the early designs evolved for the candle were made at a period when artistic design was in the ascendant. The results of the attempt to combine the modern form of illuminant with those early artistic designs is not always happy, either from the artistic or the practical point of view. Many of us must have experienced the difficulties that arise in some artistic fittings from the fact that no adequate provision has been made to allow of the introduction of the wires which are still necessary to convey the current to the lamps, and even in the cases where channels for the wires have been provided in the designing of the fittings they are often left in such a rough condition inside as to endanger the safety of the insulation. Of late years very considerable progress has been made in the redesigning of the fittings of our homes so as to make them at the same time both serve their proper purpose and be pleasing to look upon.

Electrical cooking and heating apparatus has advanced during the last decade, but there is one difficulty which is not often referred to when one compares electrical heating apparatus with heating by means of flames. I mean the difficulty of limiting the temperature rise. For convenience, for instance, to boil a kettle it is necessary to be able to generate heat very quickly for a short time. It is quite easy to construct apparatus to fulfil this condition, but if such electrical apparatus be left in service continuously it may attain a temperature which will be injurious to it. This is not the case with flame heating, as there is always an upper limit of temperature, namely, that set by the flame itself. If we could obtain materials for our heating apparatus which could be run continuously at a very high temperature, say in the neighbourhood of white heat, then this difficulty would be to a large extent overcome, because the dissipation of energy by radiation would rapidly become so great with rise of temperature that the final temperature would be reached before the heating arrangement was injured; a material with a very high positive temperature coefficient would serve the same purpose.

A further advantage of possible high temperatures of the heating part is that it enables heat to be transferred from one place to another by radiation, a method of transferring heat which we know from experience is very suitable for many heating and cooking purposes. We are all accustomed to the advantages of the glowing red fire. The last few years has seen a great progress, and I see no reason why still greater progress should not be made in the near future. What I wish to emphasize is that in my opinion the Institution as a body has not sufficiently discussed and considered these matters.

Electricity and Chemistry.

Turning next to the subjects on the borderland between electricity and the other sciences, we are all of us acquainted with the brush discharge, yet how much do we know of its mechanism? In our

high-tension machinery we are mainly occupied with trying to get rid of it and its injurious effects. Yet it has its uses. Nearly all the information in our proceedings deals with the negative question, namely, how to avoid it.

Now the electric discharge has a peculiar property of producing that modification of oxygen known as ozone, which is without doubt a strong sterilizing agent, and which may in the future have considerable applications. A modification of the conditions of the production of the discharge will cause the formation of oxides of nitrogen instead of oxides of oxygen. Oxides of nitrogen are of great commercial importance, and their production by electrical means will probably be one of the most important industrial applications of electricity.

Already in Norway between 100,000 and 120,000 kw. is employed working day and night for this purpose, and it is stated that this power will shortly* be increased to nearly 250,000 kw. The main object of fixing the atmospheric nitrogen is to form a substance to replace Chili saltpetre. The demand for this is yearly growing at an increasing rate. I will not take you over the ground which has been so ably covered by Sir William Crookes, Professor Silvanus Thompson, and others, pointing out the vital importance to the world of being able to fix atmospheric nitrogen, and showing how soon there will be a shortage of the natural material. It suffices to say that as far as one can see the price of the material in the future is likely to rise rather than fall, so that the electrically produced substitute will be able to compete more easily.

What is the mechanism, what are the causes which determine whether when an electric discharge takes place the nitrogen or the oxygen of the air will be oxidized? Certain forms of the arc discharge seem especially favourable to the production of the oxides of nitrogen. It appears that a high temperature is required to oxidize the nitrogen, but that at a lower temperature the oxides of nitrogen which have been formed decompose again, so that it is necessary to remove and cool the nitrogen compounds after formation as quickly as possible to prevent them from being destroyed.

The properties of the different compounds of oxygen and nitrogen and the active modifications of oxygen and nitrogen—for an active modification of nitrogen has lately been discovered by Professor Strutt —are really in the domain of chemistry, so we are here on the borderland between electricity and chemistry, a borderland in which one of the largest of electrical industries is already in its inception.

Although much work has been done on the subject of the nature of the discharge which produces the greatest proportion of oxidized nitrogen, yet I feel that the last word on this subject has by no means been said, and possibly a simple observation of some peculiarity of the electrical discharge may form the key to greatly improving the efficiency. Should this be the case a large nitrate-producing industry may grow up around the coal-fields of this country, an industry which will be of considerable value to agriculture.

Last year about 125,000 tons of nitrate were imported into this

country. To produce the equivalent amount of fixed nitrogen per annum would, on the basis of Norwegian plants, require about 150,000 kw.

At the moment I believe that the cost of electrical power is the chief stumbling-block to the introduction of the manufacture on a large scale in this country.

Electricity and Sound.

The borderland between electricity and the science of sound is, so far, less fruitful in interesting results. This may be due to the fact that sound consists of air vibrations, therefore all electrical methods of producing sounds resolve themselves into designing electrical motors whose work is to shake and vibrate the air.

Now the telephone receiver is such an electrical motor, and the laws that govern the efficiency of electric motors in general also govern the action of the telephone receiver.

I do not know of many researches on the efficiency of the telephone receiver, yet the question is really a practical one and of considerable importance. The telephone receiver may be looked upon as an alternating-current motor. It receives electrical energy, which it converts into the mechanical form in the motion of its diaphragm, which energy is transmitted to the air as sound waves. There is no special difficulty in measuring the electrical energy supplied to the telephone receiver to a moderate degree of accuracy. The amount of this energy that is transmitted to the diaphragm is much more difficult to estimate. The real difficulty is the determination of the amount of energy of the sound waves. If we possessed any apparatus by means of which we could measure energy of sound waves, not only could we determine the efficiency of the telephone receiver, but the apparatus would have many other useful applications. It is curious to think that up to the present we have no unit or standard of sound. We cannot specify its strength or intensity. Even the comparison of two sounds by the ear is very inaccurate; nowhere near as accurate as the comparison of two lights by means of the eye. This want of standards and methods of measurement is, I believe, one of the causes which have retarded progress in the science of sound. Can electricity, the handmaid of all the other sciences, help in this direction?

Electricity has been applied to many investigations into the characteristic qualities of sounds, and especially to those qualities which characterize the vowels. Sir John Gavey, in his Presidential Address,* and Messrs. Cohen and Shepherd † have published many interesting results, which have been of use in enabling us to decide what is the range of frequencies for which our telephone apparatus must be constructed to enable good articulate speech to be obtained.

Electricity can be utilized for the detection of small sounds. Two microphones attached one on each side of the hull of a ship enable the

* *Journal of the Institution of Electrical Engineers*, vol. 36, p. 4, 1906.
† Ibid., vol. 39, p. 503, 1907.

sounds of distant submarine bells to be heard through the water and, further, enable the captain to tell whether the sounding bell is fore or aft or broadside on, and approximately the bearings of the bell. As a means of warning ships of the dangers of the coasts, submarine sources of sound and electrical means of detecting them are destined to play a considerable part in the future. A number of stations and ships are already equipped.

The production of electrical currents whose variations reproduce the variations of the air pressure in the sound waves is of greatest importance. The present microphone is only able to deal with comparatively small currents, and therefore, good as it seems, it has serious limitations. One cannot look upon the variations in the electric currents produced by the microphone when spoken into as truly representative of the sound waves. Microphone diaphragms possess, like most vibrating bodies, free periods of their own. One has only to produce in front of a microphone a series of notes which sound approximately equally strong, and it will be found that the current variations produced are enormously greater for some notes than for others, and that great distortions take place. There is no very great difficulty in proving that this occurs for notes with approximately the same periodic time as one of the free vibrations of the diaphragm. Another difficulty resides in the fact that the resistance of the microphone is not connected to the pressure of the sound wave in any clearly defined way, at any rate for the strengths of sound used in practice, so that the microphone must be looked upon as being very far indeed from an ideal measuring instrument. Many researches have been made to try and invent new microphones. We seem to know but little of the exact mechanism of the transfer of electricity from one body to another when they are lightly in contact. Probably a thorough investigation into the nature of contacts would reveal much useful information not only on the theoretical but on the practical side, and provide data for the design of new microphones, and for the contacts used in electrical apparatus. As an example of the extraordinary sensibility of a contact under suitable conditions, I might mention the contact in Mr. S. G. Brown's relay, which by means of an ingenious automatic electrical arrangement is kept in such a condition that one feels doubtful whether there is actual contact or not.

Electricity and Radiation.

One of the most fascinating subjects on the borderland between electricity and the other sciences is the connection between electrical energy and radiant energy, and the possibility of the mutual conversion of one form into the other.

Although at first sight this subject may seem somewhat remote from the matters which occupy the minds of electrical engineers in their daily work, yet it is of the utmost importance. Something like 500 million units are used annually for lighting purposes. Light is a short wave-length radiation. If we think of the large plants that are

installed, boilers, turbines, and generators, and all the money spent in laying mains in the streets, and if we think of this simply as a means of producing a short wave-length radiation for the purpose of lighting our rooms and our public streets, we are led to inquire what sort of efficiency we are obtaining. We have heard much in the past of the great efficiency of electrical apparatus.

Electrical apparatus is very efficient. As our real object is the production of light—the conversion of the heat energy of the coal into the radiant energy of light—what is the overall efficiency? It is well known that it is very low. The experimental difficulties in the way of obtaining accurate figures are great. According to some tests by Dr. Drysdale, a 1 candle-power source of light which emits all its energy in the visible spectrum, say between the wave-lengths 0·39 and 0·76 μ, and so distributed as to produce white light, will radiate energy at the rate of about $\frac{1}{10}$ of a watt, or if the whole of the energy were to be concentrated in the wave-length to which our eyes are the most sensitive, say in the yellow green, the rate would be about $\frac{1}{17}$ of a watt per candle-power.

Looking through a number of different determinations, there seems no doubt that, even with the best forms of modern metallic filament lamps, the efficiency of the conversion of electrical energy into radiation of a wave-length suitable to our eyes probably does not exceed 5 per cent.

If we consider the losses in the boilers and engines so as to find the overall efficiency of our means of production of light, we realise that the figure is still more unsatisfactory. The thermal efficiency of the steam engine does not exceed 20 per cent, so that the efficiency of transformation of the heat energy of the coal into radiant energy as light is well under 1 per cent. There is, therefore, plenty of room for improvement. The question as to whether we can improve on this unsatisfactory result leads us to consider rather closely one of the problems on the borderland, namely, the mechanism of the conversion of electrical energy into radiant energy such as light. In the great bulk of practical sources the light is obtained by heating some solid body to such a high temperature that it emits radiant energy in the form of light. This is certainly the case with ordinary incandescent lamps and arcs. The real reason that sources of light consisting of heated solid bodies are so inefficient is that these hot bodies radiate so little of their energy at wave-lengths suited to our eyes.

In the incandescent lamp there is probably very little loss by conduction and convection; practically the whole of the electrical energy is converted into radiant energy, yet unfortunately over 90 per cent of this energy is produced at wave-lengths which are too long to affect our eyes. We can shorten the wave-length at which the hot filament gives out the bulk of its energy by raising its temperature, but there is a limit to this in the temperature which the filament will stand. Probably the main reason that the metal filament lamp is more efficient than the carbon filament lamp is that its filament is capable

of being used at a much higher temperature. This, however, need not be the sole reason, as there are some bodies which tend to concentrate their radiation in certain regions of the spectrum. The processes that are taking place in a solid body when it is radiating are not fully elucidated.

Sir J. J. Thomson has built up a wonderful theory of the constitution of matter, in which the atoms of all bodies contain small discrete particles of negative electricity which he calls corpuscles. These corpuscles, irrespective of the nature of the body or of its state, always carry the same charge of negative electricity, and it is the movement of these charges of electricity that constitutes our electric currents.

In gases the corpuscles are looked upon as being able to move about with comparative freedom, and at very high speeds. In metals there are two points of view: one that a number of free corpuscles are moving about between the atoms, and the other that the corpuscles are being handed backwards and forwards between one atom and another of the metal.

In all cases the conduction of electricity may be looked upon as a drift of the corpuscles carrying their negative charges.

Under this hypothesis, one of these corpuscles in movement is an electric current, and consequently produces a magnetic field, and as it carries an electric charge it also produces an electric field. The sudden starting or stopping of these corpuscles, or any change in their movement, will, therefore, produce varying electric and magnetic fields which, by Maxwell's theory, will constitute an electro-magnetic radiation in the form of light, the wave-length of which will depend upon the suddenness of the starting or stopping.

According to Lorentz, collisions between the corpuscles and the atoms give rise to the radiations which we know as heat and light, and it is the nature of these collisions that determine the wave-length in the solid bodies. If some such theory as this be correct, that light is produced by the starting and stopping of electrical particles, it would seem as if we ought to be able to devise some method of producing radiation of the correct wave-lengths to suit the eye without producing a number of other wave-lengths we do not require. With regard to the very long wave-lengths such as are used in wireless telegraphy, the result has already been accomplished, and we can produce a radiation of practically one wave-length direct with a comparatively high efficiency. Can we do the same thing with a short wave-length? Many attempts have been made to generate electro-magnetic radiations of very short wave-length. The methods used in wireless telegraphy are obviously impracticable. In wireless telegraphy the wave-length of the radiation is in general several times as long as the aerial conductor used as a radiator. As light radiations have a wave-length of only $\frac{1}{2000}$ of a millimetre, we should require wireless stations to produce light having aerial conductors some $\frac{1}{10000}$ of a millimetre long, in other words, our wireless station must be almost of molecular size.

I have already mentioned that in gases the corpuscles move about very much more freely, and we are consequently able to influence their movement by electrical means very considerably. If we make them collide violently, either with the remaining atoms of the gas or with the electrodes, radiations are produced, and these radiations are in some cases of sufficiently short wave-length to constitute light. The light given out by an atom in, say, a vacuum tube or flame has quite definite characteristics depending upon the nature of the material. It seems as if each atom contains or has circulating around it a number of corpuscles which can vibrate or move in certain definite manners depending upon their arrangement, and that these corpuscles emit, depending on their movement, radiation of certain definite wave-lengths producing the lines in the spectrum and the characteristic colours of the light of flames, etc. In the case of the flame arc we are already taking advantage of the light-emitting properties of heated gases. In the mercury arc we have another case of the production of light other than by the heating of solid bodies. In both these cases the high efficiency is probably due to the substitution of a gas for a solid body as the light-giving agent.

Unfortunately the mercury arc gives a somewhat unpleasant light owing to the greater part of its radiation being concentrated in a limited number of wave-lengths, mostly in the green. The advent of fused quartz, which will stand a very high temperature, as tubes for mercury arcs, has enabled the feeble radiation of the red to be somewhat improved. Up to the present mercury is the only metal that has been used in practice. Other metals have been proposed; and lately it has been suggested to use molten cadmium, which gives a very much more pleasant light, but this metal is a solid at ordinary temperatures, this necessitates somewhat special starting devices.

A minute trace of mercury is said greatly to improve the efficiency of the cadmium arc; according to Wolfke an efficiency of 6 Hefner candles per watt has been obtained.

Much work is quietly going on, of which we in the Institution hear nothing, to try completely to unravel the mechanism of the transfer of electricity through gases. There is much to be hoped for along these lines. The elaborate glass apparatus, the vacuum tubes, the mercury, the liquid air, etc., which are being used in the research make the experiments look most unpromising from the practical engineers' point of view. Yet some progress is being made in electric lighting by means of the passage of electricity through gases. Many members will remember the vacuum tube 176 ft. long which was used to light the courtyard of the Savoy Hotel. That tube, I believe, contained nitrogen, and according to the tests of Professor Fleming gave an efficiency of 0·56 candle per watt. About a year ago I saw a tube, not such a long tube, filled with the rare gas neon obtained from the residues in the manufacture of liquid air. This tube gave a most beautiful rose-coloured light. If this rare gas were obtainable in sufficient quantities we might have a rival to the flame arc. I may mention in passing that tubes con-

taining neon are now commercially obtainable and are claimed, in the larger sizes, to have an efficiency as high as 2 candle-power per watt. Further researches on the borderland between electricity and radiation will no doubt provide us with still more efficient sources of light.

I have spoken of the production of radiation by means of electricity. Can the electrical engineer solve the converse problem on a commercial scale? When we think that the whole of the energy which is now available, either stored in the coal or in the waterfall, has reached us in the form of radiant energy from the sun, one wonders whether there is not some method by which we can trap that radiant energy and convert it direct into electrical energy, without the intermediary of the plant-life of past ages which has formed the coal, or the evaporation of the mighty ocean which supplies the waterfalls.

Let us take a concrete example of the rate at which the earth receives radiation. I choose London because the complete data are available, although probably it will be difficult to find a much worse place, owing to the dirty atmosphere.

In a recent memoir of the Meteorological Office some curves are given of the rate of the radiation in South Kensington. From this memoir it would appear that on a clear day in September last year the maximum rate of radiation was as high as 0·07 watt per square centimetre, and the mean value between sunrise and sunset 0·04. On a dull day in October the mean radiation between sunrise and sunset was still as large as 0·007 watt per square centimetre, so that as a rough figure one might take the mean rate of radiation as being $\frac{1}{100}$ of a watt per square centimetre for 10 hours a day. Hence each square kilometre of the surface of London is receiving energy at the average rate of 100,000 kw. during the daytime.

If an efficiency of transformation of 10 per cent. could be obtained in converting the energy of radiation into electrical energy, we should have available 10,000 kw. per square kilometre, which would more than suffice for the present demands. The average total connections of the supply authorities in the 25 square miles north of the Thames amount to about 3,300 kw. per square kilometre.

In the clear atmosphere of the Alps the radiation is much more powerful. According to Millochau and Féry it may reach 0·17 watt per square centimetre, so that the power available is probably two or three times that mentioned above.

We are at present very far from any practical means of converting the energy of radiation directly into electrical energy, although on a small scale this conversion really takes place in many photoelectric arrangements. For instance, the action of light on the liquid potassium sodium alloy has been shown by Professor Fleming to produce a voltage as high as 0·6 volt when the liquid alloy and a platinum plate are enclosed in a highly exhausted tube and the liquid alloy is illuminated strongly. There seems little doubt that the current

that is generated in this case is produced from the energy of the light that is absorbed.

The effects so far obtained are extremely small. At the most only a few microamperes are obtainable with very strong illumination. Nevertheless, this property of sensitiveness to light, though at the moment it has no practical applications, may at any time be found to fill some useful purpose and make another case, illustrating how observations that are one day on the borderland of science may shortly afterwards be of practical use in engineering.

When it is remembered that the water-power in Norway alone is estimated to produce several million kilowatts, it is evidently better, for the present at any rate, for engineers to utilize the solar radiation by harnessing the waterfalls rather than by attempting to build radiation traps in the Sahara.

I have to-night covered much ground and touched very lightly on many subjects, more with a view of drawing your attention to the many ramifications of our science and industry than of giving a review of any branch. If, gentlemen, I have succeeded in showing the need for the Institution to take an active interest in as wide a field as possible, and for it to take, in the future, an even wider outlook on the various branches of the industry, I have attained my object.

Mr. Mordey.

Mr. W. M. MORDEY: To-night the pleasant duty falls on me to give expression to what I am sure you all have in your minds—our appreciation of the intensely interesting and very able Presidential Address we have just heard. This address illustrates well our new President's grasp, not only of main general principles, but also his intimate knowledge of the details of the applications of electricity and of electrical science. It is said that the best equipment for a prophet is a complete knowledge of the past. I think our President, in the little glimpses he has given us into future developments, based as they are on such a knowledge, has shown that he has all the necessary equipment of a successful prophet. Before proposing the formal resolution that I have been asked to move I should like, if I may be allowed to do so, to mention one or two points that may interest you. I want to draw your attention to the fact that our President is the first man who has worked through all the stages of membership of this Institution from student to President. He became a student eighteen years ago, in 1894; and in succession he became an associate, an associate member, a member, a member of Council, a vice-president, and now we have the great pleasure of welcoming him here as our President. His first appearance before this Institution many of you will remember. To me it was almost a sensational appearance. It was in January, 1899, when, with a fellow-student who is now Professor Marchant, he read a paper on "Experiments with Arcs by the Aid of Oscillographs." His second appearance was a little less than a year after that, when he read a paper of his own on "Rapid Variations in the Current through the Direct-Current Arc." I think all of us who heard those papers realized

that here was a coming man. It was quite evident that he had a splendid outfit for useful work. He was a physicist, a mathematician, a mechanician, an experimentalist, a ready speaker, and he had tireless industry. Since then, as you all know, he has done excellent service for the art and the science of electricity; he has often contributed to our Proceedings, and every time he has so contributed he has added sensibly to their value. Not only that, but his activity as an organizer and a worker on committees has been very great—work of which members who are not on the Council know nothing. I think one must not lose sight of the fact that in doing that kind of work for the Institution he has actually been making a sacrifice. I am sure he will not mind my saying so, but while I know that work has been a great gain to the Institution, I have always felt that a man with his special qualities would be better employed if kept entirely at scientific and experimental work. There is one other point I may perhaps be allowed to mention—Mr. Duddell is a very real link with the recent past. He came to us here as a most promising pupil of one who has gone from us, our late past-president, that great teacher, Ayrton. No one would have been prouder, or more appreciative, than Professor Ayrton if he could have been here to-night to have heard this address from his pupil. I will now simply move the formal resolution: "That the best thanks of the Institution be accorded to Mr. Duddell for his interesting and instructive Presidential Address, and that with his permission the address be printed in the *Journal* of the Institution." Mr. Mordey.

Sir JOHN GAVEY: It is with very great pleasure that I rise to second the resolution that has been proposed by Mr. Mordey. I think I may say with truth that those of us who had the privilege of being associated with Mr. Duddell in the early years of his professional career were very much struck by the extraordinary versatility he displayed, and the wonderful grasp of difficult problems which he always appeared to have at his command; and many of us ventured to prophesy that he would go far in his profession before many years had elapsed. It is a pleasure to us, as I am sure it is to all of you, to see him occupying this chair to-night. I have spoken of his versatility and grasp of difficult problems. Perhaps you will excuse me if I venture to give one brief illustration. A few years ago we, at the Post Office, were very much interested in a series of wireless experiments that we were carrying out. At one of our Council dinners I was sitting alongside Mr. Duddell, and discussing the questions at issue I said to him: "What we want is an instrument that will allow us to measure the received energy at varying distances, so as to prove the law, which we think we know, connecting the transmission of energy with the distance separating the two stations." Mr. Duddell thought for a few moments, and then he said, "I think it can be done." Nobody had done it up to that time. A few weeks later he came to me and said, "I have got an instrument that I think will do it." I arranged for a series of experiments, which took place in Bushy Park. A little while later we placed one of our cable ships at his disposal. He made a series of experi- Sir John Gavey.

Sir John Gavey.

ments, in conjunction with Mr. Taylor of the Post Office, in the Irish Sea, and the results of those experiments were laid before the Institution in a joint paper. But the point I want to make is that, within a few weeks of my putting the problem before him, he had solved the difficulty and laid before all those interested in wireless telegraphy facts and data for which we had been waiting for some years. I now simply second the resolution moved by Mr. Mordey, and ask you to receive it with acclamation.

The resolution was then carried by acclamation.

The President.

The PRESIDENT: I have to thank you for the cordial way in which you have passed the vote of thanks.

The meeting adjourned at 9.45 p.m.

NEWCASTLE LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

W. C. MOUNTAIN, Member.

ABSTRACT.

(*Address delivered 21st October*, 1912.)

After a close association with the manufacturing side of electrical work over the last thirty years, I feel it a great honour to have been elected Chairman of this Local Section of the Institution of Electrical Engineers, and to have the opportunity of addressing you. I think that it will be best for me to deal more generally with the class of work with which I am most intimately acquainted, namely, the application of electricity to mining work, and also the general advances that have been made in electric transmission of power.

Dealing with the general subject of my Address, it is gratifying to see that my early view as regards the possible applications of electricity for driving mining machinery—including winding, haulage, pumping, coalcutting, etc.—has been successfully substantiated, such applications now making very rapid strides. In fact, it may truly be said that no modern colliery or mine can possibly be worked economically unless it is thoroughly equipped with up-to-date electrical plant.

Some three or four years ago a serious explosion occurred at West Stanley Colliery, and, although an open verdict was returned, there is no doubt that in the minds of miners and others the cause of the explosion was attributed to the use of electricity. Although I have read the evidence very closely, however, and have been associated with a great many men who have full knowledge of the explosion, I do not think that electricity was the cause of it. At any rate, the explosion caused a great feeling of unrest; so much so, that, notwithstanding the fact that a new set of Rules for the use of Electricity in Mines had been issued about 1905, it was considered desirable to revise these Rules.

The main object of the New Rules has been to ensure a good mechanical protection to all cables, an efficient system of earthing, and an improvement in the design of switchgear, together with the better supervision of electrical plant underground. To put the matter briefly, the Rules now demand :—

(*a*) All cables working at pressures of over 250 volts must be armoured; and, in the case of alternating current, they must necessarily be of the three-core type. With continuous current single cables are permitted, also armoured, but bonded every 100 ft.

(*b*) The conductivity of the armouring with three-core cables must be 50 per cent of the conductor enclosed, and 25 per cent with continuous-current cables if single cables are used, the object of this being to ensure an efficient earthing system. It is also specified that all earth conductors must be carried to the surface, the object being to enable conductivity tests of the armouring to be made to ensure that the earthing system is satisfactory. This method of earthing in conjunction with an earthed neutral enables leakage trip devices to be used if they are considered desirable, but the earthing of the neutral is not compulsory. For 250 volts or less, single unarmoured cables may be used, but they have to be enclosed in a metallic casing protecting the whole of the cables forming one circuit in all positions where they are in any way liable to injury.

(*c*) Leakage indicators are required upon all installations. In this connection, many types of apparatus have been introduced of the simple and recording type.

(*d*) Whilst the Mining Rules apply only to underground work, their provisions as regards the design of switchgear may be taken to apply to any switchboards or controlling switches on the surface; and all switchgear must be enclosed underground, so that dust cannot lodge on live parts. This Rule has been the means of very greatly improving the type of switchgear, and we have to-day many examples of the draw-out type of switch column with oil-filled switches with the necessary isolating arrangement, also arranged to take ammeters and voltmeters, and, if necessary, leakage indicators and trip gear also; these are as nearly fool-proof as anything can possibly be. There is absolutely no difficulty in earthing satisfactorily.

(*e*) The Rules also provide that in connection with all underground lighting work at pressures of over 250 volts, the cables must be twin or concentric and armoured throughout. They provide, however, that for pressures of less than 250 volts single unarmoured cables may be used; but where they are liable to injury they must be armoured or encased in iron piping, and the armouring earthed to bank.

(*f*) In connection with the earthing rules, which are now very stringent, portable machines, such as coalcutters and conveyors, require to be earthed, whereas previously it was not necessary to do so. The memorandum accompanying the Rules gives very explicit details as to the construction of the terminal boxes on the coalcutter or conveyor switch, and on the gate-end box; and it provides that the earth connection must be made before the live connection, and that it must also be broken before the live connection.

(*g*) A further provision is made for the appointment of an electrician, and for the authorization of men underground to work electrical apparatus.

When the New Rules were under consideration, it was pointed out that many installations which would not comply with the requirements had been erected within recent years, that no accidents had occurred, and that to bring the installations in line would involve a very heavy expenditure. In my position as arbitrator appointed by the coal owners in a large section of the coalfields of England, I ascertained that in eleven collieries alone it would mean an expenditure of £49,000 in new cables, switchgear and other details, notwithstanding the fact that these pits had been free from accidents. The Home Office then granted an exemption until 1920, which provides that :—

> "The requirements of the foregoing Rules which relate to the construction of cables and other apparatus shall not before the 1st January, 1920, apply to any apparatus which was in use before the 1st day of June, 1911, and which had been constructed or had before the 1st June been adapted so as to comply with the requirements relating to the construction of electrical apparatus in mines in force before that day unless the Inspector of the district by written notice served on the Owner, Agent or Manager as regards all or any of the said requirements of the foregoing Rules so directs."

SURFACE MACHINERY.

In the North of England, coal owners are fortunate in having one of the largest electric power companies available; and given a cheap supply of electricity there is undoubtedly an enormous future for its use. When electricity can be supplied at a price reasonably near the cost at which it can be produced at the collieries or mines, coal owners are certainly well advised to utilize such capital as they would otherwise have spent on generating plant in increasing the application of electricity for winding, haulage, pumping, coalcutting and other operations underground. It must, however, be remembered that in this and other districts there is a large amount of unsaleable coal which is sufficiently good for steam raising; also that we have other sources of power, such as waste heat from coke ovens, exhaust steam, and blast furnace gas.

UTILIZATION OF GAS.

Owing to recent improvements in the design and construction of gas engines, they can undoubtedly be relied upon for large power units. Gas engines are now being manufactured of from 3,000 to 4,000 h.p., and are giving every satisfaction. The heat consumption of large gas engines in practice averages about 10,000 B.Th.U. at normal full load, and about 9,500 B.Th.U. at maximum overload. The net heat value of blast-furnace gas varies with the duty of the furnace and the character of the fuel used, but in general it is between 90 and 110 B.Th.U., that is to say, 100 B.Th.U. per cub. ft. is a fair average. With coke-oven gas the heat consumption is the same. The

heat value of coke-oven gas varies considerably, but it is generally in the neighbourhood of 450 to 500 B.Th.U., occasionally less. Taking 450 B.Th.U. as a usual figure, the consumption per b.h.p. per hour at normal full load is about 22½ cub. ft.

The exhaust gases from gas engines can also be used for generating steam, and when an engine is developing something like its full load, from 2 to 2½ lb. of steam per b.h.p. per hour, at 60 lb. pressure, are required. This steam can be utilized for driving auxiliaries, heating, and other purposes.

As regards the power available from blast-furnace and coke-oven plants, the following figures are interesting:—

The calorific value and volume of gases evolved by a blast-furnace depend upon the character of the furnace burden, and to some extent upon the method of driving; but, as an average figure for the North-East Coast practice, the gas evolved per ton of pig-iron produced is about 160,000 cub. ft. measured at atmospheric temperature and pressure. Of this gas about one-third is used by the ovens, about one-eighth by the blowing engines (if driven by a gas engine), and about 10 per cent is lost or used up in miscellaneous ways; thus about 45 per cent of the gas is available as surplus, or about 72,000 cub. ft. Taking an average heat value of 100 B.Th.U. per cub. ft. the horse-power developed by large gas engines would amount to about 30 b.h.p. for every ton smelted in twenty-four hours. If ordinary steam blowing engines were already installed, the available surplus would drop to about 25 b.h.p. per ton of pig in twenty-four hours, and might even fall below this value.

With coke-ovens the production of gas naturally varies with the quality of the coal, but an average figure is 10,000 cub. ft. of gas per ton of coal. The surplus gas, when regenerative ovens are employed, amounts to about 5000 cub. ft. per ton of coal. Where non-regenerative ovens are employed, the surplus is very much less, sometimes amounting to 2500 cub. ft. In these cases the high temperature of the escaping gases enables a good deal of steam to be evaporated by utilizing the gas with suitable boilers. Roughly speaking, I believe that about 1 to 1½ lb. of steam will be generated for each pound of coal coked; and this steam may, of course, be used for driving steam turbines or other types of engines. It will thus be seen how much power can be obtained by utilizing what was at one time a mere waste product.

It is impossible here to deal with the many applications of waste sources of power, but their value has been recognized by the Newcastle Electric Supply Company, who have already established a number of waste-heat stations; these enable the Company in turn to supply power at reduced rates to their consumers. At the same time, where a works or a group of collieries are of sufficient magnitude to use the waste heat or gas themselves in one form or another, there is no doubt that, owing to the small distribution losses, such works stand in an exceptional position for the cheap production of electricity. It is gratifying, indeed,

to see the way in which private power stations of considerable size are being installed all over the country.

I have had occasion to report upon, and also to install exhaust steam plants, and I am able to state positively that, after allowing for interest and depreciation of the plant, also for attendance, repairs, upkeep, and in some cases a certain amount of coal for producing steam for operating the turbines when exhaust steam was not available, the cost has been as low as 0·15d. per unit, and in many cases under 0·2d., notwithstanding the fact that the plants are working with a load factor of only about 30 per cent. Fortunately, in this district we have the Electric Supply Company, who are prepared, I understand, to provide upon reasonable terms a standby supply, and also to give current when the turbine is standing or is laid off for repairs. In this way the cost of his generating plant is reduced to the coal owner, since the expenditure of capital on duplicate machinery is avoided.

Electric Winding.

Since writing my first paper on "Electric Winding" the design of electrical machinery has improved, and costs have been reduced; but, even yet, for the heavy work and with cheap coal or waste heat as fuel, I still think that the modern economical steam winding engine with high-pressure boilers, and superheated steam, will be found the cheaper system of winding.

I have never seen any actual figures taken over a period of, say, twelve months which show the cost of winding by electricity for heavy duty, and it will be very interesting if some figures—particularly of winders operated from a supply company—can be given. There is no doubt, however, that for winding engines of small output there is a very large field, as small steam winding engines are necessarily uneconomical. I am glad to say that in this direction a very considerable amount of work is being done, and I think it could be increased.

A great deal of controversy has taken place as to the type of winder to use for such duty. Personally, I consider that the use of machine-cut helical gearing has assisted greatly, as it enables smaller and less expensive motors to be used. The main question is whether such winders should be provided with induction motors driving direct, or whether motor-generators or balancers should be fitted. There is no doubt that the simple induction motor has a great charm on account of its lower cost; and assuming that the source of the power supply is such that the peak load on starting does not cause any trouble, I should feel disposed to recommend it. Investigations which I have made in connection with a paper read by Mr. H. J. S. Heather before the Institution in London, show that the consumption of current is about equal to that of the more complicated motor-generator or balancer equipments. It must also be remembered that in introducing a motor-generator there is the risk of breakdown of the motor, heating of

bearings, resistance of flywheel, and the possibility of the generator also failing.

CABLES.

The use of high voltage has, of course, brought forward various types of cables in connection with mining work, and we have the choice of vulcanized india-rubber, bitumen, paper, and other compounds such as dialite. Personally, I have been an advocate of bitumen cables for many years. I know that paper-lead cables are being almost exclusively used on the Continent, and for very high voltages I have no doubt they are desirable; but for mining work, where there is risk of the lead sheathing being punctured and the paper absorbing moisture, it appears to me that bitumen cables possess many advantages. It is, of course, desirable that the cables should be constructed so that there is little or no risk of the conductors coming in contact due to decentralization.

UNDERGROUND SWITCHGEAR.

In connection with all installations there should of course be a main or master switch in each seam to enable the supply to be entirely cut off; and, in addition to this, distribution switches should be provided so that all circuits can be controlled from the one centre. The type of switchgear now on the market, *i.e.* the pillar type with draw-out arrangement for isolation, lends itself very satisfactorily to this purpose; and I think that with switchgear of this construction, with the cases properly earthed and with ammeters on each circuit and a voltmeter to the master switch, every reasonable protection is obtained.

The method of earthing the armouring on to the switch cases requires considerable attention, as it is by no means easy to obtain a satisfactory earthing system unless very great care is taken in these details. The same remarks also apply to the junction boxes and to the connections to the motor switches, controllers and the motor cases themselves.

UNDERGROUND MOTORS.

I am glad to note that the Rules in regard to the installation of electric motors underground are fairly open, but there is no doubt that it is desirable to enclose the slip rings if there is any likelihood of gas, and the switchgear must of course be accordingly constructed. I do not consider, however, so far as I have seen at present, that the total enclosure of large motors is satisfactory. It is first of all very expensive, and even if labyrinth or gauze protection is used there is a liability of the apertures becoming filled with coal dust and the ventilation greatly interfered with, so much so, that the motors might as well be totally enclosed.

If the air can be taken from the intake, this affords a solution of some of the difficulties. I may state, however, that taken

broadly I regard total enclosure as a mechanical protection only, because one must remember that, however perfect a machine may be at starting, there is always the risk of any doors or covers being left improperly secured. This leads one to the conclusion that where there is any real danger of gas in a colliery it is far better to install compressed air plant in that particular district.

Fortunately, electricity enables us to carry our power a long way inbye to a point where compressors can be placed supplying air to the auxiliary machinery; but, even then, the losses in a compressor and the conversion are necessarily great. In this connection it may interest you to know that the average disc coalcutter absorbs about 600 cub. ft. of free air per minute at 45 to 50 lb. pressure, and it is not economical to work at a higher pressure. Therefore, allowing for pipe losses, each coalcutting machine would require about 70 to 75 h.p. in the motor compressor. This means that the efficiency is something like 30 to 40 per cent, compared with the direct use of electricity. In other words, assuming that power is taken from the surface, it will be necessary to provide nearly 100 h.p. for each coalcutter with air-compressing machinery, against about 40 h.p. in the engine for operating the dynamo. Capitalized, this means, assuming a horse-power is worth £5 per annum, that if coal owners are forced, either by the action of the miners or by unreasonable restrictions, to put in compressed air coalcutters an additional annual expenditure of about £250 per coalcutter operated would have to be incurred.

In considering an electrical coalcutting scheme where three-phase coalcutters are used, I see no reason why electric motors should not be wound for a very low pressure, possibly as low as 110 volts, which could not cause a dangerous shock. This could readily be done by combining small transformers with the gate-end switches, and making a portable arrangement which could be moved about as the face advances. This would enable comparatively high-tension current to be conveyed by small cables near to the working face; and I think it would possibly result in a lower cost of plant all round than if the standard voltage of 500 were adopted.

Pumping Machinery.

Possibly one of the most important applications of electricity underground is in connection with pumping, and there are many mines to-day where it would be almost commercially impossible to pump by any other means. For many years the horizontal three-throw mining pump has held its own, and, as regards efficiency there is no doubt that this type of pump, particularly for high heads, is much better than the centrifugal.

The high-speed electric motor, however, is eminently adapted for coupling to centrifugal pumps, and assuming that the water is reasonably clean and that centrifugal pumps are constructed so that they can be readily taken to pieces and all internal parts inexpensively and quickly replaced, there is no doubt that, on account of the much lower

price at which they can be supplied, and the reasonably good efficiency which can be obtained, this pump has justly come greatly to the front. Where the volume to be pumped is large and the head moderate there is no doubt that the combined efficiency of a pump of this kind is ahead of a ram pump. I still have the feeling, however, that centrifugal pumps are being used under conditions where ram pumps could be better employed, *i.e.* when pumping small quantities against high heads, and I fully anticipate that the time is not far distant when we shall see the fashion change, and the three-throw pump will again assume its proper position.

Electric pumping has a very large field at the coal face, and also for the unwatering of districts a long way inbye, and it would be difficult to imagine how such work could be done by any other means. Again, I am inclined to think that a portable type of three-throw ram pump fitted with a motor capable of dealing with almost any likely condition of head, and also capable of pumping dirty water and working sometimes on air, is a type to be preferred to the centrifugal.

Electric Haulage.

Enormous strides have been made in the application of electricity to haulage purposes. It is no unusual thing to see main and tail and main rope haulage gears running up to several hundred horse-power. These gears can be placed in the most convenient position for economical working underground, as the question of either taking steam or compressed air to them has not to be considered.

There is, no doubt, still a very large field for the introduction of small electric haulages, of the endless rope, main rope, and main and tail type, for dealing with cross roads and gateways. If any risk of explosion exists, compressors can be used inbye, as previously stated; but in many of our pits there is no risk of gas, and under such circumstances I am sure that horses can be very largely dispensed with underground with very considerable saving.

Other Applications.

It is impossible in an address of this kind to touch upon one-tenth of the economical application of electricity; but it would not be right to leave out the question of electric driving of rolling mills. With a cheap source of power, such as can be obtained either from blast furnaces, coke ovens, or possibly from a power company, there is no doubt that by driving electrically many of the rolls, and particularly the live roller gear and other auxiliary machinery, very great economies can be obtained; but I have doubts as to the real commercial saving in applying electric motors to very heavy reversing mills.

We are greatly indebted to Mr. Ablett and others for many interesting papers on this subject; but, as I mentioned in connection with winding, I have not yet seen any really reliable figures as to the saving in the cost of rolling per ton compared with a modern steam rolling mill with high-class engines and everything up-to-date.

Electric Smelting.

A very large field exists for the refining of steel, and the manufacture of ferro-alloys, etc., by utilizing cheap electric power in electric smelting furnaces. There are two types of furnace on the market at the present time, namely, the induction furnace (which was originally invented, I believe, by Dr. Ferranti), and the arc furnace. Both appear to have advantages.

The objection to the induction furnace, particularly with high frequencies, appears to arise from the washing effect of the molten metal on the lining of the furnace, and I understand that no satisfactory lining has at present been used to overcome this difficulty. In the ordinary arc furnace this objection does not exist. I imagine that as conditions vary so greatly there may be a field for both types of furnace.

There is no doubt that by means of electric smelting very high-class steels can be manufactured at quite reasonable prices, but I question very much whether electricity can be produced in this country at a low enough cost to enable a furnace of this class to be used for the manufacture of ordinary qualities of steel.

Electric Railways.

Where electricity can be produced in considerable quantities at a low cost there should be no difficulty in competing against the ordinary steam locomotive when one considers the enormous number of engines which are either standing in their sheds doing nothing, or are possibly standing on side lines with steam up and burning coal the whole time. The steam consumption also of the modern locomotive, although greatly improved by superheating and compounding, is necessarily very much higher than with steam turbines or high-class generating machinery.

That electricity will gradually supersede steam for railway work there is no question, and I can only hope that if this does occur we shall be able to keep the manufacturing of the plant in this country.

Ship Propulsion.

The propulsion of ships by a combination of steam turbines with alternators and motors appears to open up a new field, and one which may develop considerably. The difficulty at present when turbines alone are used in ships which are required to travel usually at not more than half speed, is the high consumption of steam per horse-power. By placing an alternator on the turbine shaft, and a motor arranged for, say, two speeds or more on the propeller shaft, the turbine can always be kept up to approximately full speed.

This method of ship propulsion is extremely attractive ; but one has to bear in mind that, where the life and soul of the ship depend upon its propelling machinery, anything placed between the actual turbine shaft and the propeller shaft must be of the most substantial and reliable character.

Future Developments.

It is impossible with the rapid progress that has been made in electrical science to say what may happen in the future. We hear of proposals to generate electricity by producing gas in the coal seam itself and conveying it to the surface to drive electrical machinery. Is it not almost equally possible to imagine that some day we may have a wireless system of distribution. It may sound absurd to make this suggestion, but one cannot tell what may happen.

There is one point upon which I should like to touch before concluding, and that is, the standardization of electrical machinery. Very valuable work has been done by the Engineering Standards Committee in many directions, and it would be satisfactory to see standard conditions of contract which have been thoroughly considered not only by contractors but by consulting engineers and purchasers.

I believe that the Institution of Electrical Engineers is agreeing upon a form of contract which I hope will be generally adopted. I also think that the question of consequential damages in connection with the supply of machinery should have very careful consideration.

MANCHESTER LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

ARTHUR A. DAY, Member.

(*Address delivered* 1st *November*, 1912.)

(In the absence of the author, owing to indisposition, the Address was read on his behalf by Professor E. W. Marchant.)

Before reading my Address I should like to thank the members for electing me to the honourable position of Chairman of the Manchester Local Section, and to express the hope that during my year of office, which coincides with greater activity of the Institution under new Articles of Association, we shall increase in numbers and importance, and that under the new regime we shall experience that renewed vitality which the alterations have been designed to effect.

I do not propose to read a technical paper, but to offer for your consideration a few matters which I think might help forward the great objects of the Institution of Electrical Engineers. I think it is unnecessary for me to apologize for taking this course, seeing that not only is the application of electricity to everyday life of vital importance to the members of the Institution, but it is also of national importance inasmuch as it involves such large issues as—

1. The abolition of the smoke nuisance in our large towns ;
2. The conservation of our national supply of fuel ;
3. The utilization of waste products ;

and many other problems of a like nature, in all of which it is beyond dispute that the supply and application of electricity must take a prominent part. The enormous increase in the demand for electrical power which has taken place in recent years, both in this country and abroad, is only an indication of the proportions to which this branch of the industry will attain in the future ; in fact, I think that it is not too much to say that a cheap, efficient, and universal supply of electricity will be essential to any nation which retains its position in the front rank of nations either commercially or politically.

I do not wish to underrate the progress which has been made in the electrical world for the last thirty years. It is considerably greater than was expected, having regard to the fierce opposition of established

rivals. It is with regret, however, that it has to be admitted that relatively to other countries our electrical manufacturing industries are not as prosperous as they should be, and in making comparisons with other large industries such as cotton and iron, one is at once struck with the relatively small amount of export business which is done in English electrical machinery. This is one of the directions in which our Institution, in conjunction, say, with the Government, might be able considerably to help the manufacturers. An increased share in the foreign trade, together with the increase in the home trade which must come, should help them to take up their proper position in the industries of the world.

The first essential to the increased use of electricity is a cheap and efficient supply. This we are now in a fair way to attain in our large towns and cities; and a comparison of our own rates of charge with those in vogue on the Continent and in America will not show English towns and cities at a disadvantage. Notwithstanding this, our increase in output is not advancing as it should under such circumstances. This is partly due to inertia brought about by established methods, and partly to the conservatism of British business men and their ignorance of what can be done here and what is being done elsewhere. In other words, the main problem is commercial rather than technical. It is in this connection that the importance of the change in our Articles of Association is shown. It is true that some supply authorities have in recent years established publicity departments, and no doubt these are doing a useful work as far as they go, but they are too parochial and limited in their sphere of operations to attain the object aimed at, and in many cases are adopted to overcome a lack of business due mostly to too high a tariff of charges. The same amount of effort expended on a large scale would, if put forward by a responsible authority, have much greater effect and be relatively more economical. Although the problem of increasing outputs and encouraging the use of electricity is, as previously stated, mainly commercial, under existing circumstances it is not entirely so, for the same arguments apply to the technical side of the question, and the increased use of electricity involves doing everything on a larger scale from power station to publicity.

Owing to the way in which electricity supply has been developed in England there are far too many generating stations; and to obtain the efficiency due to large outputs at good load factors some of these will have to be converted, in whole or part, into sub-stations. Moreover, not only are there too many, but there is too great a variety in the type of current supplied, necessitating different classes of apparatus for its application. This again prevents the manufacturer from producing in quantity, and so increases the cost of apparatus; moreover, it so confuses the ordinary man in the street, who is naturally suspicious of something he does not understand, that he decides to leave it alone until somebody else has tried it. Simplicity is undoubtedly a large commercial asset, and the fact that the simplicity of electricity supply

is not now understood by the business man, is a handicap that must be overcome.

One has only to took at the history of our railways to see the large amount of money that was thrown away in consequence of the difference of gauge, which had to be converted to one standard; and before it was so converted the loss through want of interconnection must have been incalculable. Even at the present time in connection with tramways the same want of uniformity in essentials is seen, and certain towns, between which intercommunication is of vital importance, cannot have through tramway communication in consequence of different gauges. In electric tramway systems, in England at any rate, a greater uniformity of plant and systems exists than might very easily have been the case, for reasons into which there is no necessity to go; but I am afraid it cannot be attributed to deliberate design or foresight. If this had not been the case it is easy to realize the hindrance which would have been felt if a uniform voltage on trolley wires had not been adopted. The electrification of our railways is bound to come in the near future, and with the example of the steam railways and tramways before us, is it not of the utmost importance that some effort should be made to ensure uniformity, if not in detail, in main essentials? The same argument applies to electricity supply for general purposes, only with even greater weight, in my opinion. It is quite true that it is an easy matter to convert electrical energy from one form to another, but each such transformation necessitates capital expenditure and a loss which, if small, is continuous. Moreover, if electricity is to be universally adopted it must be supplied at the lowest possible price; every unnecessary loss is a handicap to the attainment of the object which we all have at heart, and which, therefore, should be one of the principal objects for which our Institution exists.

It may be contended that any system of uniformity would tend to discourage invention and enterprise. I cannot myself see that this would be so; but even if there were a slight tendency in this direction, the enormous gains would more than compensate.

A careful perusal of the ideal system of generation suggested by Dr. S. Z. de Ferranti in his Presidental Address * to the Institution in 1910 will, I think, suggest that the enormous saving of 90 million tons per year in our national asset, coal, is alone sufficient to justify our aiming at such an ideal and at once taking steps, as far as is possible, to work towards it. We may not be able to attain to it, but it clearly points out the way we should go.

Even in smaller matters very large economies are to be obtained by uniformity. I think it was Mr. S. J. Watson, in his Address † to this Section in 1909, who pointed out that a capital saving of £500,000 could be obtained by interconnecting the electric supply networks of the towns just round Manchester.

* *Journal of the Institution of Electrical Engineers*, vol. 46, p. 9, 1911.
† Ibid., vol. 46, p. 102, 1910.

It may be considered by some, perhaps, as rather late in the day to bring forward any suggestion such as I am now making; but I do not think that this need be taken into consideration. Our supply stations and networks are going to be in the future infinitely larger than they are to-day, and if extensions when made were controlled by certain general considerations, and plant when replaced were similarly dealt with, we should soon see a great difference and a greater tendency to uniformity. Moreover, it would not be necessary, or even desirable, perhaps, to aim at uniformity of the whole country on one system, as matters have progressed in a different direction as regards systems in some districts as compared with others, and the system most in use in any given area would, I suppose, be generally accepted as the system for that district.

It may be contended that in bringing these suggestions to your notice I am but pointing out the obvious; but the very obviousness of some of the points raised is, I think, my justification if nothing or very little has been done to put into practice what is really only common sense. If it is conceded that these things are desirable, viz.:—

1. Large generating stations with large units, etc., in the interests of low cost of production;
2. Co-operation between existing systems of supply with a view to economizing in the cost of station plant, and also in the cost of distribution;
3. Uniformity of systems of supply as far as practicable and at any rate in adjoining districts in the interest mainly of the manufacturer, but incidentally also of the consumer, by reason of the reduced cost of apparatus;

Is it not within the scope of the Institution to do anything towards attaining these objects eventually, and moving in that direction now? I believe it is.

I would suggest that committees be formed of members of the Institution representing each interest involved, viz.: supply authorities, manufacturers, contractors, and the commercial community. They would obtain all information necessary for a thorough consideration of the question in the area they are appointed to inquire into, and would recommend the lines on which existing supply authorities should extend, and so on to a main committee consisting of delegates from the sectional committees who would consider and revise the whole of the recommendations, if necessary. Having through these committees obtained the lines on which progress should be made, the Institution could approach the Government of the day; and seeing that the movement would be in the interests of all concerned, and also in the interests of national economy and system instead of chaos, I see no reason why the said committee should not be allowed to co-operate with the Board of Trade and the Local Government Board. As these Government Departments are important factors in the starting or extending of any supply undertaking, and are taking more interest in

the question every day, they would be able to influence considerably the type of supply started or extended.

It is, of course, impossible in an address to give anything more than a mere outline of the lines that should be taken, and no doubt a certain amount of diplomacy would be required in bringing matters to a successful issue; but if the public and the Government clearly understood the importance of the matter nationally, and the way in which it would simplify matters for the Government as well as the public, I feel that at any rate it would have a measure of success. I want it to be clearly understood that the whole process would be gradual, and would be intended to prevent the extension of the present want of uniformity and cohesion between systems rather than the enforcement of any particular system.

The Institution of Electrical Engineers, representing the best thought, knowledge, and experience in electrical matters, should have a voice and influence in Government policy with regard to the electrical industry and when legislation is being put through Parliament. In such matters, for instance, as the Regulations for the Use of Electricity in Mines, the real facts should be put before the Government by some responsible body properly qualified to give an opinion, and so prevent absurd regulations being sanctioned by Parliament simply on party lines or as the result of panic.

It is essential, I think, that our Institution should be recognized more by the Government and Government Departments, and taken more into their confidence. First, because in an industry so highly technical and involving not only business but scientific questions, it is not to be expected that the ordinary Member of Parliament will have the necessary information to form a wise opinion unless he has the assistance of those qualified by business and scientific training to give advice; and, secondly, because without the assistance—even the sympathetic assistance—of the Government it would be practically impossible to carry out such a scheme as that suggested by Dr. Ferranti. To take one item only, that of wayleaves, those who have had experience in the matter of obtaining wayleaves for electric transmission lines will realize that before such a scheme could be carried out it would be necessary to have compulsory powers for obtaining wayleaves, and I see no reason why they should not be granted. In the case of other interests, such as railways and canals, which are more or less of national importance, it is recognized that private interests must, under proper regulations, take a secondary place; the same principle ought to be recognized where energy is transmitted instead of goods and passengers. In many other ways a very little consideration will make it quite obvious that, where a scheme is national in its aims, if it is to succeed it must have, and certainly is justly entitled to, the help and assistance of the National Executive or Parliament.

Recently the Dominion of Canada has appointed a Hydro-Electric Power Commission of Canada. Although this Commission has very

much larger powers than I suggest, it is a recognition by the Canadian Government that it is necessary to have an overriding authority to control and systematize the use of the natural sources of power in the country. If this is necessary where the power resources are mainly water, and consequently everlasting, how much more is such a system of control necessary when the main source of power is a gradually diminishing quantity, as is the case with our coal.

The gradual but steady change-over of private sources of power supply to the electric supply authorities in our towns and cities foreshadows the time when these will be dependent for the supply of power for all purposes, as well as for light, upon large power houses either within or without their borders. Having in view what has taken place in our own country this last year, it seems likely that such power houses will have to be built on lines which will enable them to withstand a long siege. The number of employees in such a station would be relatively small; but, nevertheless, the plant should be made as nearly automatic as possible, with provision for a large reserve supply of coal, for the failure of the source of power, light and heat would be little less disastrous than the stoppage of the water supply.

Those who have had the responsibility of maintaining the continuity of electricity supply during the past twelve months or so, even where this has been on a comparatively small scale, have, I think, had brought home to them more forcibly than ever before the importance of electric supply to the community. How much more important will it become in the case of the larger schemes foreshadowed? It will indeed be a matter of national concern. Not only is this the case, but the public seemed to realize the position during the coal strike, and they now realize that, if the supply authorities had shut down, the results would have been even more disastrous than they were. The Government also seemed to realize the seriousness of the position, judging by the anxious inquiries which they made during the strike as to how long the public supply was able to last. It seems, therefore, a proper time to put forward a plea for a greater amount of co-operation with the Government. The danger is that the cause of anxiety having for the time being more or less ceased the matter should be shelved, and the obvious lessons to be derived from the strike lost sight of.

The fact that employees in electric power stations are not under the same obligation to continue at work in periods of public danger or crises as in the case of the employees of gas works and of water companies, is a proof that the importance of electricity supply to the community has not been properly recognized. I think it is our obvious duty to get this remedied at once.

From these considerations it is, to my mind, evident that to arrive at a scheme of electricity supply such as that indicated by our President in the Address above mentioned, it is necessary that such a scheme should be carried on in conjunction with the Government. I do not

mean it should be in the hands of the Government; but, inasmuch as the objects would be national and the advantages national, to get the full benefit of the adoption of such a scheme the Government must be consulted, and it would have to be worked in conjunction with them.

The insularity of the British Isles has advantages; but it also has its disadvantages, one of the latter being that with our present system of agriculture we are not self-supporting in the matter of foodstuffs. This at any time may be a matter of the most serious national importance. It should, therefore, be a national matter to encourage any system of intensive culture of land which would tend to lessen our dependence on foreign food. We all know that electricity can be used in this direction. Is it too much to ask the Government for a grant and to help generally to start this system on a large basis, seeing the national importance that the food supply may become at any time? A larger production of the national food supply at home is certainly a national matter, and would enable a larger proportion of our fleet in time of war to be used in direct hostilities, instead of having to be employed in keeping our trade routes open.

A comparatively small sum would effectually prove the profitable nature of intensive culture to the farmer, who would then adopt such culture from commercial motives; and if not, it would be the Government's business to see that it was done. The Dominion of Canada, recognizing that the proper cultivation of land is of national importance, are open—and especially lay themselves out—to help intending settlers on the land with advice, analysis of soils and suitable seeds, etc., and, moreover, give prizes for the best results. It is true the objects would be somewhat different in our own case, but they are equally important, and it would be stimulating the use of one, if not the most important, of our national assets. Is it not possible for our Institution, in conjunction with the Government, to do something in this direction?

Not only could electricity, from a nationally controlled scheme, be used directly in the interests of agriculture, but by its use in the chemical production of artificial fertilizer by the fixing of the nitrogen of the atmosphere, or by its use in chemical processes generally, as in the manufacture of electrolytic bleaching powder, or for the production of disinfectant, or the sterilization of water, and generally by its use in processes which would be continuous but which could be temporarily interrupted if necessary, a load factor hitherto undreamt of could be obtained for that scheme. The results as regards the cost of production would be of the utmost importance nationally, and, besides saving our national asset coal, would probably lead to the introduction of other industries which, without such a scheme, could never be started.

It is not necessary for me to point out to you more examples of the way in which electricity could be of use nationally. It enters into every phase of the life of the people at the present. It becomes more a necessity of our lives every day; but I feel that the best use is not

being made of its many applications already known, to say nothing of those which might be revealed by a larger amount of research work carried out by our best scientifically trained men. Further, so far as its universal supply is concerned, there is not that coherence and singleness of aim which is necessary to obtain the best results. I do not plead for Government ownership ; I do not believe that that would be beneficial, but I do think we should have more recognition and help from the Government, seeing that the application of electricity is so vital to public interests and touches national interests at so many points. I also think and hope that with our new Articles of Association much more will be able to be done in the direction of influencing the Government to give to the science of the application of electricity to modern needs in every direction more consideration and encouragement. If the efforts which may be made in this direction are not altogether successful in a positive sense, they will, at least, be so in a negative one, and prevent us from being hedged round with so many unnecessary regulations and restrictions that our national growth is impeded.

Those who remember the earlier legislation with regard to electricity supply will realize that we have suffered from unwise legislation ; and this is still the case to a certain extent. It is only by the Institution, representing those who know the true facts, making themselves felt in all cases of Government interference, legitimate or otherwise, in matters which concern the supply and application of electricity, that this can be prevented.

The advantages to be derived from original research are not, I am afraid, appreciated in England as they should be. Successful research in any department practically means being first in the field with the application of science in any particular direction; and to be first in the field with any new invention or discovery of importance is of the utmost value, and, moreover, means very often retaining control of that branch of the industry, with very desirable results in a commercial sense. This is often brought very prominently before us ; one recent instance being that of metallic filament lamps.

In all cases where the prosperity and scope of the industry concerned justify it, original research pays if properly carried out. Unfortunately, English electrical manufacturers are in many cases not sufficiently prosperous to enable them to consider the question properly. Undoubtedly great advantages would accrue from a larger recognition of the value of original research in connection with the electrical industry. I feel it will therefore interest you to know that the Institution of Electrical Engineers is taking this matter up seriously, and that this Section is taking a prominent part in it. I am sure there is room in England for some large industries such as have been established on the Continent and elsewhere as the outcome of original research.

If one looks at electrical progress in a large way and considers what is being done the world over, one cannot escape the feeling that

we in England are not getting the share which we have been accustomed to get in the large industries of the world. Our steam railways have led the world; but in electrical railways we hear of projects in America and on the Continent, and even in quite new foreign countries, which quite overshadow what we can show in England. Again, in metallurgical work we have been accustomed to consider ourselves, and justly so, in the front rank; but we hear of the application of electricity to this branch of manufacture being carried out on most extensive lines, something far more than we can show. The same remarks apply to other industries to which electricity is being applied, and I want it to be understood that we, the Institution of Electrical Engineers, representing the profession and industry of electrical engineering in England, have a responsibility in this matter that we dare not shirk; and that whatever there is that we can do must be done to keep the electrical industry in England in the front rank of the industries of the world.

DUBLIN LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN.

Owing to the unfortunate accident at the Bray Electricity Works on 10th July, 1912, in which Mr. W. J. U. Sowter, Chairman of the Dublin Local Section, was seriously injured, the Inaugural Address to this Local Section for the Session 1912–13 was not delivered.

YORKSHIRE LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

S D. SCHOFIELD, Member.

(*Address delivered 6th November*, 1912.)

To-night I propose to ask you to consider a few points in connection with electricity supply, in which branch of electrical engineering I have been engaged for about twelve years.

One lesson to be derived from the coal strike in the early part of this year is that it is incumbent upon all supply authorities to hold very large stocks of coal, and I consider that a six months reserve stock is by no means excessive. Ordinarily, owing perhaps to the very excellent engineering work done for our large textile and other factories, the running and maintenance of the power plant receives but scant attention, and it is only at exceptional times that the average factory owner realizes how dependent he is upon his engineer.

It is becoming a common practice for large works and factories to fall back on electricity supply undertakings when they find themselves unable to obtain coal for steam raising, and when breakdowns occur, and temporary consumers thus secured often become permanent, and turn out quite profitable. This makes it necessary to hold larger stocks of fuel than were formerly required. In providing for these contingencies it might, at some stations, under certain conditions, be sound policy to supplement a coal reserve by storing low-grade oils. This may be done by using obsolete Lancashire boilers as storage tanks, fixing them in such positions as will allow the oil to gravitate to the front of the furnaces.

At our annual dinner in February, 1910, Dr. Kapp remarked upon the tremendous developments that were taking place in the electrical driving of textile factories. The outcome of his remarks was the appointment of a committee to enquire into this subject, and to tabulate all the available information. It is of course important to know the methods which have been found most successful from an engineering, manufacturing and financial standpoint, both in this and other countries. The Textile Institute were invited to join the enquiry, as it was felt that it would widen the scope of the investigations if the active co-operation of the textile industry could be

secured. While this subject is of interest to electrical engineers, it is of no less interest to the captains of industry in the textile world. The Textile Institute, however, after several joint meetings had been held, intimated that they preferred to continue their investigations independently. Provided the work is done, and done thoroughly, it is not very material who does it; but it appears to me that since Dr. Kapp first stimulated enquiry upon this subject, manufacturers have found where their interest lies, and are adopting electric driving as quickly as it can be installed.

One of the main things to be considered in the future is the fixing of standard pressures and periodicities. Engineers interested in the manufacture of motors or incandescent lamps well know what a boon this would be to them. The day of small supply stations is passing away, and if electrical energy is to be universally adopted for industrial purposes, at prices which will be profitable alike to the producer and consumer, it can only be done when generating with large units at stations where fuel can be obtained easily and cheaply, and where an abundant supply of water for condensing purposes is available. Generating stations which were put down to supply direct-current, and which for economic reasons were necessarily in the centre of a given area, will be dismantled and utilized as sub-stations, taking current in bulk from a large central station. The question as to whether these large stations will be controlled by companies, or by groups of local authorities, will settle itself in the future.

The jealousy so often existing between local authorities may be a stumbling block in the way of group control. In past years this jealousy has often acted to the disadvantage of the community. Some years ago, a local tramway department operating a service of cars in its own town, and also, under a lease, operating a through service to an adjoining town, purchased current from the supply station in each town. One winter, owing to one of the stations being overloaded, the service of cars in that particular district had to be curtailed during the peak load. The other station, which had plenty of spare plant, offered to supply extra current which would have kept a full service of cars running in the adjoining district until the station in that town could again take on its full load. The tramway manager would gladly have availed himself of this offer, but the "Town Clerk's department" pointed out that there were legal difficulties in the way, with the result that the public suffered through a restricted car service. If this spirit entered into the policy of a group of local authorities controlling a large electricity supply undertaking, it would militate against its success.

The practice of taking large sums of money from the profits of trading departments is very dangerous. Let the prices in each trading department be adjusted to leave a fair margin over production or operating costs, as the case may be. If any sum has been taken from the rates in support of an undertaking in its early days, by

all means return such sum back to the rate fund. After providing for interest and sinking fund charges, repairs, maintenance of plant, etc., provide a fund for replacing obsolete or un-economical plant by more efficient units. Create and maintain a reasonable reserve fund, but not too large, as it offers too big a temptation to local chancellors of the exchequer, who naturally wish to keep the rates as low as possible. When all this is done, such short-life apparatus as meters, services, street lighting equipment, motors and switchgear, cooking stoves, etc., should be paid for out of revenue. In my judgment any undertaking which did that could be considered on a sound financial footing, and if there was still a large balance left it would be high time that a "Consumers' Protection Society" was formed.

At no time in the history of electrical engineering was there such marked activity in all its branches as there is to-day. The advantages of electricity for heating and cooking are now being realized, and if this new demand is properly developed it will mean not only increased quantity of apparatus in use, but also new and heavier mains, and additional generating units.

One result of the very excellent plant now provided for electricity generation and distribution is a tendency on the part of engineers to become stereotyped in their methods, and in too many cases to refuse new business because it does not fall in with "red tape" conditions. I remember some years ago when a traction undertaking was ready for running the first trial car, the supply station—owing to delay in the completion of contracts—was without traction switchboard, or switches, fuses, ammeter, voltmeter, watt-hour meter, circuit-breaker, etc. The engineer, recognizing that the success of an undertaking often depends on first impressions, fixed up a temporary switchboard equipped with two single-pole switches and fuses, made temporary connections, and when all was ready switched the battery on the line without voltmeter, ammeter, or circuit-breaker. Before the regular service of cars was commenced the proper equipment was ready for use.

In another case, textile machinery had been installed in a new factory, and the owner was anxious to start running, but was unable to do so because of the delay in delivering the engines and boilers. An agreement was made to take 40 to 50 h.p. from the electric mains for a few months; but before 12 months had elapsed the demand was for 200 h.p., and the factory owner, recognizing the advantages of an electric drive for textile work, has now decided to couple a generator to his engine, and convert the whole of his power transmission to electric driving. During the peak loads at the supply station, whilst this particular load was on, lead-covered paper-insulated cables were run at a current density of 2700 amperes to the square inch. The pressure drop was 100 volts on a 560-volt pressure; and the difficulty was overcome by running this particular feeder on a separate generator during factory hours,

quite apart from the remainder of the supply, using the three-wire balancer for the ordinary area, and balancing the special supply by means of the battery, connecting the two ends and the middle point of the battery to the positive, negative, and neutral busbars. In many other instances throughout the country, additional consumers have often been obtained by the readiness of supply authorities and companies to step in and supply electrical energy at short notice, when the ordinary power units (steam or gas) have broken down. Apart from the particular advantage to the firm in difficulties, these cases are striking advertisements of the manifold advantages of electric driving, and of the ease with which electrical energy can be obtained in emergencies.

I mention these cases merely to emphasize the point that supply engineers should always be open for business if there is a margin on the right side in it, even if it compels them temporarily to overload plant and mains. In these days of first-class plant and standardization there is a tendency to practise caution until it ceases to be a virtue. Procure the best machinery available for the particular purpose for which it is required; have it well cared for, remembering the golden rule "A stitch in time saves nine," and when the emergency does come, and the unexpected happens, this can easily mean the difference between "a very busy time" and a complete "shut-down." The human element is also an important factor in securing continuity of supply. To obtain the best results, diverse methods are necessary with different men. Discourage favouritism, and encourage ability. If a man is competent and reliable, give him a certain amount of responsibility and latitude. Make his position comfortable, and encourage him to think about his work. The average man will value his position all the more, and in an emergency will respond for all he is worth to get this machine ready, or that joint made, or the other motor fixed, or keep steam up, etc., as the case may be. Moreover, an appreciable amount of a chief's time in superintendence is saved, and the work goes on as well and as quickly when the foreman's back is turned, as when he is watching. It is a good thing for a junior to obtain a reputation as one who can be relied upon. There is no easy way to obtain it. It can only be acquired by hard work every day. Study your own work until no one else can do it better and very few as well. Then study the work of the next man above you until you can do better than he. It is surprising how much easier the work will be to you when he gets a lift and you step into his shoes. Don't think that 100 per cent efficiency in your work one week, and 50 per cent efficiency the following week, is as good as two weeks at 75 per cent efficiency each week.

These are a few reflections after a long experience of electrical supply. Although I am now leaving it, and taking up an appointment of an entirely different kind, I can conscientiously say that I know no branch of engineering which is more varied and interesting, or which gives such a practical all-round training to the embryo engineer.

SCOTTISH LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

WM. McWHIRTER, Member.

(Address delivered 12th November, 1912.)

Not only do we begin this meeting under a practically new constitution, but we must look upon this year as inaugurating a new era for our Institution. Our Section had the honour of receiving the first visit of the parent body from London, and I am sure that I voice the feeling of all the members of the Section when I say that we rejoice at the successful manner in which the Summer Meeting was carried through. No doubt some of the arrangements made by your Committee might, and would be, improved upon at any future meeting; but, when it is considered that we were entirely without precedent to guide us, it is pleasing to know that the plans of your Committee were carried through without any serious hitch. The Section is under a debt of gratitude to the Lord Provost and Corporation of Glasgow, the University of Glasgow, and the Royal Technical College, for the generous assistance given us. We are also deeply indebted to the various firms who threw open their works for the Institution and entertained the members. To the foregoing might I add that we are grateful to the committee of ladies who gave us so much assistance; and last, but certainly not least, to our Honorary Secretary, Mr. Jas. E. Sayers, for his untiring work planning and preparing, and for his never-failing courtesy.

The difficulty of determining the subject matter of an address becomes yearly more and more pronounced; but the long connection I have had with the Institution, extending to nearly forty years, invites me to glance backwards in the hope that this may be useful and may enable us to compare past with present practice and progress.

The nineteenth century was certainly a time of wonderful discovery and invention, and no department of science made greater progress than that of electricity and its twin fellow, magnetism. During the eighteenth century we had the discoveries of Galvani and Volta, followed by the work of Davy, and extended by the researches of Faraday, who, finding the science of electricity only a mass of experimental records and vague theories, devoted practically his life to inves-

tigation and elucidation of the little that was known about electricity when he entered the Royal Institution in 1813. The first period of his work there was mostly occupied with chemical investigation, and his opinion as an expert was much sought after. In 1830 his fees for such work exceeded £1000; but, fortunately for the world at large, he then decided to give up such professional work entirely and devote himself to original research.

From that time on he made the most wonderful series of discoveries almost continuously and without intermission until about 1860. In 1831 he discovered magneto-electric currents, or, as he called it, the "Evolution of Electricity from Magnetism." He thus established the correlation of electricity and magnetism that was suggested by Oersted eleven years before, a discovery which was to culminate in the construction of the many forms of dynamo and motor that now exist.

It is not my intention to say more about the work of Faraday; he ought, however, to be remembered as a worker we should all attempt to emulate. He commenced his work with none of the advantages that are open to the present-day student, and we can never sufficiently appreciate what he did for those who have succeeded him. I would advise every one of our young men to read carefully his "Experimental Researches." These volumes are the most complete record of electrical progress during the first half of the last century, and a model of how research ought to be carried out.

The period immediately following the active work of Faraday was mostly taken up with the perfection of telegraphy, both land and submarine; and here the outstanding genius of our first chairman, Lord Kelvin, enabled him to cover practically the whole field. Meanwhile the generation of electricity by means of magneto-electric induction had not been neglected, and a number of inventors were busily engaged upon the production of apparatus for this purpose.

There was, however, no rapid progress, largely, I believe, owing to the fact that no system of measurement in electrical units had as yet been devised, and, so far, everything was qualitative. In my opinion the ability to carry out exact electrical measurements did more for the advancement of the electrical industry than any other factor, and we should not forget that we are indebted for the splendid system of electrical units now at our command to the action taken by the British Association, who, in 1861, appointed a committee to determine what form and dimensions those units should have. In 1865 the first British Association units of resistance were issued; and in 1873 the name "ohm" was adopted for this unit.

Although the British Association intended that copies of the unit should be available at a reasonable price to all who applied for them, yet, curiously enough, no machinery was set up to prepare these, and it was only in 1876 that the author was able to obtain a copy made from one of these ohm standards. From this the ohm now shown was made by me and certified by Mr. Charles Hockin, who had prepared the units issued by the British Association, and had been

responsible along with Dr. Matthiessen for most of the research work carried out by the British Association Committee.

This coil has again been verified at the National Physical Laboratory (1903), and it appears that the value has fallen by about $\frac{1}{1000}$th in twenty-six years.

At this time, measuring instruments of any kind were practically unknown, or were only then coming into use in the Postal Telegraph Department and by railway companies. Generally there were no means whatever of locating a fault on telegraph lines, and the reports of faults to the linemen consisted in the intimation that a contact or "earth" existed upon certain lines, between test boxes probably 50 miles apart.

For the measurement of currents no instruments existed, and all such attempts were merely guesswork. So long as batteries, instruments and lines were of high resistance it was possible to form some idea of the battery condition by using an ordinary detector galvanometer ; but when railway-train signalling requiring the use of considerable currents was introduced, then the resistance of the instruments and circuits was kept low, and proper testing (owing to the lack of some instrument measuring in units) was wanted very badly.

My first attempt to remedy this was by the introduction of a galvanometer graduated into units called "chemics." This unit was based upon the well-known law that in electrolysis a known current would always liberate a definite weight of metal from any metallic solution, the amount of course varying with the metal used. The "chemic" as a unit of current was proposed by the late Mr. J. T. Sprague, of Birmingham, and the unit was such that in ten hours it would liberate 1 grain weight of hydrogen. Galvanometers so graduated were largely used by electroplaters, and many were graduated to show the value of the deposit in electrolytic baths in grains or ounces per hour.

The instrument introduced by me was divided in the usual way to read to tenths of the unit of current, but the telegraph inspectors and linemen had great difficulty in recording the reading correctly. This was overcome by using an extra scale, divided up and marked with the words, "Very Good," "Good," "Fair," "Requires Attention," and "Bad." The galvanometer was wound to 100 ohms resistance so as to represent an average circuit including the instruments, line, earth, etc., and when applied to the battery terminals at once showed the condition of same. This rough arrangement was very successful and reduced faults by nearly 90 per cent.

For testing the insulation of telegraph lines which had to be checked daily, the Post Office had introduced a form of tangent galvanometer with shunts, etc. This instrument was not suited to give quick and reliable work, so we introduced a form of galvanometer which was graduated to read in volts, webers and ohms. It also was made by Mr. Sprague to our specification, and proved to be a most useful instrument for general tests. The principle of it is very simple.

It depends for resistance measurements upon the use of a Daniell cell. This was easily set up, and the E.M.F. of the cell could always be depended upon within limits, and could be checked by using any known resistance.

About 1878 electric lighting was coming to the front, but still very little progress had been made towards supplying instruments measuring in volts or amperes. In fact, if you refer to works on electric lighting about this date, you will find that neither the pressure nor current is given in units.

The Paris Electrical Exhibition of 1881 gave a great impetus to electrical matters generally, and there were several instruments shown measuring pressure and current; but in all cases these had scales divided in degrees, and the values in volts or amperes were found by multiplying the deflection by a constant, or in other cases the square root of the reading had to be found and multiplied by a constant. Shortly afterwards instruments were produced which were direct-reading; and now we have instruments which have reduced measurements of all kinds to the simplest matter of observation.

The manufacture of electrical machinery has, in my opinion, made the most wonderful strides largely due to the increase of knowledge arising from the ease and accuracy with which electrical measurements can now be made.

In 1875 a Gramme dynamo for lighthouse work was shown in Glasgow. Speaking from memory, I should say it was about 5 ft. in length, 30 in. wide and 18 in. in height. It must have weighed not less than 40 cwt., the output would not exceed 6 kw., and the cost, if I remember rightly, was not less than £200. At the present time a dynamo of this weight might easily give 60 units, and the price would certainly not be more than one half.

Until 1886 the design and construction of dynamos was wholly a matter of trial and error, and so long as any well-known and proved design was followed fairly good and concordant results were obtained; but attempts to depart from any successful design by increasing the size often ended disastrously. For example, hundreds of what were known as the "A" Gramme dynamo had been made and were in regular and successful use, but when a larger machine was called for, and the "B" Gramme was designed and made, it was a complete failure. The wear and tear on commutator and brushes alone cost not less than 10s. per week. It had only one good point, and that was it lit up the dynamo room most effectively by means of the sparking at the brushes!

The Burgin dynamo, introduced into Britain by Mr. (now Colonel) R. E. Crompton, about 1881, did much to increase the use of electric lighting; but here the improvement was mostly mechanical, the machine being very imperfect electrically, or, should I say, magnetically.

As an example, about 1886 my firm designed and made a new armature with a slotted core and plain drum winding, which, used in a Burgin field, increased the output from 200 volts, 18 amperes, at

1800 revs. per minute to 250 volts, 20 amperes, at 960 revs. per minute. The efficiency of the dynamo with the old armature was about 65 per cent; whereas with the new armature it was about 80 per cent, and the wear and tear on commutator and brushes was practically nil. Looking back, I often think of the audacity with which the electrical engineer of these early days undertook all manner of work and generally carried it through with a fair measure of success.

In 1886 the splendid work carried out independently by the late Dr. John Hopkinson and Mr. (now Dr.) Gisbert Kapp cleared away all the darkness which had hitherto enveloped dynamo design, and the mystery of the magnetic field was fully explained. It then became possible, using the most elementary mathematics, to pre-determine the output of any dynamo, or to design a dynamo for any output within a very small percentage of error.

The use of carbon brushes, first suggested by Professor Forbes, was a wonderful step towards perfecting dynamo-electric machinery, and made designs possible which had hitherto been quite beyond our hopes.

After all, we are only now touching the fringe of the work which is before our Institution; we are only now coming into our own. The splendid work and researches carried out during the last century by Faraday, Maxwell, Kelvin, Hopkinson and a host of others, have raised our profession to an exact science and have given electrical engineering an importance which could not have been foreseen thirty years ago. What enormous growth has taken place since Faraday, in 1831, announced his discovery of the evolution of electricity from magnetism—the discovery which had been constantly before him since 1822, when he wrote in his note book "Convert Magnetism into Electricity"! Thirty years were to elapse before Faraday's work was carried forward, and then we had Pacinotti, Gramme, Wheatstone, Siemens and many others, all placing Faraday's work upon a commercial basis. But what a revolution has taken place since then!

Electricity, the handmaiden which had only given us telegraphy and electric bells in 1870, is now going ahead with giant strides. We have it doing work then undreamt of; driving our railway trains, tramcars, workshops and factories, winning for us the treasures of the mine, smelting our ores, and lighting our towns and cities: and who can say what achievements this century may witness?

The question each one of us ought to put to himself is—Are we taking full advantage of our opportunities; are we looking well ahead and preparing for future advances of electrical engineering? These advances must be very great, and I have no doubt the next decade will show rapid progress.

The consideration of new sites for power stations requires the utmost care, and it may be found expedient to locate these right on our coalfields, provided condensing water is also available in abundance. It would certainly be cheaper to transmit electricity than carry coal

to the power station, and vast stores of coal might easily be accumulated at moderate cost.

It is still fresh in our minds that early this year the coal strike brought us face to face with one of the greatest industrial upheavals which the nation has ever experienced, and during the time the strike lasted it is very much to the credit of the engineers in charge of our supply stations that they were able, not only to maintain their usual service, but in many cases to give additional supplies to manufacturers and others. Thus they prevented the throwing out of employment of many thousands of workmen at a time when each addition to the ranks of the unemployed meant an increased danger to the public weal. No one before the strike would have believed it possible that an industrial stoppage of such magnitude could have taken place without much greater disaster than actually occurred. We ought, however, to profit by the lessons we then received, and now some means should be considered whereby fuel can be stored up in such quantities as to enable us to deal confidently with such a crisis. Better far if means could be found and matters so arranged between capital and labour as to avoid such brutal methods of settling industrial differences. In addition to all the suffering endured, the money spent and lost by the miners and mine owners must have been enormous; to say nothing of the dislocation of transit and travelling which had to be paid for heavily. Surely, in this enlightened age, it is not beyond the power of our statesmen and leaders of capital and labour to devise means which would prevent a repetition of any such disaster.

We have on several occasions been warned recently of the slow but sure exhaustion of our coal supplies. Although coal may, and will, doubtless be discovered in virgin fields, yet there is little hope here in Britain of any great additions to our reserve; therefore the water power at our command here in Scotland must become increasingly valuable. No doubt, many new supplies will be tapped which have hitherto been neglected. It behoves us, therefore, to look around and see what can be done to find a substitute for or supplement to our coal supply.

There is stretching from Fort William to Fort Augustus that splendid chain of lochs which forms the Caledonian Canal; these lochs could supply an amount of water power which would far exceed anything we have at present in use. The level of Loch Lochy is 93 ft. above the sea. It is about 10 miles long, and has an average width of one mile. This loch, I am assured, could easily be raised to a height of 150 ft.; which would then at least double the storage capacity, and would connect it directly with Loch Arkaig, 11 miles long and nearly ¾ mile average width. This system is surrounded by hundreds of square miles of mountainous moorland, forming a magnificent gathering ground for such lochs, and with our present knowledge of high-tension electrical power distribution is within easy reach of Glasgow and the West of Scotland. The distance to Glasgow does not exceed 100 miles, and the route would nearly all be through country where there

should be no difficulty in obtaining the necessary wayleaves. I believe the power available could supply all the energy at present required in our vicinity. There is, further east, Loch Ness, 20 miles long, with an average width of one mile. The position is 60 feet above sea level, and might be raised to one hundred feet. Sooner or later such schemes will be carried out, and Scotland, by reason of her water powers, should be able to supply most of the energy required by her industrial population.

Just as the inventions of Watt and others have created the factory system, by bringing crowds of workers together in order that they might collectively use the power of the steam engine, is it not possible that the erection of power transmission lines all over the country, bringing power to every village and hamlet, may be the means of sending certain industries back to the villages and country cottages, thus preventing the overcrowding of large towns and cities, and directly assisting in solving the problem now so much debated, namely, How to get the people back to the land?

It is quite impossible even to glance at the various branches of industry which have sprung into being during the short period dealt with so inadequately in my address. Forty years ago, telephony was unknown. The steam turbine had not been invented; even rubber-covered cables had only been introduced. The incandescent lamp had only been dreamt of. During this period we have added the Röntgen rays and, only a few years ago, wireless telegraphy and many other epoch-making inventions.

I have finished; but to the youngest student of the Institution of Electrical Engineers I would say: Put your very best into your profession. You have a splendid inheritance, and it is worthy of all you can do for it. Go forward, and may success attend all your efforts!

BIRMINGHAM LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

A. M. TAYLOR, Member.

(*Address delivered* 13*th November*, 1912.)

I desire to thank you for the great honour you have done me in electing me your Chairman. One of the unwritten laws of this Institution requires that your Chairman shall deliver an address on some subject of general interest. I have thought that some remarks on Patents and Inventions might be of interest to the members of this Institution.

The prosperity of the country in general, and of the electrical industry in particular, depends to a greater extent upon new inventions than perhaps is usually supposed. A country which does not provide new developments in industry, is apt to stagnate, and will certainly be unable to compete with other countries which are providing these developments.

The "commercial element" of the country may arrange for the manufacture of standard apparatus on a vast scale and so reduce costs; but unless it is at the same time ready to seize upon the first clear step in advance in the way of producing new and useful results, or equal results by a new process at lower costs, all its commercial organization will have been in vain.

As an illustration of this, take the case of the steam turbine. It would obviously be merely courting disaster, in the long run, for the commercial element of the established industry (in this case that of the manufacture of reciprocating steam engines) to meet the advent of the steam turbine by offering to sell their engines at a cheaper and cheaper rate, and so endeavouring to keep the steam turbine out of their market.

The question then arises, whether it is not desirable, in the interests of the country at large, to give still greater stimulus to invention; as there is the obvious temptation for large concerns having abundance of capital, and control of certain markets, to seek to use this capital to enhance their profits, and to stifle competition from any new developments.

For example, a concern having large capital at its back can afford

to keep paid agents continually on the look-out for patent developments in the same, or in contiguous directions to those which it owns, and it can afford to buy up the patent rights (very often for a mere trifle, if in the hands of individuals), solely with the object of stifling any competition that might otherwise have been set up against its own standardized articles.

To take a concrete example: Suppose it had been possible for the manufacturers of reciprocating steam engines (I hope these gentlemen will pardon my invidiousness) to have bought up and stifled all inventions in steam turbines as rapidly as they appeared; the whole industry of the nation would have suffered, and developments in large power plants would have been retarded to an enormous extent. The destructive tendency of such a course would seem to be self-evident; and in the abstract, probably all will agree that the right thing to do is to offer every possible facility to the inventor to develop his ideas. But when it comes to applying these principles to a concrete case, things are not quite so clear, and it seems to me that there is a distinct temptation for the concern having capital to stultify the efforts of the inventor, if these are likely to interfere with its profits.

Such a course, which the author understands has been carried on to an extreme in the United States of America, results in the establishment of huge monopolies, and cannot be altogether in the nation's interests, besides being quite counter to the objects with which the patent system was instituted.

The author understands that the original object of the granting of patents was to encourage the making public of new ways and means of effecting given results, with a view to the general prosperity of the nation as a whole rather than that of the inventor; but at the same time to make it thoroughly worth the inventor's while to trust to a reasonable reward guaranteed by the Government rather than to the risky possibility of his making a fortune out of some process of which he could only reap the benefit by keeping it an absolute secret. Unfortunately, as the law now stands, there is almost as much risk of financial loss in patenting, or making public an invention, as in endeavouring to keep the invention a secret.

One very important reason for the apathy of the capitalist, or the manufacturer, towards the inventor is to be found in the fact that there is a profound distrust in the value of patents, and this is one of the points which it is desired to investigate in the present address.

Another point which it is desired to take up, is the inducement that should be offered by large firms to their employees to stimulate invention.

A third point is the financial assistance which, in the national interest, should be offered by independent concerns to the best class of inventors.

Reasons for Distrust of Patents.

Dealing with the first point, it is perhaps not generally known by the inventor that, even when he has survived the scrutiny of the

British Patent Office, he has only secured a reasonable chance of his invention not having been anticipated at the Patent Office, and he has no guarantee whatever that interested persons may not be able to show that the invention has previously been tried in practice in a semi-public way, even though no application for a patent may have been lodged.

Neither has he any assurance that the invention will act in the manner predicted, for on this point the British Patent Office takes no responsibility.

Neither, and this is the unkindest cut of all, has he any assurance that there is real "subject matter" in his invention; for here again the British Patent Office offers no assurance and takes no responsibility. The result of the latter defect is that, directly he gets the invention started and working successfully, his invention may be combated by interested parties in the Law Courts, and if it cannot be shown to contain "subject matter," it is disallowed.

The knowledge, in a general way, that there are difficulties of this sort to contend with, has no doubt had its influence on the capitalist, and even on the manufacturing firms which would otherwise deal with the matter.

A question, then, for careful consideration is whether the inventor gets sufficient encouragement from the State to compensate him for these uncertainties, and whether these things are not done better in other countries.

Your Chairman has found that in the United States of America they are much more careful in accepting a patent than in this country; but it is in Germany, especially, where the investigation of a patent is carried out with a thoroughness that leaves the inventor, when he emerges successfully through the ordeal, with the confidence that, even if he were attacked in the Law Courts, his patent would almost certainly not be cancelled either on the ground of lack of "subject matter" or on that of inability to work, if made as described in the specification. Another very good feature about the German Patent is that the only fee which the inventor has to pay to obtain this excellent benefit is a matter of some 20 marks, since he does not have to pay the remaining 30 marks unless he is granted the patent, which may be as much as two years later.

The question of the "validity" of a patent, or otherwise, is one of such vital importance to the inventor, and the difference between what constitutes sufficient to obtain (in England) the grant of a patent and what constitutes "subject matter" is so little understood by the average engineer, that I trust I shall be pardoned for dealing with this matter in some detail, in view of the number of younger engineers in our Local Section to whom this matter may be of interest.

"An ounce of practice is worth a pound of theory" is a well-known and correct statement; but people who have the experience do not always care to make it public property, for reasons which may be easily surmised. I must, on this account, content myself with giving an example which is common property, viz. that of the Ilgner case.

Here was a patent which had been granted in this country, and in the pushing of which large sums of money had been incurred, and which, at the instance of a competitor, who had no master-patent to offer in its place, is declared to be invalid.

The probability that this patent was also granted in Germany must not be taken to nullify the statement made earlier that they are more careful in that country than here about the question of "subject matter." There may be many reasons for its having been granted there; not the least of which may have been an influential and capable patent agent. My previous remarks are based on experience under identical conditions in the two countries, and form therefore a sound basis for comparison.

What, then, was the Ilgner patent, and where was the lack of "subject matter"?

The problem before Ilgner was to devise a system suitable for working reversible and rapidly-varying loads from a central station without causing tremendous shocks on the generating system. He employed a motor-generator intermediate between the prime generator and the final motor, which permitted the reversal of the latter and any change of speed required in the same; and he introduced a fly-wheel on the motor-generator to take up the shocks which would otherwise come on to the prime generator. He also arranged for the motor of the motor-generator to tend to slow up very rapidly with an increased load, and thus to draw upon the stored energy of the fly-wheel for the severe demand required from the generator of the motor-generator.

The Law Courts held that the employment of a motor-generator between the prime motor and the final motor was in essence the Ward-Leonard idea; that the essential idea of the Ilgner invention was the putting of a fly-wheel on the motor of the said motor-generator; that it was obvious to any competent engineer that a fly-wheel was necessary, in view of the nature of the load; that the difficulty of using a fly-wheel in connection with reversible loads was got over by combining it with the Ward-Leonard system; and lastly, that no act of invention was performed in arranging that the motor of the motor-generator should slow up with an increased load, as this was an obvious necessity if the fly-wheel was to perform its proper function.

The judgment in this case shows very clearly the erroneousness of the common idea, that any combination of known ideas which effects a new or improved result is subject matter for a patent. In the present instance the combination of the motor-generator idea (introduced primarily for regulating the speed of the second motor) with the fly-wheel (introduced at the right point of the chain to avoid the excessive demands being transmitted back to the generator), was undoubtedly a combination effecting "a new and improved result," and as such would be accepted by the British Patent Office. But, after a company had been formed, and capital spent on developing the scheme, with the supposed certainty of reaping a substantial reward later in royalties, the whole thing is quashed by an action in the Law Courts.

What, then, can be done for the inventor? Undoubtedly the right way is for the British Patent Office to be invested with powers to enable them to determine authoritatively whether an invention possesses "subject matter" before they issue Letters Patent.

And what, then, becomes of the army of patent agents and of lawyers and expert witnesses, whose field, it might at first appear, would be perceptibly restricted? And would the Patent Office be able to pay its way if it undertook all this extra work, unless it raised its fees? My answer to the last question is that, though the German Patent Office does all this extra work, it still pays its way. Last year its expenses were £256,000 and its receipts £534,000. And not only this, but the known value of a German patent induces foreigners of all countries to take out German patents and so bring grist to the mill. The answer to the first question is that the increased volume of home and foreign business would still support the patent agents, lawyers, and expert witnesses, besides greatly conducing to the country's prosperity by stimulating invention and re-establishing confidence in the value of patents.

It seems to your Chairman that the British inventor has for too long neglected to avail himself of the means of combination possessed by other individuals, and some powerful organization is needed in this country to defend his interests, and to state his case in Parliament, etc.

Something, in fact, akin to our own Institution is needed. The patent agents have their Institution, but their interests do not invariably coincide with those of the inventor—I refer perhaps more especially to the inventor who is deficient in capital—and the inventor should certainly have some place where he may rely upon getting disinterested advice, or have his wrongs redressed.

I am advised that such an Institution has recently been formed, but at the moment of preparing this address I am not fully informed as to the extent of which the *bona fide* inventor—and especially the best class of inventor, viz. the engineer-inventor—is represented on the Council. My personal opinion is that it should not contain the professional or legal element, except in the most dilute quantities.

Very severe divergencies of interests have, I understand, manifested themselves in the United States between inventors and patent agents; but this may, however, be peculiar to that country.

The Inventors' Guild of America is, moreover, stated to be "composed exclusively of independent and experienced inventor-patentees."

Before passing from this section of my address, I may perhaps give a hint to the inventor. If he cannot get the British Office routine amended in the desired direction, the German Patent Office is still at his disposal, and the "oracle" can be worked, if done the right way, for a sum not exceeding £3 to £4 if the patentee is his own draughtsman. How this may be done is perhaps not desirable to disclose in an address of this sort; but that it can be done is a matter of experience with the Author.

Inducement to Invention offered by Manufacturers.

My remarks will, of course, be understood to refer principally to electrical manufactures, as my experience lies mostly in that direction.

It is, I believe, fairly common practice for large manufacturing firms to call upon their employees to sign a document, whereby they agree to hand over absolutely to the firm all inventions they may make while in their service, and disclaiming any right whatever to participation in the profits.

The theory of this arbitrary proceeding is that the employee has got his knowledge through his employer. But, though there are occasional cases where the invention has, so to speak, been pitchforked into the employee's hands in the course of his daily work, there is the large majority of cases where the employee either comes to the employer with knowledge gained elsewhere (perhaps at his own expense, as, for instance, at college) and to the benefit of which the employer is not entitled.

Suppose, for example, that an employee has, for some reason, found it desirable to leave a particular firm immediately after he has worked out, with great labour and at considerable self-sacrifice, during his evenings at home, some important invention, the clue to which he may as likely as not have received from reading his technical paper (and there perhaps also have learnt the demand for the invention). All his labour and time has, in such an event, to be irretrievably thrown away; though it is quite conceivable that, apart from the invention itself, his employers have benefited immensely by the improved insight into certain questions (generally intricate) which he has found it necessary to investigate in connection with his patent. But the whole patent, lock, stock, and barrel, belongs to the employer, and the employee has to leave it behind him.

I had a case happen within my own knowledge, where an exceptionally brilliant engineer was positively dismissed because he refused to hand over to his employers a patent which he had taken out, and the knowledge for which he had certainly obtained before he came to the said employers.

No doubt there are many simple ideas which are the result of a happy inspiration in the course of one's daily work; but the day is gone by when anything of real value can be evolved in electrical engineering without an immense amount of brain work; and, if there is not sufficient stimulus of reward, the busy engineer can hardly be expected to spend his evenings at such problems when he can get along just as well with his employers if he does not do anything outside of his office. No doubt the difficulty is to make a proposal acceptable to the employer, and at the same time sufficiently enticing to the employee to give him every stimulus to develop his inventive powers.

There are certain improvements which might be made in present relations between employer and employee, and which are fairly obvious.

First, it should be illegal (subject, of course, to proper provision being made for the protection of the employer) for an employer to compel an employee to hand over to him his invention, on pain either of dismissal or of prejudice in any way to his advancement if he stays on with the firm employing him.

The question as to whether the employee got the idea from his employer, or whether he obtained the knowledge necessary for him to make the invention while in his employer's service, or whether his daily work was such as necessarily to provide him with the materials for the invention—all these should be left to an independent arbitrator (say, for instance, the President of the Institution of Electrical Engineers), before it is even conceded that the employer has any right to the first refusal of the employee's invention.

The employee should certainly be allowed to apply direct to the Patent Office in the first instance, if he chooses to do so. The fact that the patent fees are, under present arrangements, borne by the employer, counts for very little in the matter. In most cases, the inventor would greatly prefer to pay his own.

Financial Assistance for Suitable Inventions.

Undoubtedly the engineer with a genuine bent for invention needs every encouragement. By this I do not include the casual inventor of a new button-hook, or a boot-jack, or a trousers-stretcher, who is quite sufficiently looked after by the present patent system, but I mean the engineer who, having a good training in fundamental principles, sets out with a full knowledge of what has already been accomplished in a particular direction and has a clear idea of what he wants to arrive at, and is prepared to make sacrifices in its attainment.

Mr. Dugald Clerk said recently, in a paper read before the British Association on the subject of the gas turbine: "Most of the early experimental work upon any great and difficult problem must necessarily be of the forlorn-hope type, so far as concerns the attainment of commercial success. This is shown by the history of all great inventions. Pioneers in work of this kind should be highly honoured by engineers."

I most fully endorse this; but I think we might go a step further and say that they should also be honoured by the nation. It is a matter that concerns the nation at large just as much as it concerns the engineer. However, it is not so much the "honouring" as it is the "encouraging" which I am contending for.

Could not our Institution do something in this sense? All our premiums appear to be granted on the merits of papers read before the Institution, judged on the basis of their contribution to the general fund of knowledge, or their promotion of a good discussion. But if we had a substantial premium set aside for the inventor who may read a paper descriptive of his work (even if the object of that work be not finally attained), such premium might help him to continue his experiments or developments. And, if contributions towards such

a fund were invited on the part of members generally, would there not probably be a large response from members, and thus an additional stimulus given to original work?

Comments on Patent Procedure.

A few comments, and some hints on patent procedure, may not here be out of place.

The principal adverse comment which I have to make on the patent procedure system is that the period of six months allowed between the lodging of the provisional and that of the "complete" specification is altogether too short for inventions that involve a great deal of investigation, calculation, experiment, and development.

The period is, no doubt, ample for simple inventions; and the difficulty is, I suppose, that to enlarge the present period is apt to impose a hardship on those who want to obtain Letters Patent as early as possible. But there are sundry provisions in the Patents Act which help one even here. I would suggest that it is very hard on that class of inventors who are the most likely materially to benefit their country that they should be handicapped by having either to drop their original work (and thus lose priority) or to disclose to the public an invention at such a stage that it invites development by others who have not done the really hard work on it.

Really great inventions do not come without a great deal of work having been previously performed, nor by other than an individual with a large amount of knowledge and general capacity; and such an individual is a valuable man quite apart from his inventions, and sure to be in consequence loaded up to the full in his daily work. To such a man the spare time which he could give to the invention in six months, is ridiculously small.

As a partial remedy, I would suggest that he should be allowed, under increasing penalty, to extend the time to nine months, or even a year. The present seven months' absolute limit is not fair to such a man.

And here I may give a hint. It is not, perhaps, general knowledge that the British Office gives, before the lodging of the "complete" specification, something in some measure corresponding with the "patents of addition" which it allows after the lodging of that specification.

I refer to the embodiment of two or more "provisional" specifications in one "complete" specification, under the rule relating to "cognate" applications. Suppose, for example, that during the six months succeeding the lodging of the "provisional" one finds several directions in which difficulties have to be overcome, requiring supplementary inventions, and lodges these ideas in the form of "provisionals," then when the six months is completed all these can, with the consent of the Patent Office, be embodied in the one complete application. Thus in any question of priority, one would be allowed the date of the

lodgment of any one of the provisionals as the date of that particular stage of the invention.

This is a feature that may be extremely valuable, and in my own knowledge I have come across at least one case in which, while the inventor was anticipated by others in the first early stages of the invention, his succeeding stages of the invention transformed an uncommercial idea into one that was commercially feasible.

The above rule, then, provides a way in which the inventor can sometimes actually gain a month or two. It has been already remarked that most inventions of real progress come to one in stages. Now, it often happens that the later stages turn out to be of greater importance than the earlier stages, and in some cases, as just indicated, it may be found out that the earlier stages are either of less importance, or perhaps have been anticipated. Suppose, then, that an inventor finds the date for lodging the "final" (or "complete") specification upon him while he is in the throes of developing a new and important stage of the invention, he has the option of two things. He may either lodge his "complete" specification in an avowedly incomplete form and take out a second "complete" specification immediately afterwards, which is a distinctly costly process if he is going to lodge foreign applications as well; or he may elect to "scrap" one or two of his earlier provisionals and thereby gain two or three months, or even more, before he need lodge his "final," and thus obtain the desired time in which to develop his latest stage of the invention. This method has the additional great advantage that the date of the lodging of the first foreign application is also postponed.

With regard to other comments on the British system of patents, I have already dealt rather fully with the failure of the British patent system to protect the inventor from his patent being nullified through alleged lack of novelty. It is only fair, however, to the Patent Office officials to state that they do their work, as far as authorized by the law, thoroughly and efficiently, and are equally courteous to all with whom they have to do.

A suggestion may here be made as to whether the British Patent Law should not differentiate between "inventions" in which there is held to be "subject matter" and those in which there is not.

There is a large class of very useful inventions in which two or more independent ideas are combined to produce a new and improved result. Surely such an invention deserves better of the public than to be put upon the scrap heap after a lot of time and money has been spent upon it? Take for instance, the Ilgner case; it is very easy for a Judge to be persuaded by expert witnesses that the thing was self-obvious, but the ordinary engineer may be allowed to have his doubts about this.

In any case the fact remains that the crushing of a patent in this way is a distinct discouragement to invention, and that the best class of inventor, viz. the well-informed, plodding engineer, who has a definite purpose and the best equipment for effecting it, is the one who is the most hard hit by it. I would suggest that complete protection be

granted to this class of patent, but only, however, for a limited term of years—say, 10 years instead of 14 years. This would mitigate the discouragement which is otherwise put upon the best class of inventor.

The point upon which I desire to insist, is that anything that discourages inventions which would confer a boon upon the country producing them (though they need not be in themselves of striking novelty) is actually a blow aimed at the trade of the country itself, and should therefore be remedied with the least possible delay.

Conclusions.

The following conclusions may be summarized :—

1. Reform is needed as to the investigation of patent application ; and the rigorous methods adopted by the German Office might be copied with advantage (with improvements in the facilities for appeal).
2. Inventions requiring a great deal of investigation work should be allowed much more than six months for this development.
3. The large manufacturing firms should offer greater inducements to their employees to work out inventions than they do at present.
4. Inventors should combine to protect their own interests, and exclusively their own interests, and have a mutual defence fund.
5. Some of the scientific and engineering Institutions might give more encouragement and assistance to the independent inventor than they do at present.
6. It might be considered desirable, in the best interests of the country, to have two classes of patents—one for inventions possessing "subject matter," and another for those not possessing it, but yet possessing novelty.

In conclusion, I crave your pardon for errors of omission and commission, and trust that this address may at least be of some help to our younger engineers, even if it contains nothing of information or novelty to the older members.

WESTERN LOCAL SECTION.

INAUGURAL ADDRESS OF THE CHAIRMAN,

W. A. CHAMEN, Member.

(*Address delivered* 18*th November*, 1912.)

The formation of a new Local Section of the Institution of Electrical Engineers, of which you have done me the honour to make me your first Chairman—an honour which I very highly appreciate and for which I again thank you—seems to offer an opportunity for taking a brief survey of the present position in the West of England and in South Wales from an electrical point of view.

Roughly speaking, the area for which this Western Section is responsible comprises the counties of Gloucester, Hereford, Monmouth, Radnor, Brecon, Glamorgan, Cardigan, Carmarthen, Pembroke, Wiltshire, Dorset, Somerset, Devon, Cornwall, and possibly part of Hampshire. There would seem to be no doubt that as time goes on this large area will require more than one Local Section, but the present state of electrical development is not sufficient to warrant more being started. Of course, a large proportion of the area is at present, electrically speaking, desert land; but electricity is now very largely used in the counties of Glamorgan, Monmouth and Carmarthen, and in parts of the counties of Gloucester, Somerset, Devon, and Cornwall.

There is no comprehensive and complete record of the amount of electrical power installed within this Western district, and any estimate which can be made must of necessity be very rough. It would, however, appear that the total horse-power of motors, or devices of any kind for consuming power (including heating and lighting), cannot be less than 260,000. Included in this figure are the electrical supplies given by the municipalities of Cardiff, Newport, Swansea, Pontypridd and other towns in South Wales; also the supplies given by the municipalities of Bristol, Exeter, Plymouth, Devonport, Cheltenham, Gloucester, Swindon, Salisbury, Southampton, Bournemouth and other towns in the West of England. The figure also includes the supply given by the South Wales Electrical Power Distribution Company, the Merthyr Electric Traction and Lighting Company, the Cornwall Electric Power Company, and other companies; also the many installations, some

of them of considerable size, laid down by colliery companies and by copper and other metallurgical works in South Wales and in the mines of Cornwall.

The various uses of electricity at the present time may be grouped under the following heads: (1) Telegraphs and telephones; (2) electric lighting and illumination; (3) electric ventilating, heating and cooking for domestic and such like purposes; (4) the working of tramways; (5) the driving of machinery in factories of all kinds; (6) the driving of machinery in collieries, including the winding of coal up the shafts, the ventilating of the mines, the haulage of coal both underground and upon the surface, the driving of pumps for unwatering the mines, and in some cases the driving of coal-cutters for actually getting the coal; (7) the driving of pumps, haulage gears, winding gears, and other machinery used in the metallurgical mines, principally in Cornwall; (8) the driving of cranes, travellers, pumps and other machinery in the various docks and in the Government dockyards of Devonport and Pembroke, and at Portland.

In spite of the fact that there is at least a total of 260,000 h.p. of electric motors and other electricity consuming devices of all kinds coming under the heads above mentioned, it is somewhat surprising to find that the membership within the Western district appears to be as follows:—

Honorary Members, 1; Members, 39; Associate Members, 117; Associates, 27; Students, 29; making a total of only 213. Surely this number is very much smaller than it should be. There must be considerably more electrical engineers and other persons interested in electrical matters who are eligible for admission to the Institution of Electrical Engineers under one or other of the various grades of membership. In this connection, the new class of graduates should induce large numbers of those who are engaged in the electrical industry, but whose qualifications meantime do not reach the standard required for associate membership, to join the Institution and to obtain the benefits which must accrue to them by becoming units in such a very large and important Institution as ours has already become.

It is probable that the above list of the uses of electricity does not embrace all the different purposes for which electricity is already used; but whether it does so or not there are still to come the adaptation of electricity for wireless telephony, and for railless traction (which is receiving active attention in some parts of the district), also the electrification of railways, and there have been people bold enough to say that in due time electric power will be transmitted without wires. Last, but perhaps not least, the use of electricity in agriculture and horticulture is scarcely even so much as thought of at all in our district.

To discuss the methods and the details of the application of electricity to those purposes to which it has already been applied in the Western district would be beyond the scope of this address; but it may not be out of place to spend a few moments in considering the

possibilities of development in the other directions in which nothing has so far been done in the West.

Railless Traction.

No one who travels about the towns and villages of South Wales in which tramways already exist, either in motor cars or in other vehicles carried on wheels running on the roads, will view with anything but delight the possibility of being able to do away with tramway rails in our roads and streets. To the tramway engineer, however, the prospect of having to deal with heavy traffic without the aid of rails is rather serious. For, while it is quite true that the very heavy cost of laying and maintaining the rails will be saved, the power required for driving the cars will, on the other hand, be considerably greater on account of the roughness and unevenness of the roads, with consequent increase in road friction. It is apparent that, for the satisfactory operation of railless traction, our roads will require to be kept in better condition than is often made to suffice for horse haulage.

The existence of the trolley wire and its overhead accessories, and of the poles along the centre or the sides of the road, will be considered quite unobjectionable by all who realize that electric traction in one form or another is a necessity at the present time. But there is still a possibility of railless traction without these accessories at all. The secondary battery, which has always had its limitations of distance for such purposes on account of its serious weight, may yet be found a very useful friend in districts where electricity supply is continuously available, at least at one end of each route in question. The facility for recharging batteries, combined with regenerative control and the very high economy in the use of power possible with some of the very ingeniously designed motor-generator apparatus, appear to open up possibilities well worth the serious consideration of those who are contemplating the introduction of railless traction. Motor omnibuses have now been produced to carry thirty-six people and capable of running from 45 to 50 miles without recharging, the accumulators, which weigh about 1 ton, being easily lifted off at the journey's end and replaced by freshly charged ones. The cost of maintenance of the accumulators—one of the serious drawbacks hitherto—seems, owing to improvements in the conditions of use as well as in the construction of the accumulators themselves, to have been reduced to quite a moderate figure. The extreme simplicity of the whole equipment and the perfect and easy control of the vehicle, surpassing in these respects anything that has been accomplished with petrol or steam engines, make this method of traction most fascinating.

Electrification of Railways.

It is sometimes thought that the introduction of electric tramways or of electric traction or other forms of mechanical traction along roads must be a serious competitor to already existing railways.

It may be that in some cases this is to some extent so; but it seems probable that in time the establishment of road traction routes will be of such great advantage to railways, by acting as feeders to bring traffic to them, as considerably to outweigh the loss of some little amount of passenger traffic in cases where the road traction routes lie more or less parallel to the railway. Perhaps the fact that the creation of facilities for travelling calls into existence passengers who would otherwise never travel at all is not fully realized. The advantages of electric propulsion on railways are the absence of smoke and dirt and of hot ashes flying through the air when mounting steep gradients or hauling heavy loads, and the rapid acceleration which cannot well be managed with the steam locomotive and which results in considerable saving of time, particularly on journeys where stops are frequent. The rapid acceleration is, of course, due to the fact that the power available for starting the train is not limited, as in the case of the steam locomotive, to the actual power of the prime-mover; which latter in the case of electric traction forms part of the train itself. The electric motor, which is supplied with power from an external generating station (this can be of any size required), is capable of standing very heavy overloads for short periods of time, so that this system of traction for railways possesses a very valuable advantage as compared with any method in which the whole power required has to be generated by a prime-mover forming part of the train itself. It is also possible to make a considerable saving in the total weight of the train by attaching electric motors to the underframes of one or more of the coaches, so that there is no additional weight on the train except that of the motor and the electrical equipment—a small matter compared with the weight of the steam boiler and engine with their accessories. Another advantage which may be expected, is reduced cost of working. The question as to which is the best method of electric traction is still being warmly discussed by those who have been occupied in the electrification of railways. Perhaps, at the present time, there are three methods in serious competition: (*a*) continuous current at pressures of 500 to 600 volts, the current being supplied through an additional rail carried on insulators, and picked up therefrom by collecting shoes attached to the underframes of the vehicles; (*b*) single-phase alternating current, generally at a periodicity of 25 cycles per second, conveyed by overhead trolley wires at pressures up to about 3,000 volts and collected therefrom by suitable collectors attached to the tops of the vehicles; *c*) high-tension continuous current at a pressure of 1,000 volts or more, conveyed to the train by an overhead trolley wire in the same manner as the last-mentioned arrangement. In all three cases the current returns through the rails.

It is difficult to say which of these arrangements would be the best to adopt on the railways in our Western district. It is possible that no single one of these systems would prove the best for all cases, but that in some circumstances one might be found preferable, whilst under other conditions another might prove more suitable.

ELECTRO-CULTURE.

This would be a somewhat novel departure. It does not simply mean that electricity would be used for driving the various kinds of agricultural machinery which are at present driven by steam or oil engines—though that will probably be done in cases where electricity supply is distributed about the country and available at a low cost—but it includes the actual stimulating of the growth of crops and plants by electrically charging an overhead network of wires erected above the surface of the fields or gardens that it is desired to influence. Considerable experiments have already been carried out in this direction by Professor Lemström, and the subject is still being pursued by Mr. J. E. Newman, of Pershore, and others. Briefly, the method adopted is to produce a positive charge of electricity on the overhead network of wires, which are at a height of about 15 ft. above the ground and at a pressure of from 40,000 to 75,000 volts above earth potential, so as to cause a continuous state of electric tension and, to some extent, an actual discharge of electricity from the overhead wires to the ground. An essential part of the apparatus used for this purpose is Sir Oliver Lodge's electric valve, by means of which alternating current from a high-voltage induction coil is rectified into direct current, or at any rate into current passing in one direction only.

The results which have been obtained by means of electric stimulation in this manner have in some cases been very remarkable; but there is great difficulty in determining the exact amount of the increase attributable solely to this cause because of variations in the amount of sunshine and rain and also in temperature during different seasons, as well as owing to variation in the fertilizing properties of the manures used. These factors in themselves vary so greatly that there is, as we all know, very great variation in the crops produced in the same districts from year to year due to these causes alone. After allowing for these and other varying factors, by taking averages of results over a sufficient period of time and also averages of results obtained under varying conditions on different parts of a farm during the same time, it does appear, however, that the effect of electrical stimulation has been to produce remarkable increases. It is not likely that any material advance will be made in this direction until electricity supply is distributed in a more general way throughout the country districts; but with the progress now being steadily made by electrical power distribution companies within parts of our Western district, it would seem quite possible that considerable developments in this direction may be made during the next few years.

The importance of this departure cannot well be over-estimated. It is of course a matter of common knowledge that our country does not produce anything like sufficient in the way of food-stuffs for the maintenance of its population; and while it is quite possible that the ultimate remedy for this somewhat unsound state of things is to be found by the emigration of further very large numbers of our people to

our Colonies or to other parts of the world, it does seem possible that the more or less artificial condition of things which now exists can be remedied to some extent by other artificial means. It has been said recently by one of our highly respected professors that if coal should fail it would still be possible to produce enough alcohol from vegetable sources for use as fuel for all the purposes for which coal is now consumed. There is, therefore, perhaps no need for us to worry about the welfare of those who will succeed us in the next few generations even if our coal supplies should be worked out.

In an address of this kind it is perhaps allowable for your Chairman to take a departure into any subject with which he may be especially acquainted. It may therefore be permissible to devote a few moments to some points of general interest in the business of a power supply company. In view of what has already been said, it will perhaps be hardly necessary to make an apology for, or explanation of, the reason for the existence of power supply undertakings. Electricity is now being supplied for such a large number of different purposes, involving the use sometimes of large and sometimes of medium or even small quantities, that the advantage to the community of having an ample and largely distributed supply to draw from does not need to be dwelt upon.

In passing, one rather interesting development may be mentioned, which ought to increase considerably in the future, namely, the use of electricity from a general power supply for the purpose of working isolated sewage pumping plants. There is already one such case within some nine or ten miles of Cardiff, where the pumping is done by electric motors started and stopped automatically according to the level of the sewage in a storage sewer constructed beneath the road. The attendance upon the plant is reduced to a minimum, and the arrangement works with satisfaction and economy. There must be many cases where similar arrangements could be adopted, and it should also be possible to give considerable assistance towards the solution of the difficult question of providing small water supplies in scattered parts of mountainous districts by means of electrically driven pumps started and stopped automatically according to the actual requirements of each case.

Of course, these and many other similar applications of electricity require only very small quantities, and are absolutely impracticable except in connection with a distribution system already in existence for giving large supplies, such as those required by the collieries and factories which are spread about the country; but if once the supply can be made available on a sound financial basis all those other small supplies can be given and become a very useful addition to the revenue of a power company, as well as being of great benefit to the consumers.

It is somewhat remarkable that in spite of the many advantages of having a general supply of electricity available, difficulties are so often placed in the way of development of these undertakings by persons and bodies who might be expected in their own interests to assist

power supply companies in every possible way. Landlords—though a good many of them are extremely reasonable and quite willing to grant facilities for the erection of overhead lines across their property—sometimes refuse permission point-blank, or else seek to impose such onerous conditions as to be prohibitive. There is no compulsory statutory power granted to electrical supply companies in this kingdom for the erection of overhead lines across private property as there is in Italy and in Switzerland, where the supply companies have the right to go across any property within reason, paying compensation for any injury that the landlord may really suffer thereby. There is a court appointed for hearing appeals and for deciding the amount of compensation to be paid in case of failure to agree. There appears to be no trouble about the working of such powers in those countries, and one cannot see how any difficulty would arise in this country, or how there could be any hardship upon the owners of property by the creation of similar powers here.

Local authorities sometimes raise unnecessary difficulties, and in many instances have prevented the introduction of electricity supply within their districts, to the considerable disadvantage of the inhabitants, for no sound reason at all. There exists in the minds of some district councillors a strong conviction that all electricity supply undertakings should be in the hands of the local authorities, and not in the hands of private or public companies at all. This, of course, is a question of general policy and the honest convictions of those who hold such opinions must be respected, but there can be no doubt whatever that the electrical supply industry has been very seriously handicapped in the past from this cause.

It is much to be deplored that there has been, and still is in some cases, such a want of trust in the good faith and proper sense of obligation of those who direct and control the operations of companies. Possibly there have been cases in times past where companies have not acted with that fairness which is reasonably to be expected from those who undertake the serious responsibility of electricity supply; but the conditions now laid down by Parliament in the various Electric Lighting Acts, and by the Board of Trade under the provisions of those Acts, safeguard the public in every possible way.

It is remarkable that the Electric Lighting Clauses Act, 1899, requires that the consent of the local authority shall be obtained before the erection of overhead electric lines can be allowed by any electrical undertakers, and that it provides no remedy whatever in the way of appeal to the Board of Trade, or otherwise, against the obstruction which must result from the abuse of this provision. This point may at any time become a serious one, and the Act should be amended. It would seem that the Parliamentary Committees who dealt with the Bill omitted, by oversight, the provision of a remedy against unfair use of this power of veto, although such provisions are made in other directions by the same Act.

In several cases, however, it is gratifying to find that local

authorities and power supply companies have not only "buried the hatchet," but have entered into agreements for supplies in bulk, or have transferred Provisional Orders upon terms which must be considered reasonable from the points of view of both parties.

It may perhaps be interesting to know that in one case in this district the Urban District Council are putting down a refuse destructor with electric generating plant upon the same premises, for the purpose of converting all the energy available from the destruction of the refuse (which in some of these districts in South Wales is of an unusually high calorific value) into electricity. This is to be delivered into the mains of the power company at all or any hours of the day or night, and paid for by the power company at an agreed price. On the other hand, the District Council have obtained a Provisional Order and have entered into an agreement with the power company for bulk supplies at various parts of the district in question throughout which the power company's transmission mains already run. The Council will themselves work their Provisional Order, putting in transforming apparatus at various points and the necessary distribution system connected therewith at their own cost. In this way the Council find a market for the continuous delivery of their waste power during both day and night, and at the same time obtain the benefit of bulk supplies, for the purposes of their own Provisional Order, at a very moderate price and without the necessity of resorting to the use of accumulators, as might have proved necessary had they decided to generate power for themselves. They also save considerably in the cost of transmission.

In another case the District Council have agreed to the power company obtaining a Provisional Order for the purpose of carrying out the supply throughout their district. The Council have also made an agreement with the power company for the lighting of the principal roads and streets in the district. As this is a case where no gas company is in existence, the benefit to the residents is obvious. The company may be trusted not to be so foolish as to try to take advantage of the fact that there is no competition, in order to obtain higher prices than they would do under other circumstances. Such a policy would naturally cause dissatisfaction and in the end only damage the power company.

It is to be hoped that these beginnings of better understandings between supply companies and district councils will spread farther as time goes on.

Although the total horse-power connected to the South Wales Electrical Power Company's mains is about 20,000, several of the large colliery companies in the district have put in generating plant of their own, and in some recent cases where the power company's mains have not yet been extended the periodicity adopted by such collieries has been chosen to suit the periodicity of the supply given by the power company, with the object of facilitating the joining up of the various power stations in the district as time goes on. It is possible that the passing of another year or two may see the generating plants of some of these colliery

companies linked up with the general system of transmission of the power company in such a way that any surplus power available from one colliery at certain times of the day may be utilized at other collieries, and also so that at times of heavy load the power company's generating plant may assist the colliery company's generating plant by giving whatever extra power may be required. It may be a somewhat delicate matter to collect into one harmonious whole these various interests, but the thing is said to have already been done in parts of Germany, where the advantage to be reaped by all pulling together has been realized. The various colliery and factory owners have their own representatives upon the board of the power company, and even municipal undertakings have in some cases joined in with the general scheme, having their own deputed members representing them on the board of the power company.

The consideration of even these few points of general interest in connection with power supply undertakings may suffice to show that there is work, and a considerable amount of work too, for an Institution like ours to take in hand; this work may be looked upon as purely commercial and entirely untechnical, but it is, nevertheless, of the very highest importance to progress in the use of electricity.

THE MAGNETISM OF PERMANENT MAGNETS.

By PROFESSOR SILVANUS P. THOMPSON, D.Sc., F.R.S., Past-President.

[*Lecture delivered at the Glasgow Meeting of the Institution on 13th June, 1912.*]

SUMMARY OF CONTENTS.

FOR CONSTANCY AND POWER OF PERMANENT MAGNETS it is necessary to consider the following points:—

1. QUALITY OF STEEL: Carbon Steels; Alloy Steels; their percentage composition.
2. SHAPE AND DIMENSIONS: Bars, Short and Long; Shapes forming Nearly Closed Curves.
3. HEAT TREATMENT: Normalizing; Quenching; Temperature of Quenching; Rapidity of Quenching; Tempering.
4. MAGNETIZING: Methods; Appropriate Intensity of Field.
5. MATURING: By Lapse of Time; by Mechanical Shock; by Reheating; by Partially Demagnetizing.
6. CONSERVING: Safeguards for Conservation.

INTRODUCTORY.

It is impossible, at any rate for me, to come back to the University of Glasgow without referring to the late Lord Kelvin, from whom we have learned so much in this subject of Magnetism. Alas! he is no longer here to represent science in the University.

The magnetism of permanent magnets is a subject which interests all electrical engineers; but I doubt whether the majority of them recognize the real importance of it. In many diverse instruments everything depends on the constancy of permanent magnetism: and that constancy, now habitually attained to a remarkable degree, has not been reached without labour and long effort. We have all been familiar from our boyhood with magnets, but I doubt whether any of us has realized the number of problems which in the developments of the last twenty years have been confronted, investigated, and solved. But I have ventured to think that if one might collect the scattered data that have been gleaned in the researches of many workers during those years, and put them, as they have not hitherto been put, into a collected whole, the result would be worth recording, even though, as in the present case, the individual who has had the labour of putting the data together has practically nothing to claim in the way of originality upon his own part.

I have to speak, then, not of my own researches, but of things that have been done by other investigators in this and other lands.

Now in the first place, it goes without saying that we consider the materials which are proper for the making of permanent magnets, and, primarily, of *steel*. For all ordinary purposes there are no permanent magnets except those made of steel. I do not, of course, forget that there is also the material lodestone; nor am I oblivious of the circumstance that Professor Pierre Weiss has lately discovered a new magnetic material, an alloy of cobalt and iron, which appears to possess the property of acquiring a larger degree of magnetism than any other substance, and of retaining it to a remarkable extent. But it must suffice here to note that there is such a material, since its properties require further investigation. We have therefore to deal only with steel. Let me enumerate the heads under which we shall have to consider steel. First there are the questions of the composition and constitution of the steel. Ought we to use carbon steel or an alloy steel? If carbon steel, what percentage of carbon should we adopt? Remember that our word "steel" does duty as the name of materials which differ enormously in their mechanical and chemical properties. Formerly, when people were beginning to distinguish between iron and steel, they used to reserve the name "steel" for those kinds of iron which could be hardened by quenching, so as to take an edge when ground for service as weapons or tools, and which when duly tempered showed the valuable property of elasticity, making them useful for springs. All other kinds they called "iron," whether of the soft ductile and malleable sort containing practically no carbon, known as "wrought iron," or whether of the harder and more brittle kind containing more than 2 per cent of carbon, known as "cast iron." But in the nineteenth century the great metallurgists discovered new ways of making a nearly pure iron, capable of being cast, often containing less than a tenth of 1 per cent of carbon, nearly as soft, as ductile, and as malleable as wrought iron. Yet simply because a material called "steel" fetches a higher price in the market than a material called "iron," they gave to this remarkably pure iron the name of steel—"mild steel." Without falling into the verbal pitfall thus provided, let us say simply that for the purpose of making magnets the steel that we must use is one that is really *steel*, in the old sense. That is to say, it will be no good for making magnets unless it is capable of being hardened by quenching. A soft steel is useless for this purpose. But the hardenable steels comprise two groups of material; the high-carbon steels, containing from 0·3 to 1·5 per cent of carbon, and the alloy-steels containing, along with a certain percentage of carbon, a notable quantity of one of the metals tungsten, molybdenum, chromium, or vanadium. To one or other of these classes magnet-steels belong; and it will be necessary to consider them in detail.

A second consideration, and one which will to some extent affect our choice of material, is the question of the shape and dimensions of the magnet that is to be made. It will be made abundantly clear that

the magnetic properties of short bar-magnets, and of magnets of any short shapes, such as cubes or spheres, are very different from those which have the form of long bars, or which are bent round in circles or horse-shoes so as to form nearly closed shapes. And it does not follow that the material which is suitable for a long bar magnet or a horse-shoe will be suitable for short magnets such as are occasionally wanted.

Then there is a third consideration of a metallurgical nature, namely, the heat-treatment to which the material has been subjected at the steel works. Steel of a given composition may differ widely in its magnetic properties according to the furnace processes to which it has been subjected. Before it receives its shape it has been subjected to various heatings and coolings, as well as to mechanical processes of hammering, or drawing, or forging. Its homogeneity of structure depends on these : but it also depends on the heat-treatment that the magnet receives after having been brought to its final shape. If we are to know how to make magnets which are as powerful and as constant as possible, we must know to what temperature they should be heated before we quench them, and how long they should be so heated. We ought to know at what temperature they should be quenched to harden them, and how rapidly they should be quenched. Further, we ought to know whether after this hardening there is any advantage or disadvantage in reheating them to temper them.

A fourth consideration is the process of magnetizing them : in what magnetic field, or how? Happily this is the least troublesome of all the processes, because if you have a sufficiently powerful electromagnet to impart the magnetism, the precise mode of applying it is an unimportant issue. The magnetism which a magnet retains permanently is only a fraction, it is true, of the magnetism temporarily imparted during magnetization : but if the temporary magnetization has already been pushed to a high degree, there is little or no permanent advantage to be gained by pushing it further to abnormal values.

Fifthly, after a magnet has been magnetized, and if it has to be brought into such a state that its permanent magnetism is really constant, it must be subjected to a further process of maturing.*

Lastly, there is the question of conserving the magnetism of a magnet. Let me dispose of this, once for all. The only way to conserve the constancy of a magnet which has been prepared through all the series of processes is to see that it is never subjected either to severe heating or cooling, and never allowed to touch against any

* This process of "maturing" must be carefully distinguished from the effect, sometimes called "ageing," which occurs in transformer iron. Ageing is the term properly applied to the phenomenon which sometimes occurs in a transformer, when the sheets of iron used for the core show impaired permeability and increased hysteresis after use for a few months. The maturing of magnets may occur naturally during the first few months or years after their magnetization, as they settle down to constancy. This process of settling down may be hastened in several ways: by mechanical shock, by repeated steaming or warming, or by partial demagnetization. The term "maturing" appropriately includes all varieties of the process. Some of the operations of maturing the steel may be effected even before magnetization.

other magnet or piece of iron. The supposed safeguard of putting on a keeper is about the worst device ever suggested. Every time the keeper is suddenly put on the magnet is weakened. Every time it is suddenly pulled off the magnet is strengthened.

Magnetic Qualities of Steel.

I resume the consideration of the separate questions which have now been stated: and foremost that of the quality of the steel. Here we have the magnificent series of researches associated with the names of Sir Alfred Ewing, the late-lamented Dr. John Hopkinson, the brothers Professors Andrew and Thomas Gray; and in more recent times those of Messrs. Barrett, Brown, and Hadfield, those of Sir Robert A. Hadfield on the alloy steels; and those of Messrs. James G. Gray and Alexander D. Ross, to name no others.

The researches of Ewing and of Hopkinson are so well known that a very brief reference to the salient points which their work brought out must suffice. When any specimen of steel is subjected to magnetizing forces it acquires a temporary magnetization in excess of that which it retains when the magnetizing force is removed; and, of the magnetism which remains, a part only is retained permanently, a considerable part being readily removed by any demagnetizing forces to which it may be subjected. It is a commonplace to say that the softer the specimen the less is the fraction of magnetism which it retains permanently, and the harder the specimen the greater is that fraction. But the amount which remains when the magnetizing forces are removed is, in different specimens, held in the steel with different degrees of fixidity. Following Hopkinson, we take as a measure of the tightness with which the magnetism of a specimen is retained the amount of demagnetizing magnetic force which must be applied in order to reduce the magnetization to zero. And the "coercive force," or fixidity, is defined as equal (and opposite) to that amount of demagnetizing force.

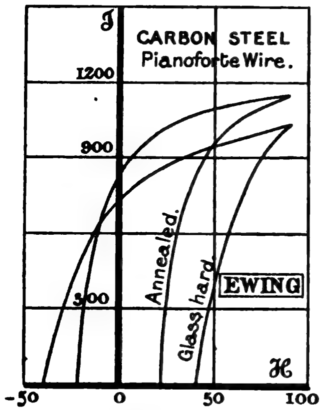

Fig. 1.—Hysteresis Loops (Ewing).

Let us illustrate the matter by a diagram (Fig. 1), taken from Ewing, relating to a carbon steel wire which was magnetically tested in an annealed state, and again tested after having been made glass-hard by quenching when nearly at a white heat. Comparison of the two hysteresis loops shows that the soft steel, when subjected to a magnetizing force of about $\mathbf{H} = 100$, acquired a magnetic flux-density having a value of $\mathbf{B}_{\text{max.}} = 14500$; the value of the intrinsic magnetiza-

tion being $I_{max.} = 1145$. When the magnetizing force was reduced to zero the residual value of **B** fell* to about 10400, or $I_{rem.} = 827$; and it required a demagnetizing force of about $H = -22$ to reduce the residual magnetism to zero. In the hard state an application of the same magnetizing force produced a flux-density of about $B = 12600$ only, which fell to about 9600 on the removal of the magnetizing force. But the demagnetizing force required to reduce the flux-density to zero was now $H = -40$. Stated otherwise, the hard steel showed actually a lower value of the remanence $I_{rem} = 764$, as against 827 for the soft steel; but this lesser amount of residual magnetism was held more tightly, since the coercive force for the hard steel was 40 as against 22 for the soft steel.

It will be convenient here to present a Table in which are collected together a number of similar data for various specimens that will be referred to in the course of this lecture.

The hysteresis loop provides us then with the means of defining the two most important facts that we ought to know about any steel that is to be used for making permanent magnets—the value of the remanent magnetization and the value of the coercive force: the height of the point where the curve crosses the vertical axis, and the breadth of the loop on either side of the zero-point. And of these two things by far the most important is the latter—the coercive force. We shall consider any steel to be unsuitable for modern requirements if it does not have the value of I_{rem} at least as high as 800, or if the value of H_c does not exceed at least 60.

Our next question is how the carbon content of a steel affects these two properties. It is well known that low-carbon steels do not harden appreciably on being quenched; and, broadly speaking, we may say that a carbon steel containing less than a quarter of 1 per cent carbon does not harden. Neither is such a steel of the slightest use for a permanent magnet. Carbon steels containing from 0·75 to 1 per cent of carbon, known as die-steels, sett-steels, and chisel-steels, are suitable for such articles as stamping dies, hammers, miners' drills, chisels, shear-blades; those containing from 1 to 1·15 per cent for drills, milling cutters, screwing dies, which require a greater hardness; those containing 1·25 per cent for turning tools, fine drills, small milling cutters, which must be capable of still further hardness; 1·5 per cent for razors, fine turning tools, and files, which have to be extremely hard. The hardness which they can acquire on quenching increases with the carbon content.

The hardening produced by carbon is, in fact, up to a content of

* In considering other hysteresis loops I shall, for brevity, denote the remanent value of **B** by the symbol $B_{rem.}$ and the value of **H** which measures the coercive force as H_c. For the purpose of this lecture it will be convenient to plot the intrinsic magnetization **I**, rather than of the flux-density **B**; the residual magnetization, or "remanence," being then denoted as $I_{rem.}$ Since $I = (B - H) \div 4\pi$, and **H** is almost always small compared with **B**, we may consider approximately $I = B \div 4\pi$. As the values of B_{rem} run over a range of from about 6000 to 11500, those of I_{rem} will run from about 500 to about 900 in such steels as come into our consideration.

DATA OF MATERIALS MAGNETIZED WITH A MAGNETIZING FORCE OF $H = 100$, OR UPWARDS.

Material.	Percentage Carbon.	Percentage Tungsten.	State of Hardness.	B_{max}.	B_{rem}.	I_{rem}.	H_c.	Authority.
Swedish wrought iron ...	trace	—	Very soft	17400	6900	550	0·80	Du Bois and Taylor Jones
Softest selected iron... ...	trace	—	Very soft	17430	10400	804	0·44	Kamps
Piano steel wire	0·95 (?)	—	Annealed, soft	14500	10400	824	22·00	Ewing
Piano steel wire	0·95 (?)	—	Glass-hard	12600	9600	760	40·00	Ewing
Low-carbon steel	0·06	—	Quenched at 1000° C., soft	19800	7812	625	3·40	Mme. Curie
High-carbon steel	1·20	—	Quenched at 800° C., hard	15080	8060	645	58·00	Mme. Curie
Haarlem magnet steel ...	—	—	Hard	16900	10048	800	56·00	Du Bois and Taylor Jones
Allevard steel	0·59	5·50	Not quenched	18700	11250	900	26·00	Mme. Curie
Allevard steel	0·59	5·50	Quenched at 770° C.	17500	10500	800	73·00	Mme. Curie
Böhler's Styrian steel... ...	—	—	Soft	17850	9950	790	34·00	Du Bois and Taylor nes
Böhler's Styrian steel... ...	—	—	Hard	14000	7570	600	75·00	Du Bois and Taylor Jones
Remy tungsten steel ...	—	—	Hard	15145	10157	808	63·00	Eichel
Remy tungsten steel ...	—	—	Very hard	16070	10040	800	77·00	Du Bois and Taylor Jones
Medium tungsten steel ...	0·89	3·08	Quenched at 760° C., hard	11000	7330	572	58·80	Swinden
Whitworth tungsten steel ...	0·51	4·01	Quenched at 900° C., hard	—	—	610	37·00	J. G. Gray and A. Ross
Molybdenum steel	1·25	(Mo) 3·36	Hard ($\delta = 23$)	10000 (?)	4651	370	85·00	Mme. Curie
Chilled cast iron	—	—	Chilled at about 1000° C.	9000 (?)	1775 1850	218 229	52·80 51·10	Campbell Campbell
Lodestone	—	—	—	—	—	350	50·00	Du Bois

1 per cent, proportional to the amount of carbon present. And so, also, broadly speaking, is its coercive force. Later on I shall refer to some important investigations by Mme. Curie on the alloy steels: but she made also some highly significant measurements on carbon steels. Fig. 2 gives two hysteresis loops determined by her. The one marked A relates to a low-carbon steel containing only 0·06 of 1 per cent of carbon, and was therefore practically a pure iron. The other, marked B, was a fairly high-carbon steel containing 1·2 per cent of carbon. Both had been quenched, B at about 800° C., when it hardened; A at about 1000° C., which, however, did not harden it. It will be seen that for both kinds the value of I_{rem} was nearly alike, being 625 for the low carbon and 645 for the high-carbon steel; but the low-carbon steel held its magnetism very loosely, the coercive force being only about 3·4; while the high-carbon steel held its magnetism much more fixedly, the

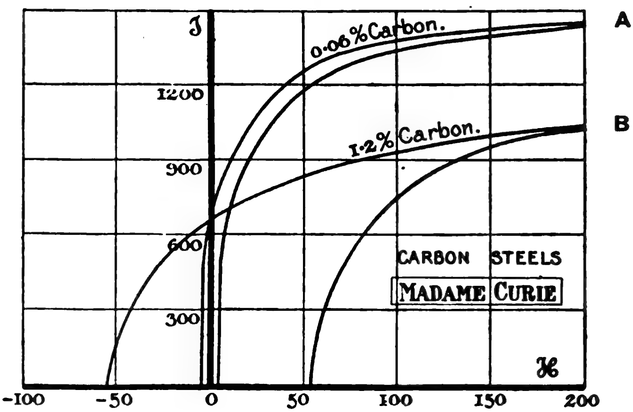

FIG. 2.—Hard and Soft Carbon Steels (Mme. Curie).

coercive force being about 58. According to Professor Arnold, though the magnetic permeability of a specimen is inversely proportional to its carbon content, the amount of permanent magnetism retained is directly proportional to the carbon content.* This statement is not true of the values of I_{rem}; but is more nearly true of the values of the coercive force. The next diagram, Fig. 3, is due to Dr. Carl Benedicks, and shows the variation of the coercive force with the carbon content. From this we see that the mild steels having less than 0·25 of 1 per cent of carbon have very little coercive force; but that the coercive force increases between 0·5 and 1 per cent, and at 1·15 per cent carbon content reaches a value of about $H_c = 50$. Obviously this affords a first reason why high-carbon steels make far better permanent magnets than do the low-carbon sorts.

* Arnold regards the constituent hardenite (see p. 103) as a definite subcarbide of iron, of composition $Fe_{24}C$; and holds that the permanent magnetism depends upon the amount of this substance present.

But it has been known for many years that certain steels containing other constituents beside carbon possess special qualities both as respects hardness and as respects magnetic fixidity. The sort known as Mushet's steel, the earliest of the great class of tool steels, was found to remain hard even when very hot; and when forged it was self-hardening without having to be quenched in cold water. It was found to contain* from 7 to 12 per cent of the metal tungsten, besides having from 1½ to 2 per cent of carbon. In recent years many varieties of self-hardening tool steels have been produced by metallurgists, some containing tungsten, others containing other metals such as molybdenum or chromium, also manganese or vanadium. All such steels belong to the class to which the generic name of *alloy steels* is given, to distinguish them from the pure carbon steels. The alloy steels may or may not contain carbon; and they vary enormously in their properties. Aluminium steel and silicon steel are very soft; and, as Sir William

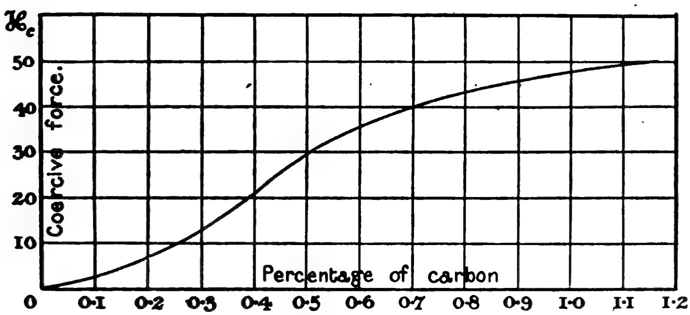

FIG. 3.—Variation of Coercive Force with Percentage of Carbon (Benedicks).

Barrett has shown, may even surpass pure charcoal iron in having a great permeability and very little coercive force. Manganese steel is very hard, and, curiously, is almost non-magnetizable. Many of these alloy steels are coming into use in the industries; and their introduction has opened out a new branch of the steel industry. Magnetically several of them are of extreme interest. For many years it has been known that a particular kind of steel coming from the forges of Allevard, near Grenoble, made most excellent permanent magnets. The Allevard steel is a tungsten steel containing about 5½ per cent of tungsten and about 0·59 per cent of carbon. Fig. 4 shows the tests made by Mme. Curie on a specimen of Allevard steel. Curve A shows the magnetic behaviour of the specimen as received from the forge, not specially annealed, neither specially hardened. Curve B shows its behaviour after being heated to 770° C. and quenched at that

* The Mushet steel manufactured by Messrs. S. Osborn & Co. has, on the average, 5·8 per cent of tungsten, 1·65 carbon, 2·12 manganese, 0·45 chromium, and 1·36 silicon. B. E. Jones gives 8·22 tungsten, 2·3 carbon, 1·72 manganese, and 1·6 silicon.

temperature in cold water. The value of I_{rem} was decreased from 900 to 800, and the coercive force was increased from 26 to 74, by quenching.

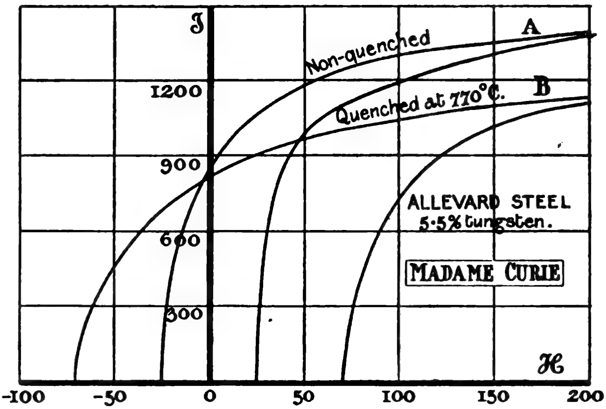

FIG. 4.—Unhardened and Hardened Tungsten Steel (Mme. Curie).

Many steel-makers now make special magnet steels, using alloys of tungsten and other metals. Magnet steels can be obtained from various Sheffield firms: Messrs. W. Jessop & Sons; Messrs. Edgar Allen & Co.;

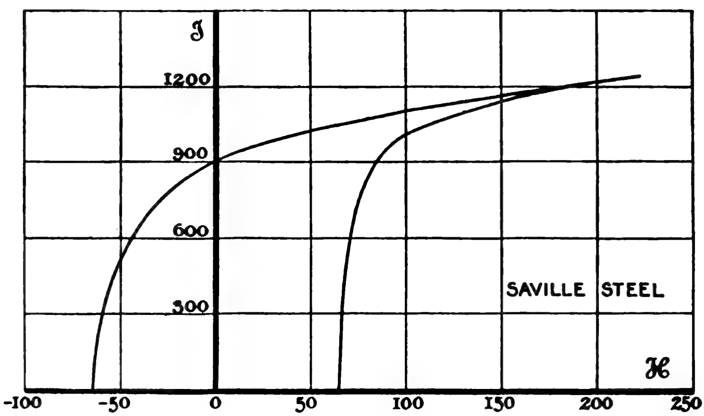

FIG. 5.—Saville's Tungsten Steel (Hardened).

Messrs. Seebohm & Dieckstahl; Messrs. S. Osborn; Messrs. J. J. Saville & Co.; Messrs. T. Firth & Sons; as well as from certain Continental firms such as Heinrich Remy, of Hagen in Westphalia; Ch. Pinat & Cie, Allevard, Isère; and Messrs. Böhler Brothers, of Kapfenberg and Vienna. An example is afforded by a tungsten steel of Messrs. J. J. Saville, of Sheffield, Fig. 5, which has a remanence

$I_{rem} = 917$, and a coercive force of 65, which exceeds that of any carbon steel. I shall return to questions affecting the alloy steels later on.

Modern Metallurgical Views on Steel.

But if modern metallurgy has taught us that the chemical composition of the steel is important, it has also taught us that there is something equally important, namely, the constitution or structure of the steel. Perhaps no advance in recent times has had an importance comparable with that connected with the application of the microscope to the study of metallic structures. The science of metallography, founded by Sorby, and extended by Ewing, Roberts-Austen, Stead, Rosenhain, and Beilby in this country, and by Osmond and many other workers on the Continent, is revealing the interior secrets of the metals and alloys. As the result of micrographic researches steels are known to contain certain structural constituents known as ferrite, cementite, pearlite, martensite, hardenite, austenite, etc., which can be recognized in the microscope. Of these, ferrite appears to be—in the carbon steels at least—pure iron in small definite crystals; and cementite appears to be a definite carbide of iron Fe_3C. Pearlite, so called from its nacreous lustre, is that particular mixture of constituents which has the lowest transformation point, and which, remaining mobile in the solid mass down to about the temperature of 690° C., is the "eutectic" * (or most fusible) of all possible of the carbon-iron alloys. It has a composition of about 0·9 of 1 per cent of carbon to 99·1 per cent of iron. If a (mild) steel containing less than this percentage of carbon is heated to its melting-point, and allowed gradually to cool, its solidification begins by some ferrite (pure iron) freezing out and forming crystals throughout the plastic mass; which crystals grow at the expense of the rest until the remaining mobile part has reached the percentage of pearlite, when it all solidifies, the ferrite crystals being found enclosed in a surrounding matrix of pearlite.† On the other hand, if a high carbon steel containing over 0·9 per cent of carbon is melted and allowed to cool, cementite will be formed first, and as the solidified mass cools, the percentage of carbon in that part which is still in a mobile state, will fall until the percentage of pearlite is reached, when all the enclosed residue becomes solid, the masses of cementite being then found to be surrounded by a matrix of pearlite. But if the cooling instead of being gradual is sudden, other changes occur, differing with the proportions of carbon present, and with the suddenness of the cooling. In high carbon compositions rapid cooling results in the formation of characteristic structures which influence the mechanical properties. Thus martensite is a constituent which usually exhibits a structure apparently consisting of interlaced needle-like crystals, making the steel very

* Howe's term *æolic* is certainly preferable here to the more commonly used term *eutectic*. For while *eutectic* properly signifies most fusible, we have here to deal with a solid undergoing transformation, and with a composition that has lowest temperature of transformation.

† At this point in the lecture a number of micrographic photographs were projected upon the screen in illustration of these metallographic discoveries.

strong, as required for steel rails. The hard constituent called hardenite (which may be a solid solution of carbon in iron, or possibly a subcarbide of iron) is of the same percentage as pearlite, and is formed during any cooling that is so rapid that the particular pearlite structure has not time to form. This constituent appears to be the most important one in magnets. Another structure called austenite, which consists of zigzag streaks in a mass of hardenite, appears to be a pure 1·6 per cent carbon steel, a solid solution of carbon or of

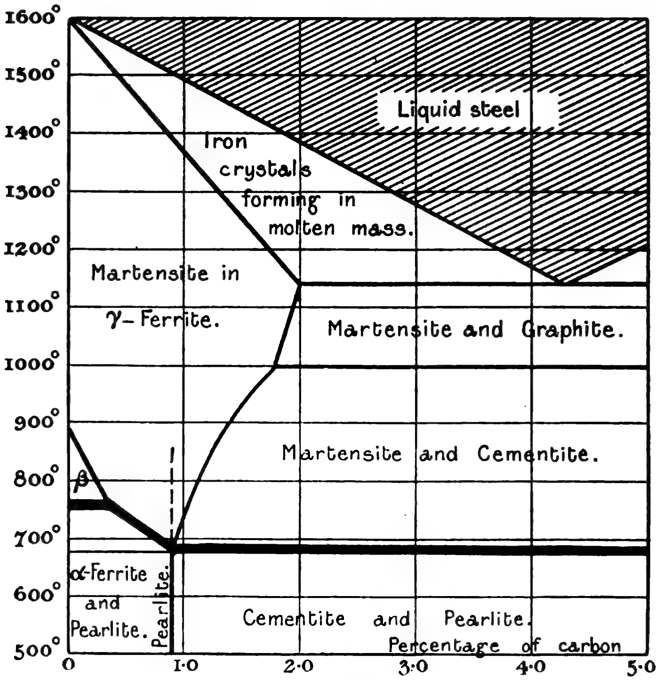

FIG. 6.—Diagram of Equilibrium States of Carbon Steels at Different Temperatures.

cementite in the rest of the steel. The subject is so wide that it is quite impossible here to give any adequate summary of it: suffice it to say that metallurgists have sought to explain, from the properties of these constituents, the mechanical properties of different kinds and qualities of steel, and to account for the phenomenon of mechanical hardening. But the matter does not stop here. Certain lines of reasoning have led to the hypothesis that iron itself can exist in three different states, or phases, which some regard as allotropic forms; and these pass spontaneously into one another by mere change of temperature. Pure iron at ordinary temperatures is soft and highly magnetizable, and is called Alpha iron. When heated to about 750° C. it passes into

the Beta state, in which state it is rather harder and non-magnetic. When heated to about 860° C. the Beta iron passes into the Gamma state (also non-magnetic). On cooling the changes occur in the reverse order. Fig. 6, which is due to Roberts-Austen,* modified by Roozeboom and others, summarizes the foregoing matters in a graphic way. In this diagram the percentage of carbon in the steel is plotted horizontally, from 0 (pure iron) on the left to 5 per cent (cast iron) on the right. Temperatures up to 1600° C. are plotted vertically. For ordinary steels containing from 0 to 2 per cent, the melting-point lies between 1600° and 1380° according to the carbon content. Below these temperatures, for some way down the diagram, the material is plastic consisting of gamma-ferrite mixed with martensite, or (if a greater proportion of carbon is present) of martensite mixed with cementite. As the plastic stuff cools its constitution alters; the various regions of the diagram indicating the state that is stable at the various temperatures. A steel with the particular composition of 0·9 per cent of carbon (indicated by the dotted vertical line) is that which remains homogeneous, or unsegregated, to the lowest temperature, namely, about 690°, when it changes into pearlite.

Now we wish to connect these metallurgical discoveries with the magnetic properties of the steels. The first and most notable connection is indicated by the thick line which runs nearly horizontally across the diagram. It marks the temperatures at which the steels of different percentage composition cease to be magnetic. Steels of every kind when heated above about 700° C. cease to be magnetic. That is, they are not attracted by a magnet, and cannot act as magnets. They all, with certain rare exceptions, regain their quality of magnetizability when cooled down again below that temperature. For all high-carbon steels the temperature is from 680° C. to 690° C.; for low-carbon steels a little higher; and for pure iron itself about 760° C. The consideration of this matter will be resumed under the head of heat-treatment.

Effect of Shape and Dimensions.

It has long been known that, for a given material, long bars are more retentive than short ones; and that nearly-closed forms such as horse-shoes, or rings with a slit across them, are more retentive than forms that have their ends widely apart. Nearly forty years ago, as I remember, Lord Kelvin taught us that short bars have no magnetic memory. Squat forms such as short cylinders, cubes, or spheres, even if made of the hardest and best steel, have surprisingly little retentivity. Long bars of soft stuff usually keep much more magnetism than do short bars of hard steel. The reason is that the poles of every magnet exercise a self-demagnetizing influence on the body of the magnet: and this self-demagnetizing influence depends both on their shape and on their strength. The self-magnetizing effect is best dealt with by means of a

* Fourth Report of Alloys Research Committee. *Proceedings of the Institution of Mechanical Engineers*, Parts 1 and 2, p. 70 and p. 90, 1897.

self-demagnetizing coefficient or factor, which in the case of cylindrical bar-magnets depends only on the ratio that the length bears to the diameter. That ratio, the so-called dimension-ratio, is, as you will presently understand, of immense influence upon the qualities of the magnet. In a bar magnet the poles are continually acting on one another, and exercising demagnetizing forces on the parts of the magnet that lie between them. The shorter the bar, the nearer together are its two poles, and the greater the demagnetizing action which they exert on the steel in the middle of the bar. Let H_d stand for the

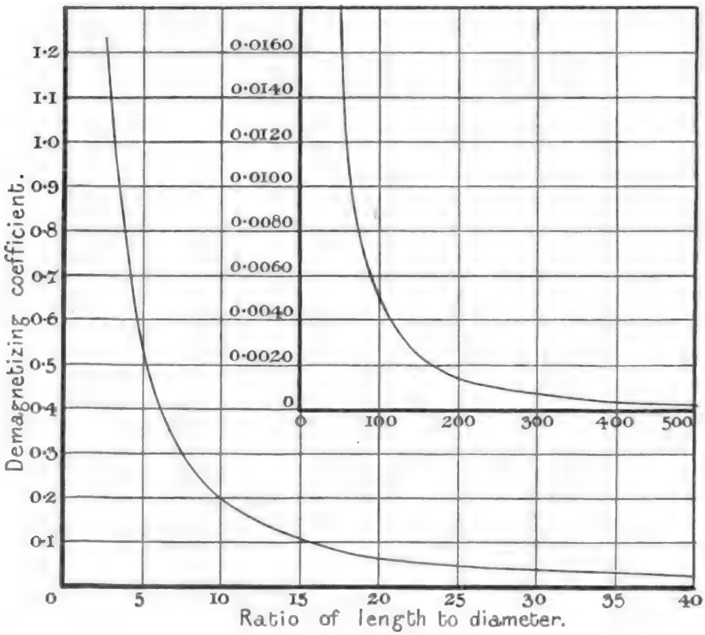

FIG. 7.—Dimension-ratios and Demagnetizing Coefficient.

demagnetizing field due to the poles; its value is found to be proportional to the intensity of the intrinsic magnetization **I**. We may, then, write $H_d = D \times I$; where D is the coefficient of demagnetization; and obviously it may be stated as $D = H_d \div I$. It is possible to determine the values of D for cylindrical magnets of different lengths and thicknesses by pure experiment. It is also possible to predetermine them by theoretical calculations from the properties of ellipsoids, though approximately only. Various persons, including Sir Alfred Ewing, Professor Ascoli, Professor Du Bois, Dr. Riborg Mann, and Mr. C. L. B. Shuddemagen, have determined their value. The most recent figures are those published by myself * and my former assistant, Mr. E. W. Moss. Our

* *Proceedings of the Physical Society of London*, vol. 21, p. 622, 1907–9.

figures, which agree closely with those of the earlier investigators, were undertaken to extend the measurements to shorter bars, and bars of rectangular section. Fig. 7 gives graphically the values of the demagnetizing coefficient as it falls from 1·2, for rods that are 2·66 diameters long, to 0·0223 for rods that are 40 diameters long. Numerical values are given in the following table, in which the values for rods that are more than 40 diameters long are taken from the dissertation of Riborg Mann.

Dimension-ratio, δ	2·5	2·66	3·55
Demagnetizing Coefficient	1·3	1·2	0·83

l/d	4·44	5	5·34	6·66	8·86
D	0·618	0·53	0·483	0·352	0·233

l/d	10	13·3	15	17·72	20
D	0·198	0·129	0·108	0·0826	0·069

l/d	25	30	35·6	40	50
D	0·0438	0·036	0·0255	0·0223	0·0182

l/d	60	70	80	90	100
D	0·0131	0·0099	0·00776	0·00628	0·00518

l/d	150	200	300	500	1000
D	0·00251	0·00152	0·00075	0·00018	0·00005

Passing on to the case of slit rings, it has been shown by H. Lehmann and H. Du Bois that the demagnetizing effect of a slit in a ring magnet is nearly proportional to the width of the slit, that is to say, to the width of the gap in the magnetic circuit. It would be precisely proportional if there were no magnetic leakage, and if the gap itself were small compared with the radius of the ring. The demagnetizing coeffi-

cient is, in fact, about 0·035 per degree of the width of the slit. But if the ring, instead of being merely slit, is provided with enlarged pole-pieces which approach one another, so as to leave a narrow gap between the two polar areas, the demagnetizing coefficient may be very greatly reduced. Fig. 8 represents diagrammatically such a magnetic circuit, with enlarged polar surfaces. The dotted line represents the mean path of the flux along the magnet core. Let this length be called l_m, and let the width across the gap be called l_g. Let the area of cross-section of the magnet core be called A_m, and the area of cross-section of the flux at the gap—practically the same as the area of either pole-face—be called A_g. Also, let ν denote the coefficient of allowance for magnetic leakage, that is, the ratio of the total flux in the magnet core at its

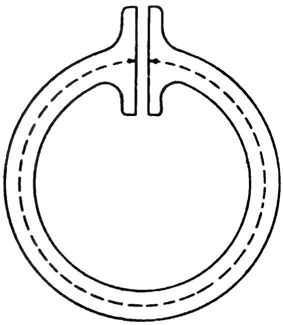

FIG. 8.—Theoretical Magnetic Circuit.

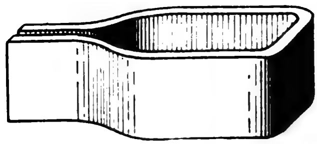

FIG. 9.—Form of Magnet used in Thomson-Houston Meter.

middle to the useful flux in the gap. Then, in terms of these quantities the demagnetizing coefficient may be written—

$$D = -\frac{4\pi}{\nu}\left\{\frac{l_g}{l_m} \cdot \frac{A_m}{A_g}\right\}$$

From this it appears that the tendency to self-demagnetizing may be reduced at will by making A_g large or l_g small, or by providing a large magnetic leakage. In 1888 Hookham pointed out the significance of the ratio above enclosed in brackets, and gave the empirical rule* that for magnets that should be both powerful and constant that ratio should not exceed $\frac{1}{10}$. But with modern steel, and with allowance for magnetic leakage, higher values may be permitted.

As an example may be cited a magnet of the form shown in Fig. 9

* Mr. Hookham, writing in 1912 to the author, in reply to a query whether he considered this rule to be still adequate, said that having been indirectly experimenting on it, ever since 1888, in the application of it to magnets in hundreds of thousands of meters, he has found it a safe law. He adds: "It is really a rule for using the steel to the best advantage—for producing the most intense permanent field in an air space of given distance between poles and given cross-section with the greatest economy of steel. If magnet steel improved in quality, i.e. in retentive power, the length of magnet might be reduced; but there has been no appreciable improvement—I doubt if there has been any."

used in the Thomson-Houston meter. The values measured were: A_g, 84 sq. cm.; A_m, 2·1 sq. cm.; l_g, 0·375 cm.; l_m, 24·5 cm.; and ν was ascertained by experiment to be 1·35. Hence for this magnet D=0·00358, which is the same as for a rod 130 diameters long, or as for a plain slit ring having a slit only $\frac{1}{10}$ of a degree wide.

Professor Ascoli, of Rome, who has devoted much attention to the permanency of magnetism, has suggested a graphic method of handling the question of the degree to which the residual magnetism of a magnet is reduced by this self-demagnetizing action. If, neglecting magnetic leakage and the resulting variations of I along the body of the magnet, we assume a uniform mean value, and if we also assume that the demagnetizing coefficient of a given magnet is a constant at all stages in the cycle of magnetization, we may then ascertain the intensity of the demagnetizing field at any stage as follows:—

In Fig. 10, let the values of I for any given steel be plotted against the values of **H**, giving the usual loop curve, as determined for a very

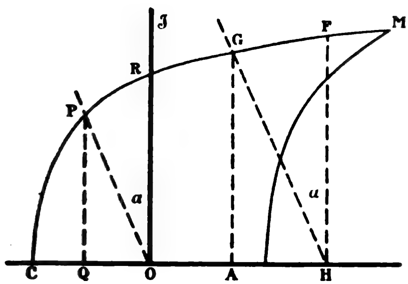

FIG. 10.—Construction for Demagnetizing Coefficient.

long bar or for a ring, that is to say, as determined under such conditions that self-demagnetizing actions are absent. Suppose that the applied magnetizing forces have been carried to a certain maximum, and then lowered so as to have the value indicated in the diagram by the length O H, and that, following the curve from the highest point M to the point F, the intrinsic magnetization I has fallen to the value H F. It is desired to learn how much further it will fall in consequence of self-demagnetizing influence. To ascertain this, draw from H a line H G, meeting the curve at G, making an angle G H F or a, such that tan a is numerically equal* to the demagnetizing coefficient. (For example, if the magnet has a dimension-ratio of 12, reference to Fig. 7, p. 92, shows that its demagnetization coefficient will be 0·15; and the angle whose tangent is 0·15 is about 8½ degrees). Then the length G A is the corrected value of I; for it is the value of I which corresponds to a

* This is on the assumption that I and **H** are plotted to equal scales. But usually a much wider scale is adopted for **H**. Hence the plotted angle will be such that its tangent is equal to the numerical value of the demagnetization coefficient multiplied by the ratio of the scales used for I and **H**.

magnetizing force equal to the difference between the applied magnetizing force O H and the demagnetizing force A H. Now apply this construction to the case where the whole of the impressed magnetizing force has been removed. If demagnetizing influences were absent, the value of I_{rem} would be denoted by the line O R. But drawing the line O S making the angle α with O R, we obtain T S as the corrected value of the remanent magnetization.

An example of the use of Ascoli's construction is afforded by the next diagram, Fig. 11, where the steel is assumed to be Remy's tungsten steel, as used for magnets, with I_{rem} of 808 and a coercive force of 63. Draw through the value $I = 1000$ a horizontal line, and mark on it the points that correspond to values of **H** equal successively to -50, -100, and -150. The sloping dotted lines joining these points to the origin will then correspond to the successive demagnetization coefficients of 0·05, 0·10, and 0·15. The first of these sloping lines corresponds to a dimension-ratio (see p. 92 above) of about 25. If, therefore, we were to

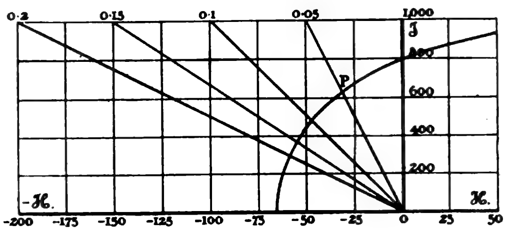

FIG. 11.—Effect of Demagnetizing Coefficient in reducing the Permanent Magnetism.

make of this steel a bar magnet 25 diameters long, its intrinsic remanent magnetization would not be $I_{rem} = 808$, but it would, by reason of the self-demagnetizing of the bar, fall to the point P, where the first sloping line crosses the curve, that is, to about $I = 640$. Similarly, a shorter bar that was only 12 diameters long, having (see p. 92) a demagnetizing coefficient of about 0·15, would only retain an intrinsic magnetization of about $I = 360$.

The influence of the dimension-ratio upon the magnetism of bar magnets is shown by some experiments of the late Professor Thomas Gray. Using a glass-hard charcoal steel, he found the amount of the remanent magnetization to increase regularly as the dimension-ratio of the bars was increased, as follows :—

Dimension-ratio	10	16	20	31	44	50	73	105
Remanent Magnetization ...	216	256	288	312	344	376	512	528

The values of I_{rem} do not approach the figure of 800, this being a carbon steel; and it appears that the magnet that was only 10

diameters long had a magnetization less than half as intense as that of a magnet that was 105 diameters long, though both had been subjected to equally powerful magnetizing forces. The self-demagnetizing action of the short bar is very evident.

Two other examples of the dependence of the remanent magnetism upon the dimension-ratio of the magnet may be cited from the researches of Mme. Curie :—

CARBON STEEL (0·84 % CARBON. QUENCHED AT 770° C. $H_c = 53$).

Dimension-ratio	20	22	71	Infinity (ring)
Remanent Magnetization ...	420	480	580	640

TUNGSTEN STEEL (5·5 % TUNGSTEN ; 0·59 % CARBON. QUENCHED AT 770° C. $H_c = 74$).

Dimension-ratio	20	23·5	Infinity (ring)
Remanent Magnetization... ...	560	680	850

It was formerly supposed, from the researches of Scoresby, Jamin, and others, that a laminated magnet, that is, one built up of steel strips assembled together, with common pole-pieces, was more powerful than a solid magnet of equal weight and length. In old days, before the use of powerful electromagnets to magnetize the steel bars or horse-shoes, this may well have been true, provided each lamina was separately magnetized as strongly as possible before assembly. But with the modern kinds of homogeneous fine-grained steels now available, and with modern processes of magnetization, this alleged superiority of laminated magnets is illusory. Moreover, Ascoli has shown that bundles of wires or of strips possess the same self-demagnetizing coefficient as do solid magnets of the same form and size.

HEAT-TREATMENT.

We will now enter upon the question of the requisite treatment which the magnet must receive in the furnace, and in the quenching bath, if it is to yield the best performance. But this is only part of a much bigger subject, the alterations which steels undergo in their physical properties when raised or lowered in temperature.

It is found that when any kind of steel is steadily heated, or cooled, the rise or fall of its temperature is not uniformly continuous, but that there are certain pauses or points of arrest, during which the tempera-

ture of the material remains temporarily unchanged. Fig. 12* indicates the general nature of these pauses. If observations of the temperature

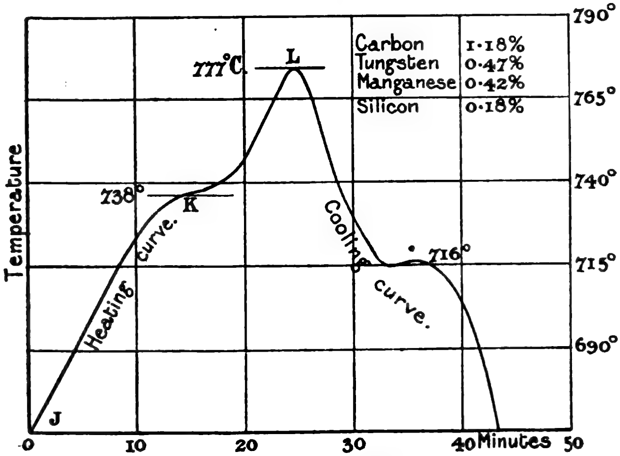

FIG. 12.—Heating and Cooling Curve.

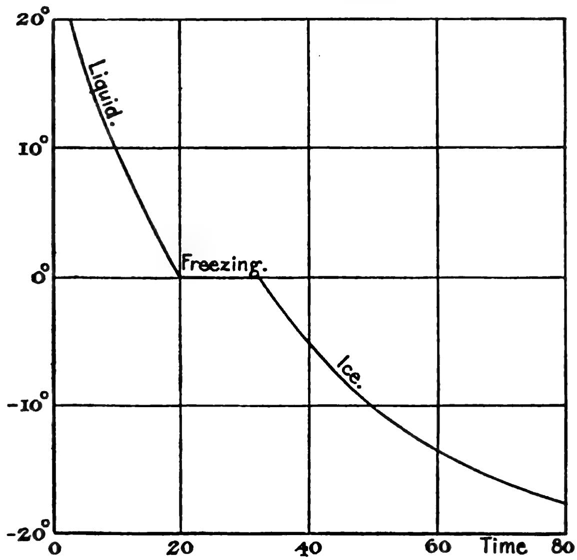

FIG. 13.—Cooling Curve of Water.

are made every few minutes, and plotted as a curve, or if continuous observations are made with an electric recording pyrometer, the curves

* Fig. 12 is taken from the researches of Mr. S. Neave Brayshaw. Figs. 13, 14, and 16 are taken from Mr. J. W. Mellor's book *The Crystallization of Iron and Steel* (1905).

are found to exhibit these pauses, sometimes more than one, during heating or during cooling. For example, a high-carbon steel containing 1·2 per cent of carbon will show a pause when the rising temperature reaches about 730° C., after which it goes on ascending; and another pause during cooling when it has fallen to about 690° C. The low-tungsten steel of Fig. 12, containing also about 1·18 of carbon, has the rising pause at 738°, and the pause during cooling at about 716°. Similar peculiarities are noticed in the curves which depict the behaviour of some other substances; as, for instance, the curve of the cooling of water when it freezes into ice (Fig. 13). Here, in cooling water by a freezing mixture, down to —20° C., the pause occurs at 0°,

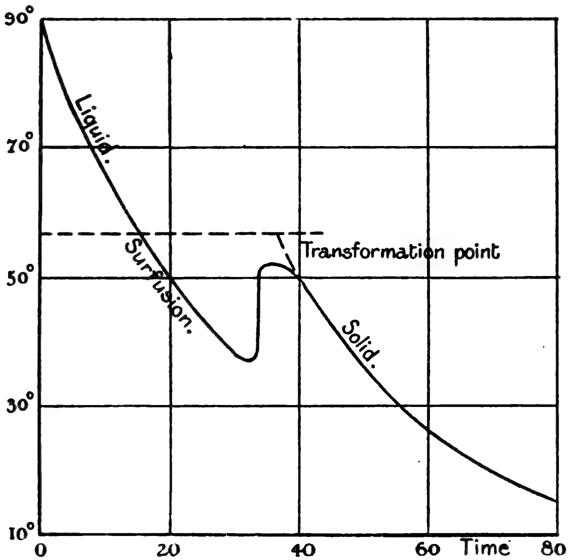

FIG. 14.—Cooling Curve showing Solidification after Surfusion.

that is, at the temperature when it freezes. The arrest of the fall of temperature until solidification is complete is a very well-known fact; and the pause indicates that at this temperature the physical change or transition from liquid to solid takes place. The latent heat of liquidity of the water is given up as it solidifies, and during the period in which that latent heat is being given up the temperature cannot fall. Suppose we take some crystals of common "hypo"—that is, sodium thiosulphate —and melt them in a flask. They melt at about 56° C. Let the molten liquid be heated up 20° or 30° higher and then left to cool, and let observations be taken of the falling temperature. A curious result follows. The substance is one which exhibits the phenomenon of surfusion. It remains liquid even when cooled a few degrees below its proper freezing-point, which is 56° C., and may even be cooled down

to 25° C., or 20° C., without solidifying. But if it is then caused to solidify by contact with a minute scrap of crystal or "hypo," it begins to solidify, and the temperature at once rises to about 56° C.; after which solidification the fall of temperature is resumed. Here, again, the pause and the kink of recovery, which the curve (Fig. 14) shows, indicate a definite transition or change of physical state; the transition being in this case accompanied by such a liberation of latent heat that there is not merely a pause but an actual transient rise of temperature. Now this phenomenon can be paralleled in the behaviour of steel. It was discovered in 1873, by Barrett, that if a bright red-hot piece of steel (such as an old file) is watched during its cooling, when it has cooled down to a deep red colour it suddenly shines up again more brightly, as if it had been heated up from within; as is indeed

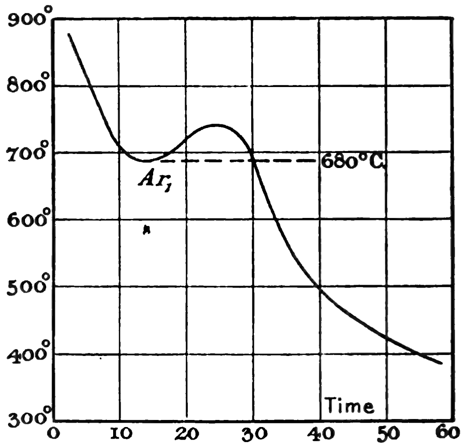

FIG. 15.—Cooling Curve of Steel, showing Recalescence (Roberts-Austen).

the fact. It undergoes a transformation of some physical sort, accompanied by a liberation of latent heat which actually makes the temperature rise again. The phenomenon is known as "recalescence." It can best be shown by stretching a piece of piano-wire across a room, and heating it by passing an electric current through it until it is brightly red-hot. On switching off the current it cools, and when it reaches the temperature of transition it recalesces; and the momentary rise of temperature can be seen not only by its increased brightness of glow, but by the expansion which occurs, making the wire momentarily sag.

The cooling curve of certain kinds of steel shows clearly* the

* Apparatus for observing and recording such cooling curves was originally devised by Le Chatelier and by Roberts-Austen, as described in the first Report of the Alloys Research Committee in 1891. More distinct results are sometimes obtained by adopting a differential method, and by recording in the curves, not the fall of temperature through a given time, but the time taken to produce a fall of one degree. The

occurrence of recalescence. For, as in Fig. 15, the temperature falls below that of the transition period (680° C.), then rises about 60 degrees, and again falls.

Now see what is found with pure iron. At a temperature of 1700° or 1800° C. it is liquid, but when it cools to about 1600° it freezes solid; and during solidification there is a large pause in the fall of temperature. Then the cooling curve falls steadily until a point, marked Ar_3 in Fig. 16, is reached, when again there is a pause at about 860°; and another pause,* marked Ar_2, occurs at about 750°.

These pauses unquestionably mark the transition of the steel from one physical state to another, heat being absorbed or evolved during the change of state. It was held by Osmond, and with some modification is held by other metallurgists, that they mark the transformation of

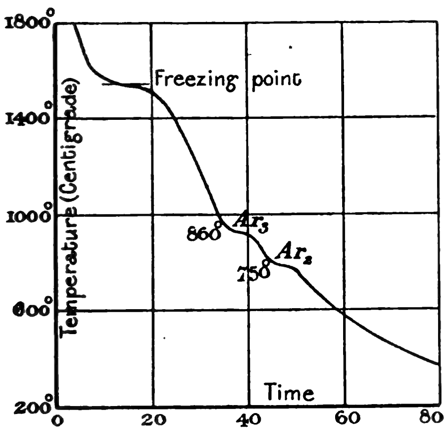

FIG. 16.—Cooling Curve showing Transformation Points of Pure Iron.

the iron in the steel from one of its phases or states to another. Thus, during heating, alpha-iron on reaching Ac_1 absorbs some heat and is transformed to beta-iron; and beta-iron on reaching Ac_2 is transformed to gamma-iron; and the reverse transformations occur during cooling. It will be noted that always, and for every specimen, the corresponding

whole question of the recording of recalescence curves, and in particular of such inverse curves, is admirably expounded by Rosenhain in the *Proceedings of the Physical Society*, vol. 21, p. 180, 1907–9.

* The notation is due to Tschernoff and Osmond. There are, in general, three transition points, or points of arrest, in any heating or cooling curve of steel. Those on a heating curve are marked Ac, the c indicating *chauffement*. Those on a cooling curve are marked Ar, the r standing for *refroidissement*. The lowest number is the lowest temperature. Thus Ar_3 means the highest of the three transition temperatures on a cooling curve. They are usually not very sharply defined, being ranges of temperature rather than points; and they differ in different brands. For instance, Ar_3, which for pure iron is at about 860°, is in a mild steel lower. In a 0·02 per cent carbon steel Ar_3 begins at about 840° and extends to about 800°. In a 1 per cent carbon steel Ar_3 occurs at about 710° or 720°, when it is difficult to distinguish from Ar_2 and Ar_1, and the point is then denoted as Ar_{321}.

transition points are a little higher on the heating curve than on the cooling curve. Thus, for the low-tungsten high-carbon steel shown in Fig. 12, Ac_1 is at about 738°, and Ar_1 at 716°. Also, generally, any increase in the percentage of carbon lowers the transition points, and the presence of other elements—manganese, chromium, silicon, etc. —affects them. The non-magnetic gamma-iron, which in pure iron exists stably only at temperatures over 860°, will, when carbon is present, exist stably to a lower temperature. In a 0·3 per cent carbon steel the gamma-iron will persist stably down to about 780° or 740°, when it changes to alpha-iron (or to beta-iron and then to alpha-iron), evolving heat and causing the recalescence. In a 0·6 per cent steel the change occurs at 735° to 705°. In a 1·2 per cent carbon steel the gamma-iron is transformed straight to alpha-iron at about 690°. These things are summarized diagrammatically in Fig. 17.

For us the most important fact of all is that when any specimen has

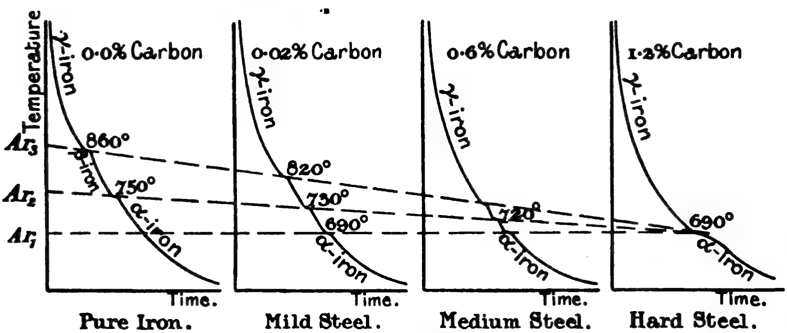

FIG. 17.—Cooling Curves for Steels of Different Composition (after J. W. Mellor).

cooled down through the recalescence stage it is capable of being magnetized. A steel, if of such a composition that it can harden, hardens if quenched at a temperature above recalescence. If it is cooled slowly through the recalescence stage and quenched at a lower temperature it is not thereby hardened, or only hardened imperfectly. And as the coercive force of magnets is bound up with their being properly hardened, it is imperative for magnets which are to have the greatest coercive force that they shall be quenched at a temperature above that at which they recalesce; that is to say, they must be quenched at a temperature such that the iron in them is still in the non-magnetic or gamma state. And they must be quenched *quickly*.

Now this raises the much-disputed question as to the reason why steel becomes hard when quenched. An old and fantastic idea, which we may at once dismiss, was that some of the carbon solidified as minute diamonds. Another suggestion is that in the sudden cooling the hard gamma-iron has not time to transform itself into alpha-iron, and remains fixed. Another is that the constituent hardenite, an alloy

containing about 0·9 of 1 per cent of carbon (which if cooled slowly has time to transform itself into pearlite), is arrested and remains excessively hard. On this view hardenite is in quenched steels the hard matrix which holds together the crystals of ferrite, if the steel has less than 0·9 per cent of carbon, or the crystals of cementite, if the steel has more than 0·9 per cent of carbon. No final view has yet found acceptance. But why should the hardening by sudden quenching confer the power of magnetic retentivity? Apparently the gamma-iron and all the several constituents, whether called ferrite, austenite,

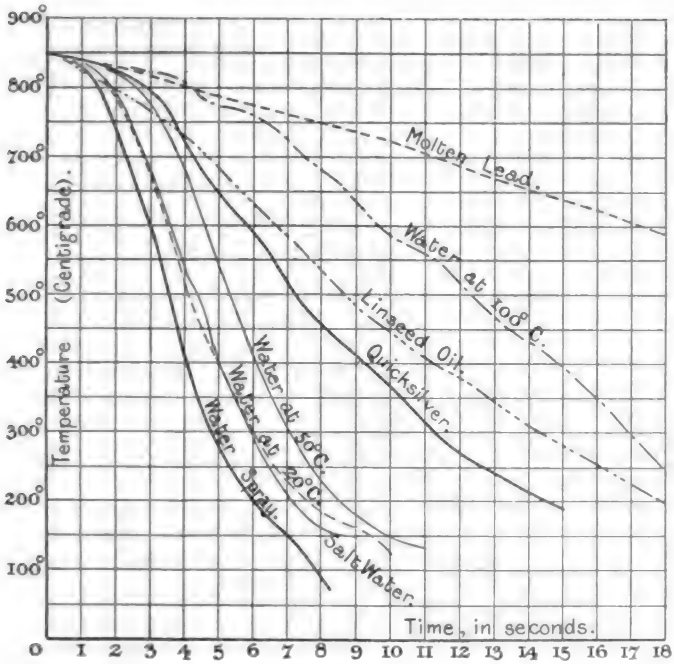

FIG. 18.—Quenching Curves (adapted from Le Chatelier).

martensite, sorbite, hardenite, or cementite, are non-magnetizable at temperatures above the recalescence point. Apparently also, from the recent researches of Dr. S. W. J. Smith, cementite is non-magnetic * at temperatures above 210° C. Why should the quenching, which presumably arrests the transformations that some of these constituents would otherwise undergo in cooling, render them capable of retaining magnetism? It cannot be said that any satisfactory answer † has yet

* This is perhaps the reason why magnet-makers consider that those carbon steels which make the best permanent magnets contain less than 0·9 per cent of carbon.

† In a paper published after the delivery of this lecture, Dr. S. W. J. Smith suggests that ferrite contributes nothing, or less than nothing, to the *permanent* magnetism.

been given. According to the modern view, the act of magnetization in iron or steel consists in the production of molecular arrangements, in which the magnetic molecules, whatever they be, are brought into alignment with the magnetizing forces. This is the theory of Weber as modified by Ewing, and of its general truth there can be no doubt. But this at once raises the question: Why quenching confers on the steel the greater power to retain the aligned rows or groups of magnetic molecules in position, and to prevent them from instantly returning into a miscellaneous disposition when the magnetizing forces that have constrained them into alignment have been removed? One thing emerges from the conflicting theories: that sudden cooling of the steel, at whatever stage, tends to conserve whatever composition, structure, or state, the substance possessed at that temperature, and which it would have lost if it had cooled slowly. Bearing this in mind, it will be evident that rapidity of cooling is as important as the right temperature. We must cool the steel, but how quickly, and to what degree? It is obvious that if we plunge a brightly red-hot bar into ice-cold water it will be chilled more rapidly than if we plunged it into hot water, or boiling oil, or molten lead. But if we apply a water-spray at, say, 20° C., it will chill the bar quicker than plunging it into water at 0° C. Some kinds of steel will not withstand a quenching in ice-cold water, but split or crack. Thin bars which present more surface, relatively to their solid contents, than thick ones, are more rapidly chilled than thick ones, though both are dipped into the same brine. Tool-makers and magnet-makers have their own procedures, the results of experience; but some of the recipes they follow are quite absurd. There is no advantage in adding to the water in the quenching tank, stale beer or other organic liquids. By means of his hardness testing machine Brinell found the quenching of a 0·1 per cent mild steel, having previously a hardness of 99 on the Brinell scale,* to produce the following hardnesses with the cooling agents named:—

	Temperature of Bath.	Hardness.
	° C.	
Molten lead	350	112
Boiling water	100	118
Wood tar...	80	121
Cold water	20	149
Brine	20	156
Soda solution	20	202

* Stead gives following values of the Brinell scale of hardness: Purest Swedish iron, 87; rail steels, 200 to 210; Clarence pig-iron, 104 to 160; grey mottled, 153; white iron, 418; hardened steel file, 560. The Brinell testing machine is manufactured by Messrs. J. W. Jackman & Co., of London and Manchester.

The curves of Fig. 18, which are adapted from data given by Le Chatelier, show the rapidity of cooling with the different agents named on the curves. A water-spray is the most efficient. Recently Messrs. James G. Gray and A. Ross have used liquid air to produce extremely rapid cooling. The one really important thing, however, is to make certain that the steel before quenching has been heated up above Ac_3, that it is still above Ar_3 when quenching is applied, and that it is entirely cooled in the interior below Ar_{321} before quenching is stopped. *It must pass as rapidly as possible through the recalescence point*; all else is relatively unimportant. An example will suffice. Mme. Curie took a bar about 20 diameters long, of a 0·84 per cent steel, which when annealed showed feeble retention of magnetism, I_{rem} being only 85 and H_c only 8. This steel must be heated up to 730° (the value of Ac_3) to reach the transformation point; and it recalesces at 680° (which is Ar_{321}), on cooling. Now this bar was heated up to 705° and quenched in cold water, and then magnetized and tested. It was then heated up to 770° and quenched, and again magnetized and tested. In the first case it had never reached the transformation point, and was not, when being quenched at 705°, in the non-magnetic state. Whereas in the second case it had been heated up till transformed into the non-magnetic state, and was still in the non-magnetic state at 770° when quenching was applied. The following were the results :—

	$I_{rem.}$	$H_{c.}$
Annealed	85	8
Quenched, in the magnetic state, at 705° ...	130	14
Quenched, in the non-magnetic state, at 770° ...	410	52

The same bar was again heated up to 800°, and allowed to cool slowly in air until its temperature fell to 690°, when it was quenched, with the result that I_{rem} was 380, and H_c 50; little inferior to the preceding case. Here the quenching was only just at the temperature of recalescence. There could be no clearer proof of the absolute dependence of the coercive force upon the quenching being so conducted as to carry the steel quickly through the recalescence stage. Now the file-makers are quite familiar with recalescence, and know that the file will not harden properly in the quenching bath until it is first heated above the temperature of recalescence. A very good instruction for quenching magnets would be to say to the practical workman: heat and quench as you would for a file. And it is a rule with the workman that "quenching at the lowest temperature at which it will harden produces the strongest and toughest tool." * Mr. Brayshaw has re-

* B. E. Jones. See also V. A. Stobie, *Journal of the Institution of Electrical Engineers*, vol. 42, p. 675, 1909. But this is not necessarily true of high-tungsten steels.

marked that in the hardening both of tool steels and of magnet steels there is a great difference between the hardening that is *good* and the hardening which is *best*.

Here the point arises whether there is any advantage in heating up the steel, prior to quenching, to a considerably higher temperature than Ac_3. And this raises the whole question of the prior heat-treatment of the steel. In the processes, whether of rolling, drawing, or forging, by which the magnet is brought to the desired form, the steel is mechanically altered in its structure. Its crystalline grains are dragged over one another and deformed. Steel that has in this way been "worked" differs from a mere steel that has been cast; it is no longer homogeneous. Hence it is important to "normalize" the steel by a preliminary slow heating up to 850°, or 900° (or even higher), and after being kept at this height for a few minutes, cool in air to recrystallize the substance with a fine grain.

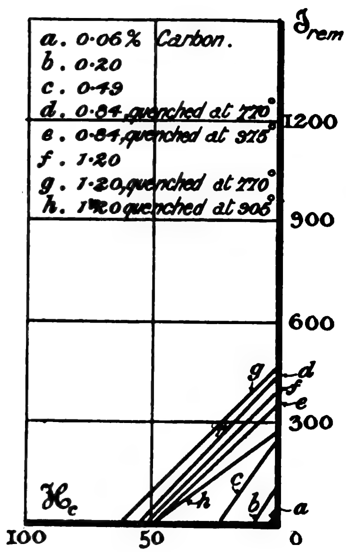

FIG. 19.—Diagram of Carbon Steels. Researches of Mme. Curie.

Mme. Curie has examined the effect on the magnetic properties of the temperature to which the hot steel is carried before being quenched. The results for carbon steels are graphically shown* in Fig. 19. It will be noticed how useless the low-carbon steels are. The 0·84 per cent carbon steel, with $\mathbf{I}_{rem}$ 426, and $\mathbf{H}_c$ 53, when quenched at 770°, showed distinctly worse properties if quenched at 975°; the remanence falling to 358 and the coercive force to 48. The best result was the 1·20 per cent carbon steel, hardened at 770°, giving a remanence of 460 and coercive force 60. But this steel when heated to 905° degenerated, giving a remanence of only 264, and a coercive force of only 48. So far as carbon steels are concerned therefore, it appears that it is a distinct disadvantage to quench at temperatures greatly above the recalescence point. Mme. Curie also tried the effect of a cyclic variation between temperatures a little above and a little below that of recalescence. Any specimen carried through such a cycle undergoes two transformations. She found that for the high-carbon steels the effect of carrying the specimen one or

* In Mme. Curie's memoir no curves are given for these steels, only statistics of numbers. In Fig. 19 these numbers are plotted on the axes, and simply joined by straight lines, without attempting to reconstruct the curves. So also in Fig. 21, to the same scale, for the magnet steels for which Mme. Curie gives the curves.

more times through such cycles is generally to improve the quality, and, in fact, may repair the deterioration produced by overheating.

Gumlich has recently shown that in carbon steels having more than about 0·6 per cent of carbon, the coercive force on hardening at 800° is greater than that after hardening at 900° or at 1100°.

Recently Messrs. J. G. Gray and A. Ross have given some data of a Whitworth tungsten steel, of composition 4·01 per cent tungsten, 0·51 carbon, 0·13 manganese, 0·19 silicon, of which a rod 111 diameters long was magnetized in a field of $H = 150$. Annealed from 900° it showed a remanence of 540 and a coercive force of 10 only. If reheated to 900° and quenched at 450°, the respective values became 570 and 7·5. But reheated and quenched at 980° the remanence rose to 610 and the coercive force to 37.

In none of the preceding examples were the specimens tempered: they were all in the hardened state as they came from the quenching bath. But since many magnet-makers temper their magnets after hardening them, some consideration of this process is necessary.

Tempering.

Tempering is a well-known term meaning softening * the steel by a more or less gentle reheating. The workmen in the steel industries

Tempering of Steels.

Deg. C.	Tint of Film.	Service.
220°	Pale straw	Short bar magnets, lancets
230°	Straw	Razors and tools for turning cast iron
240°	Yellow	Tools for turning wrought iron, pen-knives, short compass-needles
260°	Orange	Long bar magnets, cold chisels, planes, gauges, hatchets, drills
270°	Orange to purple	Chipping tools, tools for turning brass
280°	Purple	Strong springs, sword blades
300°	Blue	Compass needles, watch-springs, fret-saws
320°	Pale blue	Large saws

are accustomed to judge of the degree to which a steel has been

* "The use of the word *temper* by translators, and persons more or less remotely connected with the usages of the steel trade, to denote a change brought about by quenching from a high temperature, which every craftsman calls *hardening*, leads to confusion, and should be abandoned entirely."—H. Brearley, *The Heat Treatment of Tool Steel*, 1911, p. 9.

The following quotation from p. 26 of *The Nomenclature of Metallography*, officially issued in 1902 by the Iron and Steel Institute, is authoritative :—

"Tempering.—(Regulating; hence, in the special case of modifying the maximum hardness of steel, *rendering softer*.) The act of partially or wholly undoing what has been previously done by 'hardening.' Tempering must be preceded by 'hardening.' The term 'oil-tempering' [meaning quenching in oil] which has obtained some currency, should be 'oil-hardening,' as the result of the process referred to is to harden, whereas to temper is to withdraw or modify previously conferred hardness."

softened or let down in temper by the tint which it assumes on the surface in consequence of the formation of a film of oxide. The following table states briefly the chief services for steels of different tempers, and the temperatures to which the steel must be reheated to attain the respective tints. There is some indefiniteness about the matter, because a higher temperature applied for a short time will confer the same tint as a lower temperature for a longer time. Thus a razor-blade heated at 230° C. for 20 minutes acquires a straw tint; and a saw-blade (itself of a lower-carbon steel) if heated in a muffle at 320°, will in two or three minutes turn straw-colour; and, if the heating is prolonged, will then turn yellow, then orange, then purple, and finally blue.* If heated to 400° C. (just under a red heat) all the colour goes, the steel being then annealed soft.

The question of there being any advantage in tempering bar magnets was investigated over twenty years ago by Barus and Strouhal.† They used wires of "English silver steel," ‡ which were very carefully selected, and from which were cut a number of rods of different lengths so as to have different dimension-ratios, varying from 10 diameters long to 50, or in some cases 120 diameters long. The specimens were first heated bright red hot and quenched with cold water so as to be all glass-hard. They were then systematically tempered by heating in steam at 100° for an hour, then for 2 more, 3 more, and 4 more hours; then in aniline vapour at 185° for 20 minutes, 40 minutes, 2 hours, 4 hours, 6 hours; then in a molten lead bath at 330° for 1 minute, then for 1 hour; also in molten zinc at 420°; finally annealed by heating to visible redness and slow cooling. Between each successive stage measurements were made on their hardness and on their specific magnetism, that is, on the amount of magnetic moment per unit of mass, which they retained after being well magnetized. The table on page 109 gives a brief summary of only one series of these elaborate experiments.

* The following quaint instructions for the fashioning of compass-needles are taken from the *Magneticall Advertisements* of William Barlow, printed in 1616. "The substance in any wise ought to bee pure steele, and not iron. For most assuredly steele will take at least tenne times more vertue then iron can doe, but especially if it hath his right temper. And that is this: Heat it in the fire untill it be past red hot, that it be whitish hot, and quench it in cold water suddenly: So is it brickle in a manner as glasse it selfe, and is at that time incapable of the vertue of the *Loadstone*. Then must you, laying it upon a plaine table, warily rubbe with fine sand all the blacke cullour from it, if before you put it in the fire, you annoynt it with soape, it will scale white of it selfe, then heat a barre of iron well neare red hot, and holding one end of the needle with a small paire of tongs, lay the other end upon the hot barre, and presently you shall see that end turne from a white to a yellowish, and after to a blewish cullour, then take that end with your tongs, and doe the like unto the other, thrusting it forward upon the barre until the cullour of the whole needle become blewish: then throw it on a table, and let coole of it selfe: and so is he of the excellentest temper, and most capable to receive the greatest power from the Magnet" (p. 67).

† *The Electrical and Magnetic Properties of the Iron Carburets*, by Carl Barus and Vincent Strouhal. Washington (United States Geological Survey), 1885.

‡ A carbon steel containing from 1 to about 1·25 per cent of carbon, and very carefully melted in the crucible so as to have a fine grain with a silvery fracture. Formerly it was supposed to have a trace of silver in it; but analysis has never shown any.

In Fig. 20 the results are plotted out. One fact leaps at once into view: that, whether the steel be hard or soft, long bar magnets retain

State of Temper.	Hardness.	Mean Remanent Magnetization, I_{rem}.				
		$\delta = 10$	$\delta = 20$	$\delta = 30$	$\delta = 40$	$\delta = 50$
Glass-hard ...	45·7	188·0	300·8	348·8	372·0	386·4
Straw tint ...	26·3	172·2	321·6	396·2	430·4	452·0
Blue tint ...	20·5	154·4	366·4	536·0	643·2	698·4
Annealed soft ...	15·9	35·4	89·6	164·0	254·4	356·8

a higher magnetization than short ones. A glass-hard bar 50 diameters long retained twice as much as one 10 diameters long; and a perfectly

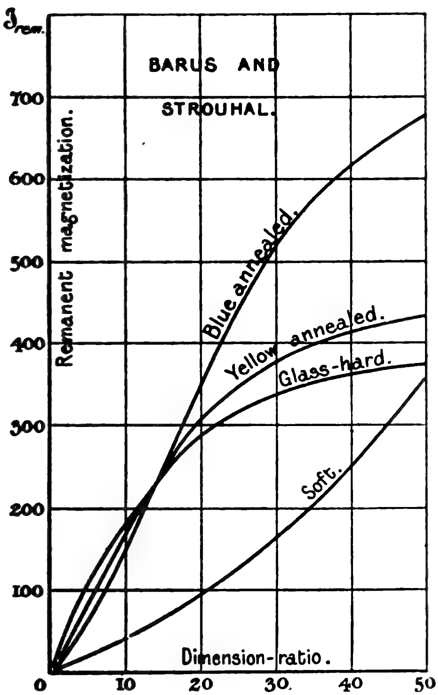

FIG. 20.—Result of Tempering Carbon Steel (Barus and Strouhal).

soft bar 50 diameters long retained ten times as much as one 10 diameters long. This table gives no values for the coercive forces, and does not state with what degree of fixidity these values were held.

Another inference from the table is that for short bars, a glass-hard state is the best, since each successive degree of tempering lowered the remanent magnetization. For the long bars, 50 diameters long, it appeared that while I_{rem} had the value 386 for the glass-hard state, the value rose to 452 in the straw-tint temper, and to 698 in the blue temper. Clearly, tempering had raised the susceptibility of the steel; but this does not prove the blue temper to be superior, since it tells us nothing about the fixidity. It is certain that in the blue temper the coercive force must have been less than in the straw temper or in the glass-hard state.

So far, as regards carbon steel. But when we turn to the alloy steels we find the facts to be no less significant. On this topic no researches are more important than those of Mme. Curie.

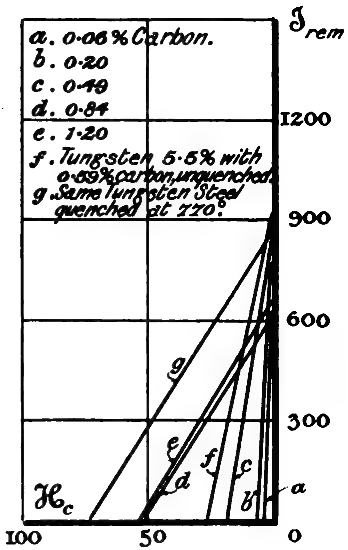

FIG. 21.—Diagram of Magnet Steels (Researches of Mme. Curie).

Fig. 21 gives the results for five carbon steels and for one quality of tungsten steel, namely, Allevard, having 5·5 per cent of tungsten, and 0·59 per cent of carbon. The two curves for high-carbon steel (0·84 and 1·2 per cent of carbon respectively) are about as good as can be found for any carbon steel, giving I_{rem} as 670 and 645, and H_c as 58 and 53 respectively. These high values relate to specimens formed as rings, for which the dimension-ratio is therefore infinite. The Allevard steel not quenched gave I_{rem} 900 and H_c only 26; but when quenched at 770° C. it gave I_{rem} 850 and H_c 73. Another ring of tungsten steel from Assailly, containing 2·7 per cent of tungsten and 0·76 of carbon, gave 800 and 69 as the corresponding values. The same steel as a bar about 20 diameters long gave only 500 and 68 as corresponding values. A very hard special Styrian steel, "Boreas steel" of Böhler Bros., containing 7·7 per cent of tungsten and 1·96 per cent of carbon, quenched at 800° C., showed a coercive force of 85, but a remanence of only 370, in a bar about 20 diameters long. This steel is a self-hardening tool steel, very difficult to work. For short bar magnets it would surpass all other tungsten steels.

Mme. Curie also examined a molybdenum steel, having 3½ to 4 per cent of molybdenum, and from 1·25 to 1·72 of carbon. That with 1·25 surpassed even Boreas steel, as it gave H_c of 85 with I_{rem} of 530 even in a bar only about 20 diameters long. The brand having

1·72 of carbon quenched at 745° gave $H_c = 73$, $I_{rem} = 465$. If quenched at higher temperatures the values of the coercive force increased slightly, while those of the remanent magnetization fell considerably.

In view of these remarkable facts, and the widespread recognition given to the value of the tungsten steels, it seems important to add an epitome of our present knowledge concerning tungsten steels in general, and their properties as dependent on heat-treatment. It appears that those tungsten steels that have been found good for magnets fall under two groups: (1) those containing from 5½ to 7 or 8 per cent of tungsten with about 0·5 per cent of carbon, and (2) those containing 2½ to 3½ per cent of tungsten with about 1 per cent of carbon. Those with the higher proportion of tungsten are of course dearer. The preference shown to high-tungsten low-carbon steels over the lower-tungsten higher-carbon steels may be due to the circumstance that the former are somewhat more easy to work. There are, of course, tungsten steels with other proportions. It will be convenient to regard those of the second group as medium tungsten steels.

The standard source of information on the composition and properties of tungsten steels is the well-known paper of Sir Robert Hadfield, in the *Journal of the Iron and Steel Institute* for 1903, pp. 14–118. From this paper it appears that metallurgists hold that tungsten does not *of itself* harden iron; but that it helps to prevent such carbon as is present from segregating (as it would in a very high-carbon steel) as graphite, or (as it would do in a moderately high-carbon steel) from separating itself as carbide of iron (cementite), and to maintain it in the condition of hardening carbon—that is, as either a solution of cementite in hardenite, or perhaps as a sub-carbide of iron. Certainly the presence of tungsten tends to prevent the formation of large crystals, and to promote the retention of a very fine grain with a silky fracture. Chromium also tends to maintain carbon in the hardening condition, and probably manganese and vanadium act similarly. All these metals tend of themselves to lower the magnetic permeability of the steel, but tungsten in a moderate percentage does so to a less extent than do chromium, nickel, or manganese. If Mushet steel is heated to about 850° and cooled slowly, a marked recalescence occurs at about 660°. High-tungsten steels, such as Mushet's, can be softened by heating to a temperature below redness and then quenching in water. Steels, generally, soften by being annealed at a temperature immediately below that of the point at which their transformation point during heating (Ac_{123}) occurs. The addition of tungsten may produce a steel which has its recalescence point actually below visible red, and such a steel is self-hardening in air without being quenched in cold water. Osmond remarks of Hadfield's tungsten steels that they show three distinct cases of heat behaviour. (1) If not heated above 850° they show cooling curves similar to those of carbon steels with the same carbon content. (2) If heated to 1040°, the higher transformation points Ar_3 and Ar_2 are not altered, but Ar_1 is lowered

(3) If heated to 1300° the transformation points Ar_3 and Ar_2 are also lowered and tend to rejoin Ar_1. There follows this significant if paradoxical result, that quenching a tungsten steel at 600° will yield soft, medium-hard, or very hard product according to the temperature of immediate previous heating. Dumas, commenting on this, suggests that prolonged heating to the higher temperatures tends to separate a double carbide of iron and tungsten, while double carbide is not dissociated at 850°. Osmond has made observations on Allevard steel, which may be summarized thus :—

Temperature of Heating.	Transition Temperature.	Condition after Quenching.
920°	695°—685°	Soft
1015	675—665	Soft
1310	505—485	Soft

Temperature of Heating	Temperature of Quenching.	Condition after Quenching.
830°	630°	Mild
1020	625	Hard but filable
1310	555	Very hard

Hadfield's paper of 1903 contains cooling curves taken by Osmond from a number of specimens of tungsten steels prepared by Hadfield. From these curves Figs. 22, 23, and 24 are selected. They relate to four alloys of the following compositions :—

Mark of Alloy.	Percentage Content.			
	Tungsten.	Carbon.	Silicon.	Manganese.
"B"	0·20	0·15	0·04	0·22
"G"	1·49	0·21	0·07	0·25
"H"	3·40	0·28	0·06	0·28
"J"	8·33	0·46	0·08	0·28

The curves are constructed as differential curves in which the abscissæ are proportional to the number of seconds taken to fall through a given range of temperature. It will be seen from Fig. 22 that the steel "B," which is a low-tungsten steel, differs in no respect from that of an ordinary carbon steel, while the curves "G" and "H," which are for medium-tungsten steels, show a lowering of Ar_3 and of Ar_1. In the high-tungsten steel "J," containing 8·33 per

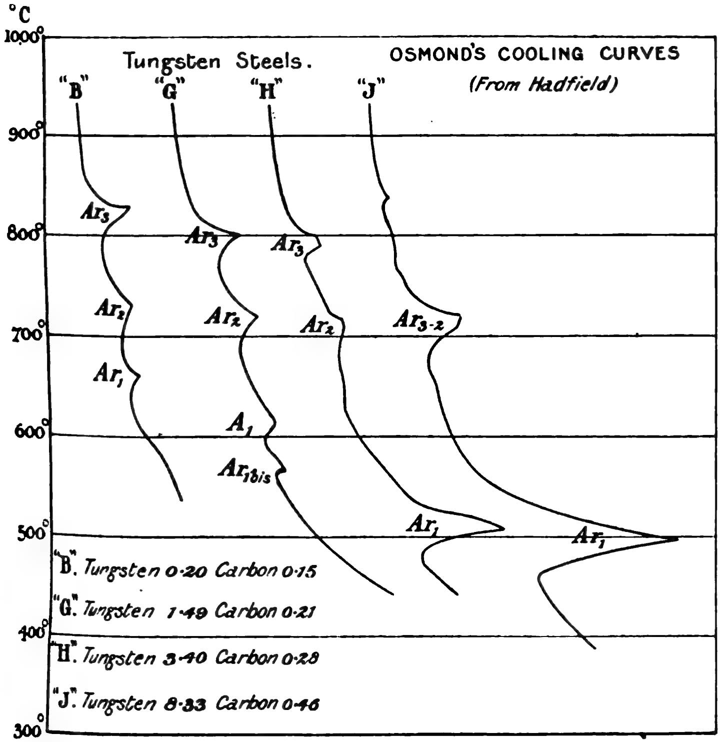

FIG. 22.

cent of tungsten, this effect is still more strongly marked. Also the medium-tungsten steel "H," Fig. 23, when not heated above 850°, shows no lowering of the transformation points, as compared with a carbon steel of equal * carbon content. But when cooled from higher temperature it shows the lowering of Ar_1 and even of Ar_2 and Ar_3.

In the case of the 8·33 tungsten steel "J," the effect of raising the

* Whether tungsten forms a double carbide in the hardening constituent is an uncertain point. Some light might be thrown on this and kindred points, if instead of stating the mere percentages present, the chemical compositions were stated in terms of the relative numbers of gram-molecules present.

temperature prior to cooling is brought out; for when not heated above 850° the recalescence point occurs at about 680°, whereas after being heated up 1040° the recalescence point is lowered to 500°, and after being heated to 1920° it is still further lowered.

From the general trend of Hadfield's observations, it would appear that the best results as regards hardening would be to choose a steel containing from 4·5 to 6·5 per cent of tungsten, from 0·4 to 1·5 per cent

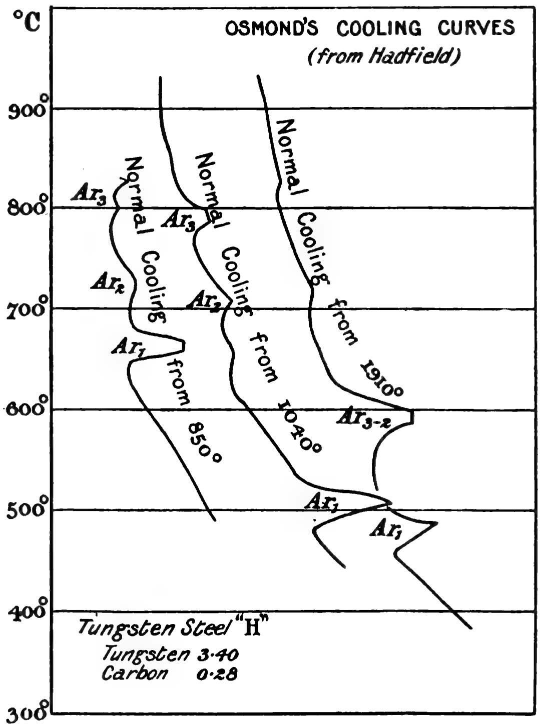

FIG. 23.

of carbon, from 0·2 to 0·4 of manganese; forge as cool as possible; heat to 1310° for a short time; allow to cool to 600°, then quench. In forging tungsten steels the usual rule is: heat to a yellow heat (about 1000° C.), and do not go on forging after the temperature has fallen to a medium red (about 760°).

In the discussion of Hadfield's results, Barrett remarks that in the steel containing 8·33 per cent of tungsten there appears a new transition point below the usual Ar_1; that this point occurs at 530° to 490°; and that it is specifically due to tungsten.

As the result of Hadfield's researches on the alloy steels (including

also the manganese steels and nickel steels), we know now of three types of material :—

1. Those for which the transformations occur at or above the temperature at which iron becomes magnetizable (soft iron and mild steels) ;
2. Those for which the transformation that confers hardness occurs at a temperature below that at which iron becomes magnetizable, but above ordinary atmospheric temperatures (high-carbon steels and the hard-alloy steels) ;

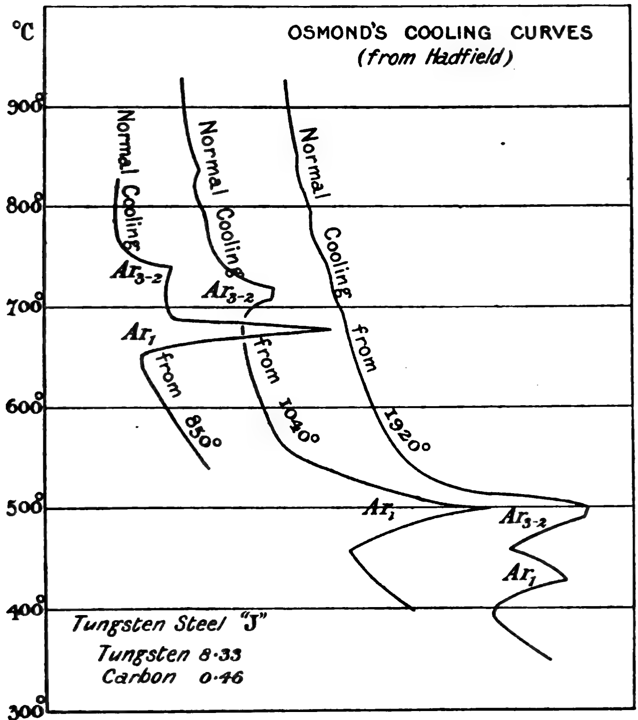

FIG. 24.

3. Those for which the transformation that confers hardness occurs below ordinary atmospheric temperatures, and which can become magnetizable (if at all) by prolonged heating and very slow cooling (Hadfield's manganese steel, and certain nickel and chrome steels).

Materials of the first type are magnetic, but are useless for permanent magnets. Materials of the third type are self-hardening, but non-magnetic. Materials of the second type are hardening, and some are self-hardening, but are magnetizable.

In a research on the stability of magnetism in 1902, Ascoli compared with Allevard steel a tungsten steel containing 4·16 per cent of tungsten and 1·15 of carbon from the steel works of Glisenti, near Brescia, and found it to be almost equally excellent, and indeed better for long magnets.

In a paper read before the Iron and Steel Institute in 1907, Mr. T. Swinden described the properties of a number of steels containing about 3 per cent of tungsten and with carbon content varying from 0·14 to 1·24. He showed that with all these steels if they are heated up to a high temperature (960° for low carbon, to 1,130° for high carbon) the temperature of recalescence is thereby lowered to about 570°.

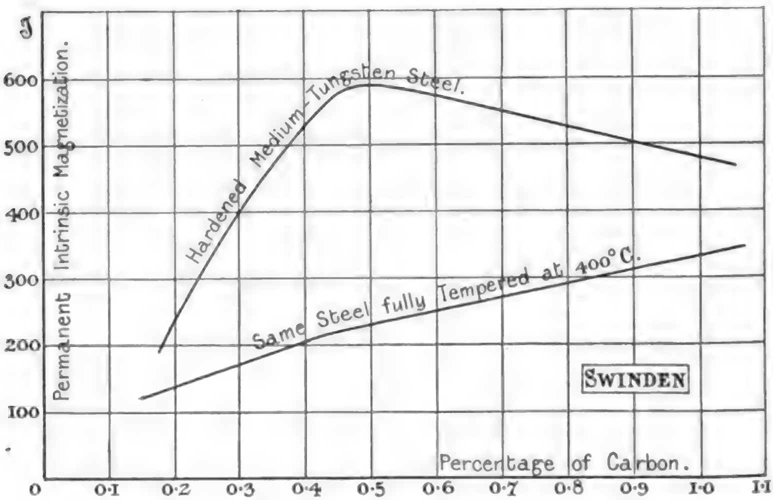

FIG. 25.—Relation between Remanence and Carbon Content in Medium-tungsten Steel.

In a second paper read to the Institution of Electrical Engineers in 1909,* Mr. T. Swinden described an investigation into the magnetic properties of this same series of medium-tungsten steels containing from 3 to 3·25 per cent of tungsten, and of carbon content from 0·144 to 1·07 per cent. He used bars having a dimension-ratio of 32. His conclusions are therefore not directly applicable to high-tungsten steels or to magnets having other dimension-ratios. His specimens were treated as follows: They were normalized by heating to 950° for 15 minutes, and cooled off in air. To harden them they were heated to 900° (changing the excess cementite cell-walls to mere specks) and then cooled to 810°, or 780°, or 760°, or 740°, according to the carbon content, and then quenched. They were tempered by three successive stages: (1) at from 60° to 75° for 14 hours; (2) at from 80° to 85° for 13 more

* *Journal of the Institution of Electrical Engineers*, vol. 42, p. 641, 1909.

hours; (3) at 400° for 1 hour. At each stage of the process they were examined magnetically to ascertain the value of the remanence and of the coercive force. Fig. 25 shows the variation of the remanence with the carbon content, and Fig. 26 the variation of the coercive force with the carbon content,* both diagrams relating to the hardened and to the tempered steels. It will be seen that in these 3 per cent tungsten steels tempering is useless. Also the maximum remanence occurred when about 0·45 per cent of carbon was present; but the coercive force showed an increase up to the highest proportion of carbon. Had bars of greater dimension-ratio been used, for which the self-demagnetizing factor is smaller, the remanence curve for the lower

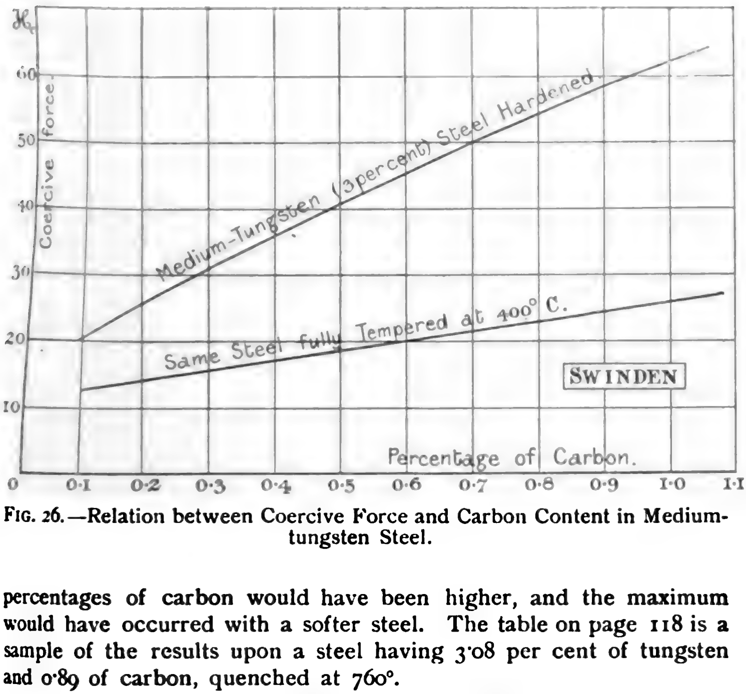

FIG. 26.—Relation between Coercive Force and Carbon Content in Medium-tungsten Steel.

percentages of carbon would have been higher, and the maximum would have occurred with a softer steel. The table on page 118 is a sample of the results upon a steel having 3·08 per cent of tungsten and 0·89 of carbon, quenched at 760°.

In 1910 Mr. Shipley N. Brayshaw brought before the Institution of Mechanical Engineers a research on the hardening of carbon and low-tungsten tool steels. Though this paper deals only slightly with magnetic matters, it is of great importance from the thoroughness with which it handles the heat-treatment and mechanical properties of these steels. Two qualities only of steel were examined, namely, such as are used for making milling cutters: (1) a carbon steel containing from 1·14 to 1·16 per cent of carbon, with 0·37 to 0·4 of manganese; (2) a

* Fig. 26 should be compared with Fig. 3, the corresponding diagram for pure carbon steels.

tungsten steel containing 0·47 to 0·57 of tungsten, with 1·15 to 1·16 of carbon and 0·42 to 0·57 of manganese. This low-tungsten steel recalesces in the range from 731° to 725°. The recalescence point is retarded, that is, lowered, if the steel is previously heated to 890°. The hardening point (Ac_{123}) is definite within six degrees. Brayshaw employs a bath of molten mixed chlorides of sodium and potassium in which to "soak" the steel before quenching. If heated above the recalescence point, and then soaked at a temperature between *Ac* and *Ar*, and then quenched, it is softer than if so soaked without previous heating. If raised more than twenty or thirty degrees above the recalescence point, and then quenched, the hardness is less than if quenched without being heated so high. Identical results are produced on quenching in oil from temperatures anywhere between 760° or to 940°. Heating for a short time to 810° or to 880° before quenching produces great hardness, but 30 minutes' soaking at such temperatures impairs the steel. Soaking for even 7½ minutes at 880° impairs the steel. Quenching in brine at 5° is perceptibly better than quenching in brine at 24°. The ill effect of heating for 30 minutes at 840° to

	Remanence.	Coercive Force.
Quenched at 760°	584	58·8
Reheated 60–75°, 14 hours ...	583	58·6
Reheated 80–85°, 13 hours ...	582	57
Annealed 400°, 1 hour	1159	21·7

900° is scarcely improved by soaking for 30 minutes at 760°. A good result is obtained by a short soaking at 880°, followed by a lowered temperature; and best when this lower temperature is near or a little above the hardening point. Thus on a steel with 0·5 per cent tungsten, 1·15 of carbon, 0·31 manganese, and 0·21 silicon, a 10 minutes' heating at 893° followed by 30 minutes at 731° yielded a hardness of 600 (Brinell's scale), while if the lower temperature was prolonged for 120 minutes the hardness fell to 418, and after 240 minutes to 321.

Holborn, in 1891, examined several sorts of tungsten steel to ascertain what was the most favourable temperature to which they should be heated for quenching. For Böhler's steel with 2·8 per cent of tungsten and 1·05 carbon, and for Seebohm's steel with 2·16 tungsten and 1·14 carbon, he found (for rods of dimension-ratio 11) the best temperature to be 850°. With higher temperatures the remanence was less.

At the present day tungsten steels are used by almost all magnet-makers. Messrs. H. Shaw & Sons, of Sheffield, the old-established

firm which for three generations has made most of the horse-shoe magnets sold by cutlers, and those used by potters, as well as magnets for compasses, still use plain carbon steel (0·64 per cent of carbon) for cheap commercial magnets, on account of the price; but for higher grade magnets they use tungsten steels or other steels of special composition. In France, Allevard steel and Marchal steel are almost exclusively employed. In Germany, Remy steel is undoubtedly the most generally employed. Large quantities of tungsten steel for magnets are every year exported from Sheffield and from Germany into the United States. Tungsten steel is invariably used in the magneto-generators employed for ignition in automobiles. Professor W. Brown examined chromium steels containing from 0·75 to 9·5 per cent of chromium. They have high coercive forces; but there seems no advantage in adding more than 2½ or 3 per cent of chromium.

A few remarks may be added concerning modern self-hardening high-speed tool steels; for though their magnetic properties are as yet mostly unexplored, these brands can now be obtained commercially; and if they can be brought by quenching into the magnetizable state, their extreme hardness would suggest their being possessed of very great coercive force. The Taylor-White high-speed steel, used for tools for turning hard steel, has the composition of tungsten 8·5 per cent, carbon 1·25, and chromium 4 per cent. Another high-speed steel which works well as a tool, even when red-hot, contains tungsten 5, molybdenum 4, and chromium 4 per cent. Other self-hardening steels, according to Brearley, contain very varied proportions; tungsten from 2·44 to 24 per cent, carbon 0·4 to 2·19, chromium 0 to 6, silicon 0·21 to 3; manganese being sometimes used instead of chromium. There is a tendency to use more tungsten and less carbon than formerly; a much-used proportion being 12 tungsten with 0·8 of carbon. Vanadium steel with 10 per cent of vanadium and 1·1 of carbon, quenched at white heat, is still harder.

We have seen that some kinds of steel will give a remanence of as much as 800 or 900, with various values of the coercive force up to 50, 60, 70, or even 73 (see table on p. 85). We have also seen that some other kinds of steel will give coercive forces of 70 or 75, even up to 80; but these generally have values of the remanence (even in cases of zero demagnetization coefficient) under 700. The ideal sought for at the present time is a steel of such composition that, when properly treated, it shall have a remanence of 800 and a coercive force of 80. *No such steel has yet been produced;* but assuredly it is not unattainable. And with the great modern advance in metallurgical knowledge, it is not beyond the bounds of hope that some day a steel may be produced with a remanence of 1000 and a coercive force of 100. Researches on the alloy steels from this point of view are much needed.

Magnetization.

All the old processes of single touch and double touch may be at once set aside in favour of the modern method of using an electric

current. In the case of bar magnets they are magnetized by putting them inside a long magnetizing spiral at least twice as long as themselves, or for short bars by placing them between the poles of a suitable electromagnet. In the case of horse-shoes they are magnetized by placing their poles in contact with the poles of a suitable electromagnet. In either case the field to which they should be subjected should not be less than $H = 250$. And, since values twice as great can readily be reached with a magnetizing spiral (requiring, in fact, a circulation of about 1000 ampere-turns per inch length of the tubular coil), this is a simple matter. For horse-shoes, if the electromagnet is one in which there can be a total number of ampere-turns not less than 500 times the number of inches of length along the steel of the magnet, it will suffice, though a higher number has some advantage. For ordinary steels it makes little difference either to the remanence or to the coercive force whether the value of the field to which it is subjected be $H = 100$ or $H = 1000$. For hard tungsten steels it is advisable to use the strongest fields available. To produce a field of $H = 1$ along 1-in. length of air requires a circulation of 2·02 ampere-turns. To produce a field of $H = 1000$ requires, therefore, 2020 ampere-turns per inch. If a horse-shoe having a total length of 10 in. of steel is to be subjected to a magnetizing force equivalent to $H = 500$, the electromagnet against which its poles are placed should be excited by at least 10000 ampere-turns; or if its coil is of a wire that will carry 10 amperes, it should have at least 1000 turns—a requirement easily fulfilled. Little depends on the duration of the operation. Lord Rayleigh has shown that the resulting magnetism depends on the maximum value, not on the duration of the applied field. When a very strong magnetizing current is applied only for a moment, the operation is called "flashing." There is a slight advantage in repeating the magnetization a few times by turning off the current, and turning it on again. There is a slight advantage in subjecting the magnet to mechanical agitation, by percussion, during the application of the magnetizing force. There is also a slight advantage in not turning off the current too suddenly, since any electric oscillations produced at the break of the circuit are deleterious. It has at various times been suggested to magnetize the magnet while hot, and to keep it under the influence of the magnetizing forces while it cools. Any advantage gained by this awkward process may be better reached by the simpler way of using when cold a somewhat more powerful electromagnet.

One special arrangement employed at the Thomson-Houston works for magnetizing the magnets used in electricity meters is worthy of mention. These magnets are of a horse-shoe shape with the flat limbs brought near to one another in parallel planes with a narrow gap between them (see Fig. 8). When such magnets are applied to the poles of the magnetizing electromagnet (the poles of which are necessarily very close together), there is a considerable tendency to magnetic leakage across the gap. In order to oppose such leakage and drive the

magnetic flux round the bend of the steel, a rapidly revolving copper disc is lowered into the gap during the application of the magnetizing forces; the eddy-currents induced in the disc tending to oppose any flux through it.

Maturing of Permanent Magnets.

It is a commonplace that ordinary so-called permanent magnets are apt in the course of time to lose a part of their magnetism and to become weaker. I have already alluded to the deleterious effect of slamming on the "keeper." But there are several causes which contribute to the decay of the magnetism of magnets. Mechanical shock, changes of temperature, contact with other magnets or with iron, exposure to demagnetizing forces, are amongst these. Apparently the mere lapse of time effects a deterioration. Put a newly made and newly magnetized magnet, after measuring its strength on a magnetometer, on a shelf in a dry, cool cellar by itself, and you will find if you measure it again after a few weeks or months that its magnetism will have diminished.* Put it in a window where the sun can shine on it by day, and the periodic warming and cooling of day and night will cause its magnetism to decay more rapidly. Long bars and horse-shoes and other nearly closed forms are found to deteriorate more slowly than short bars; doubtless because their self-demagnetizing coefficients are less. But in all magnets there seems to be a definite law of decay, by virtue of which there appears to be a limit down to which each tends. Briefly, in every newly made magnet, the so-called permanent magnetism appears to consist of two parts, a removable or sub-permanent part which slowly decays, and a really permanent part. Mechanical shocks and charges of temperature promote the disappearance of the removable part; for an old and well-used magnet, though weaker than it was when new, is, as Lamont found,† much more constant in its magnetic power than a new one. The law of decay is illustrated by Fig. 27, where the heights of the ordinates represent the strengths of the magnet, diminishing as time goes on. The height T represents the temporary or removable part of the remanent magnetism, and the height P the really permanent part. The time taken to settle down to constancy, and the proportion that is really permanent, vary vastly with the quality of the steel, and with the dimension-ratio; for in short bars there is always a self-demagnetizing force, proportional to the magnetism present, tending to produce decay, and the soft steels with small coercive force must go on losing their strength until the self-demagnetizing force is so weak that the coercive force is able to

* Newly hardened steel appears to undergo a slow change for months or years. Brant, in 1909, measured the hardness of 20 rods of Stubbs's steel, which had been made glass-hard by Barus in 1885, and found (using Barus's electrical method of measuring hardness) that they had in twenty-four years lost nearly 20 per cent of their hardness. Other materials show secular changes in their properties. Every thermometer-maker knows how glass after having been melted goes on slightly contracting for many months.

† Lamont found that a new bar magnet lost from 15 to 35 per cent of its strength on being dropped on a hard floor, while an old one lost less than 1 per cent.

cope with it. Assuming that no specially deteriorating actions, such as violent shocks or considerable changes of temperature, etc., are present, the progress of the decrement in strength ought to be logarithmic; hence the curve of Fig. 27 ought to be represented by the equation—

$$y_t = P + T\epsilon^{-at}$$

where y_t represents the ordinate at any time t, and where a is the coefficient of logarithmic decrement. This formula was proposed by Hansteen.

Those who are concerned with measurements of terrestrial magnetism have long been aware of the decay of strength of the thin bar magnets used as standards. Measurements of the progressive loss of power of bar magnets have been made by many observers—Lamont, Airy, S. H. Christie, Cancani, Loomis, and others; and the temperature

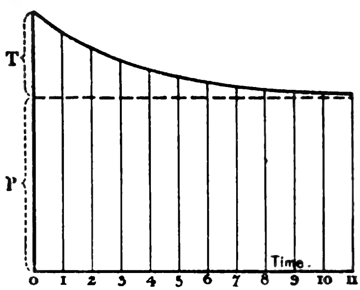

FIG. 27.—Curve of Decay of Magnetism.

coefficients connecting the variations of strength with rise and fall of daily temperature have often been determined.

Lamont, using thin carbon-steel rods (knitting-needle steel) for terrestrial magnetic measurements, found the decay to be greater in summer then in winter. The following are his data for the monthly losses, taking the initial magnetism as unity :—

	1848.	1849.
January	0·0000	0·0000
February	0·0003	0·0001
March	0·0003	0·0002
April	0·0008	0·0005
May	0·0014	0·0007
June	0·0022	0·0011
July	0·0028	0·0016
August	0·0032	0·0022
September	0·0028	0·0022
October	0·0017	0·0013
November	0·0009	0·0007
December	0·0005	0·0001
Total loss in year ...	0·0169	0·0107

Barrett, Brown, and Hadfield examined a number of alloy steels in the glass-hard stage, as to the amount of loss in six months from their being magnetized in a field of only $H = 45$. They were bars with dimension-ratio of 33. A bar of Swedish charcoal iron of about the same dimensions was added for comparison, with the following results:—

Material.	Chemical Composition.					H_c.	Percentage Loss in Six Months.
	C.	Mn.	Si.	W.	Cr.		
Swedish charcoal iron	0·028	—	0·07	—	—	1·10	40·0
Silicon steel, mild ...	0·220	0·18	0·44	—	—	0·80	11·0
Medium-tungsten steel	0·280	0·28	—	3·50	—	5·70	6·6
High-tungsten steel ...	0·760	0·28	—	15·50	—	12·90	2·7
Tungsten chrome steel	0·250	—	—	2·00	0·75	5·30	14·0
Chrome steel	0·430	—	—	—	3·25	—	3·6
High-chrome steel ...	1·090	—	—	—	9·50	—	10·4

Bosanquet in 1885 made a number of bar magnets of "best cast steel" and hardened them to the utmost. He measured their magnetic moments for several months and found the following average values:—

February 18th	12,039
March 3rd	11,822
March 15th	11,767
April 8th	11,620
September 18th	11,119

Rise of temperature was found by Canton and other observers to produce a temporary fall in the remanence, and fall of temperature to produce a rise; but the percentage change per degree of rise and fall varied greatly, as measured by different observers. If we write the equation in the usual form—

$$M_1 = M_0 \left\{ 1 + a(t_1 - t_0) \right\}$$

where M_0 is the magnetic moment at the initial temperature t_0, and M_1 that at t_1° C., and a is the coefficient of change per degree; then a was found by different authorities, for such variations as occur in daily or yearly temperature ranges, to have values varying from $-0·000044$ to as much as $-0·00112$. Wiedemann found that if a bar was strongly magnetized, and then partially demagnetized (by an opposite magnetizing force), it would, on being heated, lose some magnetism if the

previous reduction had been small, but would gain some magnetism if the previous reduction had been large. Also, that if a newly made magnet is repeatedly heated and cooled, the magnetization lost at each heating is only partially regained on cooling, causing a progressive loss, until finally a constant state is reached in which the magnetization lost on being heated is restored on cooling. Old magnets are found to be much more constant in this respect. These facts have led to the idea of *maturing* newly made magnets by subjecting them to processes imitating those that go on naturally, with the intent of bringing the magnets rapidly into a state of constancy. Such processes are: (1) repeated gentle heating and cooling, (2) protracted

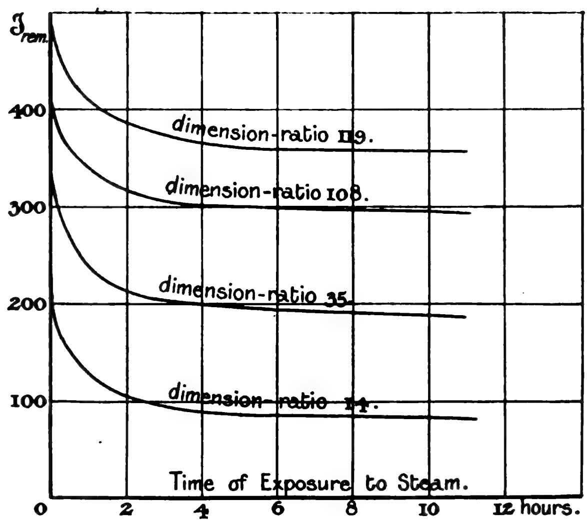

FIG. 28.—Effect of Prolonged Heating to 100° C. (Barus and Strouhal).

gentle heating, (3) repeated subjection to mechanical shock, and (4) partial demagnetization.

We owe to Barus and Strouhal a valuable examination of the beneficial effects of protracted gentle heating. Fig. 28 depicts the results they found on subjecting their glass-hard bar magnets of Stubbs's steel (a carbon steel) to heating by steam at 100° C. for a period of 12 hours. It will be seen that the values of the remanence fell in a regular way, assuming a constant value after some 10 hours of steaming. The final as well as the initial values of the remanence varied with the dimension-ratio. The remanence of that one which was 119 diameters long fell from 488 to a constant value of about 360. Unfortunately, no measurements were made of any change in the coercive force. But measurements of the hardness (by specific

electric resistance) showed that this also was reduced *pari passu* to a constant value about 83 per cent of the initial hardness. Barus and Strouhal also observed that the final remanence is the same whether the steaming takes place before magnetization, or whether the rod is first magnetized, then steamed, then remagnetized. In any case, long thin magnets lose a less percentage of their magnetism by being thus steamed than do short ones. In a special research on hardness, Barus found that glass-hard rods of Stubbs's steel, 30 diameters long, lost about 10 per cent of hardness in three years by natural decay, an amount which could have been produced in about three hours by immersion in steam at 100° C. He concluded that mean atmospheric temperature acting on quenched steel for a period of *years* produces a diminution of hardness about equal to that of hot steam acting for a period of *hours*.

To test the effect of this maturing by steam-heating, Barus and Strouhal exposed a glass-hard magnet to 60 hours' steaming, remagnetized it, and steamed it again for 15 hours. Its remanence, originally 320, was reduced thus to a constant value of 258. It was then dropped from heights varying from 1·5 to 0·5 metre on a wooden block, in various ways, and measurements were made of its strength after each treatment. The magnetism did not vary more than 0·54 per cent after any such rough usage.

Mme. Curie examined the effect of reheating on various steels (see p. 110). She found that protracted exposure to steam heat diminished both the coercive force and the remanence. Thus, 10 hours at 100° reduced the coercive force of a 0·84 per cent carbon steel from 51 to 48, and that of Boreas steel from 85 to 78. Reheating to 200° was disastrous, as the following table shows, for three kinds of hard steel.

Square Steel Bars of 20 cm. Length and 1 cm. Side ($\delta = 23$).	Carbon Steel, 0·84 per cent.		Allevard Steel.		Molybdenum Steel.	
	H_c.	$I_{rem.}$	H_c.	$I_{rem.}$	H_c.	$I_{rem.}$
Hardened state	51	422	69	574	79	429
Heated 3 hours at 100° ...	45	415	65	583	73	433
Heated 10 hours at 100° ...	45	396	63	549	71	418
Heated 16 hours at 100° ...	44	397	62	544	70	416
Heated 24 hours at 100° ...	44	390	61	540	70	413
Heated 8½ hours more at 150°	37	363	46	465	50	417
Heated 7½ hours more at 200°	28	310	37	401	40	382
Total loss per cent at 100°	13	8	12	6	12	4
Total loss per cent at 200°	41	27	47	33	50	11

Three other bars were treated at 60° C., with the following results:—

	Allevard Steel.		Boreas Steel.		Molybdenum Steel.	
	H_c.	$I_{rem.}$	H_c.	$I_{rem.}$	H_c.	$I_{rem.}$
Hardened state	70·7	561	79·8	336	82·5	496
Heated 7 hours at 60° ...	69·7	572	78·3	340	79·7	494
Heated 42 hours at 60° ...	69·9	564	79·0	339	79·8	490

In 1887, W. Brown examined the effects of percussion on a number of silver-steel rods, all 10 centimetres long, which after having been made and magnetized were laid aside for different periods, and then examined as to the percentage loss in magnetism that they showed when suddenly dropped from a height. The following table gives the results:—

No.	State.	Diameter.	Dimension-ratio.	$I_{rem.}$	Percentage Loss due to Percussion, after laying aside for					Total Percentage Loss.
					1 hour.	20 hours.	44 hours.	1 month.	3 months.	
1	Glass-hard ...	0·3	33	324	1·98	2·0	1·95	1·04	0·8	7·77
2	Glass-hard ...	0·2	50	355	2·96	3·2	1·48	1·00	0·0	8·64
3	Tempered yellow	0·3	33	348	6·03	6·1	4·80	5·40	6·2	28·53
4	Tempered yellow	0·2	50	364	4·00	3·5	3·76	2·60	4·0	17·86
5	Tempered blue	0·3	33	422	11·80	10·8	9·71	11·80	7·5	50·60
6	Tempered blue	0·2	50	563	8·20	8·2	8·18	7·50	8·7	40·78

The superior resisting power of the glass-hard magnets is very marked. The superiority of the bars of higher dimension-ratio to those of lower ratio is also evident; the exception in the case of bar No. 2 is probably accidental. Brown finds cylindrical bars with rounded ends less sensitive to percussion than either flat-ended cylinders or ellipsoids of same length and girth. Brown also finds for glass-hard rods of Stubbs's steel that if of 1·6 millimetres diameter there is no advantage as to remanence in making them more than 80 diameters long; and if 10 millimetres diameter, no advantage in making them more than 8·1 diameters long. This result is difficult to understand.

The conclusion is that, in maturing by reheating, both the remanence and the coercive force are diminished for all kinds of steel, but that by prolonged reheating to 60° a constant state is reached, with a reduction, in the case of the best steels, of from 1 to 3 per cent only.

Mme. Curie also investigated the effect of maturing on the liability to loss by shocks. A number of similar bars were dropped, end-on, from a height of 85 cm., upon a flag-stone, and then dropped, broadside-on, from a height of 30 cm., upon the same stone. After each repetition of this process the magnetism was measured, and the process was repeated till a constant value was obtained. Bars of soft steel lost from 45 to 83 per cent of their remanence. For the others the following table gives the chief results :—

Bars of Dimension-ratio $\delta = 23$.	Bars in Hardened State.				Bars matured at 100° C.		
	H_c.	$I_{rem.}$	No. of Falls.	Loss per Cent.	H_c	No. of Falls.	Loss per Cent.
Carbon steel (0·5)	23	210	30	0·23	22	20	20
Allevard steel ...	73	560	50	0·50	68	100	7
Boreas steel ...	86	390	5	1·50	—	—	—
Molybdenum steel	73	448	10	2·90	69	30	3

The effect was then tried of subjecting some of the same bars (dimension-ratio 23) to a partial demagnetization to mature them, and they were then tested by shocks of the same sort as before. The values of I_{rem} are given here for two bars :—

	$I_{rem.}$
(1) *Carbon Steel* (0·5 per cent carbon).	
Bar magnetized to saturation	200·0
After many shocks	152·0
Remagnetized, then demagnetized in field $H = 8·5$	150·0
After many shocks	Lower
Remagnetized, then demagnetized in field $H = 13·8$	118·4
After 4 days had spontaneously increased to ...	123·0
After many shocks, remained constant	123·0
(2) *Allevard Steel.*	
Bar magnetized to saturation	676.0
After many shocks	658·0
Remagnetized, then demagnetized in field $H = 6·4$	663·0
After many shocks	636·0
Remagnetized, then demagnetized in field $H = 14$	632·0
After many shocks, constant at	632·0

It appears that a reduction of about 6 per cent rendered the Allevard bar insensible to shocks, while the carbon-steel bar had to be reduced by about 40 per cent to attain constancy. Mme. Curie found that for all such bars, if of really hard steel, a reduction of 10 per cent sufficed to render them immune from shock. For longer bars or for more nearly closed circuits the reduction necessary would of course be less. Moreover, magnets so reduced by about 10 per cent are immune from loss by changes (over a limited range) of temperature. A bar of Allevard steel, hardened and matured at 60°, and then fully magnetized, will lose a little if again heated to 60° and cooled. But if its magnetization be reduced by about 10 per cent it will be immune from loss by a subsequent heating to 60° and cooling. For bars so treated the temperature coefficient is about —0·0002 for Allevard steel and —0·0003 for molybdenum steel.

Mme. Curie also found that the intensities of the fields required in these bars to reduce their magnetization by 10 per cent was :—

	H_c.	Field needed.
Carbon steel (0·5 per cent carbon)	21	3·5
Allevard steel, hardened, and matured at 60°	71	13·0
Boreas steel, hardened, and matured at 60° ...	78	27·5
Molybdenum steel, hardened, and matured at 60°	80	21·0

In a series of memoirs Klemenčič has carried inquiry a little further. He used tungsten-steel bars of Böhler & Co. of five different brands marked "O," "OO," "UI," "43," and "45." They were magnetized in a field of H = 865, produced by a magnetizing current that was several times reversed and suddenly switched off. He investigated the relation between the dimension-ratio and the temperature coefficient; the figures being deduced from three series of his observations (see table on page 129).

He concluded that the temperature-coefficient varied inversely as the dimension-ratio. If the magnetizing current was gradually reduced instead of being abruptly broken, the remanent magnetism was found to be slightly higher, the augmentation being from 0·1 per cent for long thin rods, to as much as 5 per cent for short thick ones. The difference was ascribed to the action of eddy-currents in the steel.

Another point tested was the decay of the remanence with time. The percentage loss was found to depend on the dimension-ratio.

An average of the various brands of steel showed that glass-hard magnets with dimension-ratio 10 lost from 3 to 4·5 per cent in 15 months, while those with dimension-ratio 25 lost only from 1·25 to 1·98 per cent in the same time. Another set of bars of dimension-ratio 25 were matured in steam for 12 hours. After a year they had lost from 0·12 to 0·25 per cent of their magnetism, of which the greater part occurred within a day or two of their being made. In the following 11 months the percentage losses were from 0·025 to 0·0134 only.

Another point investigated was the influence of allowing time to elapse between hardening and magnetizing. A number of silver-steel rods were hardened, and then, after lapse of time varying from 0·6 of a minute to an hour, were magnetized. Their magnetic moments were

Dimension-ratio.	Species of Steel.		
	"45."	"UI."	"OO."
10	0·000445	0·000480	0·000281
15	0·000300	0·000345	0·000186
20	0·000215	0·000265	0·000139
25	0·000172	0·000215	0·000145
38	0·000134	0·000139	0·000139

then measured at regular intervals of time, and the decay was observed. It was found that the regular fall in their strengths was practically independent of the time when they were magnetized, and depended only on the time that had elapsed since they were hardened. This proves that there is a sort of self-annealing going on in the newly hardened steel, and that this molecular settling down of the material is scarcely influenced by the process of magnetizing. Further, though the percentage decrease in magnetization is practically the same for all rods, if reckoned at the same length of time from the hardening, yet the absolute value of the remanence (at the same lapse of time since hardening) is less the earlier the rod was magnetized after hardening. It is good to let the steel settle down before magnetizing. The settling down is hastened by boiling at 100°. The conclusion is that the secular changes—at any rate in magnets of the older carbon-steel kinds—are sequelæ of the hardening process and are not sequelæ of the magnetizing process.

Krüse, continuing the researches of Klemenčič on Böhler's steels, examined their behaviour when subjected to shock, and to the effects

of bringing them into contact with masses of iron from which they were suddenly pulled away. The magnets were matured on the plan of Barus and Strouhal, and were then caused to fall on a marble slab 20 times from a height of 1 m., and then 20 times from a height of 1·94 m. The following were the results :—

Brand of Steel.	"45."	"43."	"UI."	"OO."	"O."
Coercive force	57·0	59·0	63·0	76·0	84·0
Percentage loss when $\delta = 10$	6·7	8·2	5·9	4·8	4·4
Percentage loss when $\delta = 25$	6·7	5·9	5·5	5·4	3·5

It will be seen that the percentage loss was least for the brands which had the greatest coercive force.

Krüse also investigated the effect of placing cylinders or plates of soft iron in contact with the ends of his matured bar magnets and then suddenly pulling them off. This is a matter which the author investigated * for horse-shoe magnets over twenty years ago, and found that while a magnet was weakened by slamming the keeper on and gently sliding it off, it was strengthened by sliding it on and suddenly detaching it. Krüse found by a repeated detachment a decrease of about 0·1 per cent for magnets of dimension-ratio 25 and of 0·3 per cent for those of dimension-ratio 10. He does not say whether the iron was slammed on or not. Newly magnetized and matured magnets showed an increase of about 0·5 per cent. Drawing the magnets over the face of an iron plate weakened the magnets by about 20 per cent for those of $\delta = 25$, and by about 28 per cent for those of $\delta = 10$.

Thomas Gray, in 1885, examined the percentage changes of magnetization produced in bar magnets of silver-steel that were produced by placing them in a weak demagnetizing field of value $\mathbf{H} = -1$. The reduction so effected varied from 0·8 per cent for bars of a dimension-ratio of 10, to 0·43 per cent for those of a dimension-ratio of 100.

It is much to be desired that some investigator would make an examination of tungsten steel and other alloy steels, as thorough as that of Barus and Strouhal on carbon steel, as to the effects of maturing, by heating and partial demagnetization, in rendering them immune from change by shock or temperature.

Ascoli compared Allevard steel with Glisenti steel (see page 116) in respect of their ability to withstand shock. He also determined the percentages to which the respective magnetizations must be reduced to secure final stability of the permanent residuum. The

* Silvanus P. Thompson, *The Electromagnet* (1892), pp. 213 and 408.

results are embodied in the following table. The magnetizing fields were $H = 100$ or a little more.

Dimension-ratio.	Percentage Loss by Shock.		Percentage Reduction for Stability.	
	Allevard.	Glisenti.	Allevard.	Glisenti.
10	29·2	30·3	18·6	22·4
15	16·5	17·0	21·6	22·2
20	10·9	11·1	19·4	22·6
30	5·7	5·9	18·6	20·5
40	4·1	4·0	—	—
50	3·5	3·2	12·4	16·6
100	2·6	2·3	9·4	10·3
∞	2·4	2·1	8·2	8·7

It will be seen that while Glisenti steel lost less by shock than Allevard for long magnets, it lost more for short ones, and required a greater reduction to become stable.

Ashworth has studied the temperature coefficient, and the coefficient of permanent loss which a magnet undergoes by being heated (once) from room temperature to 100° C. and again cooled to room temperature. His results relate chiefly to pianoforte wires of drawn steel of an unknown composition. He found drawn steel to be in a peculiar molecular condition, such that for $\delta = 109$ its temperature coefficient is positive until it has been once heated red-hot, after which the coefficient is like that of other steels, negative. For a dimension-ratio of 43 such a drawn wire had zero coefficient. He found hard steels to have the lowest temperature coefficients. In the following table I_i represents

Material.	δ.	I_i.	I_f.	α.	β.
"Magnet steel" (carbon), hardened ...	8·0	53·2	40·6	—0·001370	—0·270
Mushet steel, hardened	12·0	185·9	132·6	—0·000690	—0·287
" " "	17·0	215·2	177·8	—0·000970	—0·174
Cast iron, hardened ...	11·8	193·1	167·8	—0·000180	—0·131
" " ...	13·1	210·9	180·1	—0·000160	—0·141
Piano wire, glass-hardened...	16·0	252·0	216·0	—0·000603	—0·150
" "	48·0	551·0	520·0	—0·000228	—0·090
" "	80·0	570·0	580·0	—0·000097	—0·030
" "	96·0	556·0	569·0	—0·000055	—0·020

the initial remanence, and I_f the final remanence after being raised from 14° C. to 100° C. and again cooled to 14° C. The symbols α and β are respectively the ordinary temperature coefficient (according

to Whipple -0.00029 in the average), and β the coefficient of the irreversible or permanent loss.

Doubtless the values of β would have been greatly reduced had the magnets been properly matured. The dependence of these coefficients on the dimension-ratio is well illustrated by the data on glass-hardened piano wire.

Cancani found bars of "English steel," which were about 11 diameters long, to possess a temperature coefficient, when glass-hard, of as low a value as -0.000436, but when annealed soft, of as high a value as -0.002635. When such glass-hard bars were tempered by regular stages from pale straw (at 221°) to a pale green (at 332°) the temperature coefficient rose in regular sequence from -0.00135 to -0.00179.

Durward has examined matured bar magnets of various kinds of steel, and has shown that in "crescent" drill rod (a tungsten steel), matured by Barus's process, the coefficient of loss β varied from about -0.0012 for a diameter-ratio of 5, to -0.0006 for $\delta = 10$, -0.0003 for $\delta = 20$, and -0.00025 for $\delta = 32$.

Cast-iron Magnets.

At various times cast-iron has been proposed as a material for permanent magnets, and B. O. Peirce, in 1905, showed that chilled cast iron, though inferior to tungsten steel, yet would make good magnets. Ashworth (see p. 131) had shown that its temperature coefficient is low. Campbell, in 1906, showed that if quenched at about 1000° C. —a difficult operation because of the brittleness of the material—its coercive force is quite equal to that of the carbon steels; and he found values of the remanence from 200 to 229, and coercive forces from 48·9 to 52·8.

Practical Processes for Producing the Best Permanent Magnets.

We may sum up the results in a few paragraphs. To produce permanent magnets, which are both powerful and constant in their power, a tungsten steel should be used having from 5 to 8 per cent of tungsten, and from 0·4 to 0·6 of carbon. Chromium up to 2 or 2·5 per cent may be present, but the presence of the following elements should be avoided: manganese, titanium, copper, sulphur, and phosphorus.

For bar magnets there is an advantage in having the dimension-ratio as large as possible, as this gives not only a higher remanence but a lower coefficient of temperature variation. For horse-shoe magnets, and for all those which are to be used in instruments where extreme constancy is required, the gap between the poles should be as short as possible, and the polar areas should be as large as possible. From the point of view of constancy there is an advantage in having a considerable stray flux (through a magnetic shunt or otherwise) in addition to the useful flux.

The forging of the magnet, whether bar or horse-shoe, should be done with as little working of the material as possible, and at as low a temperature as is convenient. After forging, to normalize the material it should be heated to 900° C., lowered to 750° C., and there maintained for a time, then cooled off. To harden the magnets, they should be raised to 950° C. for a period not exceeding 5 minutes; and then lowered to about 700° C. and quenched at this temperature in brine or oil, at a temperature under 20° C. Some brands of high tungsten steel appear to yield a better result if quenched at some temperature between 770° and 850° C., or even at higher temperatures.

With tungsten steels there appears to be no advantage whatever in tempering the steel. With carbon steels there is no advantage in tempering the magnet if it is one having a small dimension-ratio (as for short bars). But bar magnets of carbon steel, if their length is as much as 20 diameters, may be tempered to a straw tint; or if as much as 40 diameters long, or more, they may be let down to a blue tint, and will then receive more magnetism. Any letting down below a straw tint, however, impairs their power to resist decay and usage.

The magnets should then be matured either by boiling or steaming them for ten or twelve hours, or by heating them to 60° C. for 20 or more hours. There is some advantage in letting them cool several times during this process. For a magnet that is intended to be used at some particular temperature, and is required to be as constant as possible at that temperature, there is some advantage in subjecting it to a number of cyclic processes of alternately heating it to a few degrees above, and cooling it to a few degrees below, that temperature at which it is intended thereafter to be used.

The magnet should then be magnetized by means of an electro-magnet, or, if a bar magnet, in a powerful magnetizing coil, to the highest degree of magnetization possible. There is some advantage in reversing its magnetism a few times. There is no advantage in magnetizing for a long time, as the result depends on the maximum magnetizing force applied, and not on the duration of the application. But in the final magnetization the magnetizing current should not be suddenly switched off, but should be diminished gradually to zero. There appears to be a slight advantage in subjecting the magnet during magnetization to mechanical percussion, by striking it with a hammer of brass or other non-magnetic hard material.

For magnets that are to be given extreme constancy, there is an advantage in then slightly reducing their magnetism by subjecting them to demagnetizing forces, a reduction of from 5 to 10 per cent being sufficient. It is doubtful whether this process is necessary * for magnets the form of which is a closed circuit with a very narrow gap.

* Mr. George Hookham kindly informs me that for meter magnets the firm of Chamberlain and Hookham do not resort to partial demagnetization. "We flash our magnets," he says, "to saturation, and never think of reducing them, and find them sensibly permanent—that is, if they have only the air-space to deal with. If they have other reversing influences to contend with, as in alternating-current meters, then we may reduce them."

There is supposed also to be some advantage from the point of view of constancy in subjecting the finished magnet to percussion.

The rationale of the advantage of reducing the power of the magnet by a partial demagnetization is worthy of a brief consideration. On p. 92 above were set forth the reasons why the self-demagnetizing

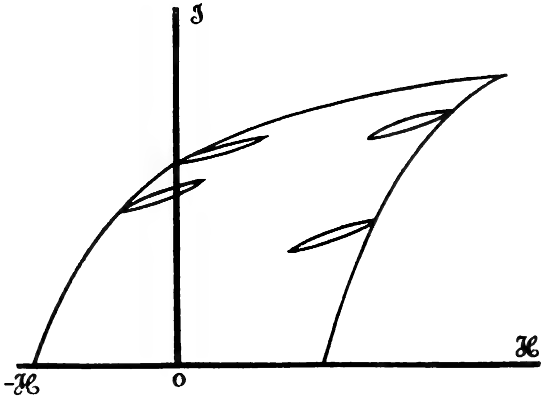

FIG. 29.—Subsidiary Hysteresis Loops.

factor of a magnet reduces the remanent magnetism; and in Fig. 10 was given a graphic construction, due to Ascoli, explanatory of the same. Ascoli has himself further studied the stability of the remanent magnetism, as have also Du Bois and Taylor Jones. It was remarked long ago by Ewing that if at any point of a hysteresis loop the

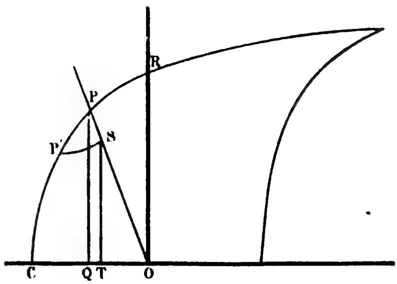

FIG. 30.—Diagram of Partial Demagnetization.

magnetizing force is partially reduced and then raised again, the resulting curve shows a subsidary narrow loop, the slope of which is, in general, quite small. That is to say, at every stage of magnetization there is a removable part of the magnetism which vanishes on a partial demagnetization and is again restored on the cessation of the demagnetizing force. And for this reversible part, the quasi-permeability

is small. Fig. 29 shows a number of loops corresponding to such small cycles of operation. Now consider a permanent magnet of such steel that the remanence, if all demagnetizing influence were absent, would be represented in Fig. 30 by the height of the line O R. Now let there be a self-demagnetizing coefficient, and let the line O P be drawn at such a slope that the tangent of the angle R O P represents the demagnetizing coefficient. Drop the perpendicular P Q. O Q will represent the actual self-demagnetizing magnetic force. Then the remanence will be reduced to P Q; and though O C is the coercive force of the material, yet it is only the excess of O C over O Q, namely, the length C Q, which represents the reserve that the magnet has to resist external demagnetizing influences.

Now let the magnet be slightly further temporarily demagnetized by an external demagnetizing force down to the point P′. On removing this demagnetizing force the magnetism will spring up to S; the height S T now representing the permanent magnetism that is left behind, and which is, say, 10 per cent less than P Q. Although the remanence is thus reduced, yet the stability will be greater. For the self-demagnetizing force is now only O T, and the length C T, which represents the reserve of coercivity of the magnet, is greater than before.

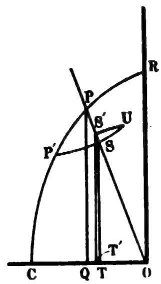

FIG. 31.—Diagram of Stabilization by Subjecting to Small Cycle of Magnetizing Forces.

But we may carry the process a stage farther. After applying a small external demagnetizing force to bring the value down to P′, apply an equal small remagnetizing force, and bring the magnetization up, as in Fig. 31, to the point U. Then remove this external force, when the narrow loop will return from U to a point on the line O P at S′, at the height S′ T′. The result is then that the permanent magnetism will be represented by the line S′ T′, which is slightly greater than S T, while the reserve of coercivity C T′ is almost as great as before.

It would, indeed, be advantageous to repeat this small cycle several times over, so as to bring the magnetic state into a final condition, where the effects of any accidental external magnetic influences will be a minimum. In brief, just as the thermal stability of a magnet is best assured by subjecting it to small cycles of heating and cooling about the point of temperature at which it is thereafter to be used, so its magnetic stability is best assured by subjecting it to small cycles of demagnetizing and remagnetizing force about the point of magnetization at which its magnetism is thereafter to remain constant.

CONCLUSION.

After the remarks made in the course of this lecture as to the need of research in various directions, both as to the production of alloy steels of the desired magnetic properties and as to the further studies

of their magnetic and thermal constants, it is needless to enlarge on the field that is open for extensions of knowledge. There must be co-operation between the steel-makers on the one hand and the physicists on the other hand in any such investigations. Magnet steel of high quality is in great demand to-day. We know that the Sheffield manufacturers are alive to the demands of modern science, particularly in the matter of special steels for tools, in which they were the pioneers. Even to-day, in spite of all tariffs and all competition, they are sending vast quantities to America and to the Continent. But only lately I heard that an order for 300 tons of magnet steel at £60 a ton had been placed with a firm having no connection with Sheffield. The production of magnet steel is a branch of business which, even more than the manufacture of tool steel, depends on the co-operation of science with industry. Certainly our Institution will not be slack in endeavours to bring about such a co-operation, when once the interests involved are realized, and the problems awaiting solution have been set forth in the light of day.

[A number of permanent magnets of different forms, constructed by different firms, Messrs. W. F. Dennis & Co. (Remy steel), MM. Ch. Pinat et Cie, of Allevard, Messrs. the Thomson-Houston Company, and Messrs. H. Shaw & Sons, of Sheffield, were shown upon the lecture-table.]

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Proceedings of the Five Hundred and Forty-fourth Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 28th November, 1912—Mr. William Duddell, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting, held on 14th November, 1912, were taken as read, and confirmed.

The list of candidates for election and transfer approved by the Council for ballot was taken as read, and it was ordered that it should be suspended in the Hall.

Messrs. H. G. Solomon and C. P. Hammond were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

Arthur John Beckett.
Captain Chetwode George G. Crawley, R.M.A.
Giuseppe Faccioli.
Arthur John Fuller.
Francis Bennett Gerrard.
Charles Chandler Gilchrest.
Professor Andrew Gray, M.A., LL.D., F.R.S.
James Alexander Hamp.
William Arthur Ker.
Waldemar Koch, Dr. Eng.
David Barclay Mellis.
Professor Charles Proteus Steinmetz, Ph.D.
Alfred Langrish Stephens.
George Henry Thurston.
Theodore Zettel, Ph.D.

As Associate Members.

Frederick Addey, B.Sc.
Andrew Alexander-Todd.
William Day Appleton.
George Rowland Archdeacon.
Fred. Charles D. Atkin.
Sydney Pearse Barnes.
Reginald John Bellerby.
Charles Reginald Belling.
John Birch.
William Lister Bird.
Percy Roberts Blake.
Frederick von Blanckensee.
Harry Brakes.
Harry Percy Brown.

ELECTIONS—*continued.*

As Associate Members—continued.

George Henry Bryant.
James Waldron Bussey.
Charles Alfred Butlin.
Charles Lees Calvert.
Francis Glyde Chaffey.
Arthur Reginald Chaytor.
Frederick Ernest Chilton.
George Francis Cole.
Harry Cooper.
Charles Theophilus Crisp.
John Robinson Davison.
Claude William Denny.
Howard Percy Devereux.
Andrew Service Duncan.
Percy Dunsheath.
Howard Maurice Edmunds.
Herbert Willoughby Ellis.
David M. Eunson.
Gwilym John Evans.
Joseph Hugh Evans.
Frank Farndon.
Herbert Halyburton Fell.
John Ferguson.
Savile Rutter Fletcher.
Thomas Joseph Flynn.
Cornelius Henry Fox.
Harry Smith Furneaux.
James Gardner.
Arthur Ernest H. Gates.
Sidney Frederick J. Gilliver.
David Goad.
Bernard Watkins Gothard.
Edward Bentall Gray.
Edward Joseph B. Greenwood.
Harold Paul Greenwood.
Alexander George G. Guthrie.
John Hamilton.
Joseph Henry G. Hampshire.
Reginald Willway Harding.
Edgar Harper.
Burton Tyas Hawkins.
Gerald Summersell Haydon.
Charles Culmer Hodges.
Lloyd Leslie Horrell.
John William Houghton.
Captain Frederic Arthur Iles, R.E.
Frederick Grosvenor Jackson.
George Edward Keane.
James Aloys Keasley.
Alfred George Kemsley.
George Lingard Kirk.
Reginald William Klitz.
Charles William Knighton.
Henri Leverrier.
Stanley Llandaff B. Lines.
Sydney Charles Lloyd.
Pelham Berkeley Longbottom.
Donald Eugene McDonnell.
Neil Gloster McLeod.
José F. Martins-Guimaraes.
Thomas Spencer Midgley.
Charles Edwin Morgan.
Arthur Hyatt Morse.
Arthur Montague Mulliner.
Harrison Noel Newcomb.
James Alexander Omand.
Arthur James Ostler.
Thomas Richard Overton.
Charles Lascelles Parrington.
Hubert Parry.
George Parsons.
Frank Gervas Payne.
William Pearson.
Henry George S. Peck.
Ivor Augustus D. Pedler.
Charles Sidney Perry.
Walter Charles S. Phillips.
Thomas Edward Pickford.
Joseph James Pigg.
Arthur Podmore.
William Henry Powell.
Leslie Hampden Pratt.
George E. James Pople.
Vinayak Shankar Randive.
Albert Henry Rayner.
William Redman.
Lewis Fry Richardson.

ELECTIONS—*continued.*

As Associate Members—continued.

George Berkeley Rolfe, B.Sc.
John Alexander G. Russell.
Henry Rimer Sanders.
Tom Alfred Sedgwick.
Louis John Simon.
Bernhard Gustav J. Sluyterman.
Arthur Clifford Smith.
Charles Smith.
James Arthur Smith.
Sydney Herbert Smith.
Henry Drummond Staniar.
Ernest Wilson Steele.
Harry Potts Stokes.
Frank Bertram C. Sutthery.
Tom William Swift.
Benjamin Blenkinsop Swinburn.
William Hughes T. Swire.
William Tuxford Tallent-Bateman.
Reginald Charles Thackeray.
Ernest Byers Thomas.
Frederick Lewis Thomas.
Nathaniel Herbert Thomas.
Francis Joseph Tillson.
Frederick James Turquand.
Walter Edgar Twells.
Arthur Waller.
Percy Webberley.
Thomas Frederick Wells.
John Galbraith Westlake.
Oliver Wheeler.
John Charles White.
William John Williams.
Arthur Frederick Williamson.
George Edward E. Williamson.
John Wilson.
Lancelot Edward Wilson.
James Lysle Winter.
Stanley Hercy P. Wolferstan.
Harold Lee Wood.
William Leonard Wreford.

As Associates.

Alfred Ernest Brooks.
William Thomas Clough.
Howard C. Levis.
Robert Macmillan.
Lieut. Aureliano de Almeida Magalhaes.
Paul Henry Marco.
Fred J. Robinson.
Harold Charles Wright.

As Graduates.

William T. Aked.
Harold Godwin Alger.
George Alfred Applebee.
Ralph Gladstone Ashton.
Thomas Henry Banks.
Dudley Bartels.
Jal R. Batliboi.
Biharilal Batra.
Edwin J. L. Bennett.
Thomas Dudley B. Bowater.
Walter Stanley Bracher.
Wilfrid Branfield.
Hugh Alexander Brown.
John Alexander C. Campbell.
Enrique Cangas.
Frank Gordon Cerexhe.
Percy Herbert Chesterton.
James Coid.
Harry Cotton.
Edward Rainier Crook.
Charles Linn Dick.
John Elders.
Frederic James Elliott.
John Lindsay Ferguson.
John Fitzgerald.
Howard John Fountain.
Randal Eugene Golden.
Lionel Allan G. Grant.

ELECTIONS—*continued.*

As Graduates—continued.

Bertram James Grigsby.
Harry Ernest Hawley.
Frank Burton Hellon.
Henry Charles Jay.
Eric Ford Jones.
Arthur Abraham Koppel.
Thomas Langlois.
Harry Eugene Lintott.
Joseph Lythgoe.
George Melven Macilwraith.
Alexander McIntosh.
William Adam G. McLean.
Edward McSwiggan.
Thomas Marsden.
Morres James H. Molyneux.
Cuthbert Edgar Monks.
Roland Hans Nebel.
Thomas John Nelson.
Frederick James N. Nibloe.
Louis Nordwald.
Albert George Norris.
Oscar Parker.
H. P. Parry.
Cyril Vivian Peake.
Ernest Arthur Pells.
George Arstall Platt.
Ernest Herbert Pottle.
William Reginald Radford.
Richard Silcock.
Herbert Willis Smith.
William Samuel H. Smith.
Thomas Stephen Stout.
Friedrich George Stuckenschmidt.
Frederick George Thomas.
George Levett Thorburn.
Alfred Walsh.
Horace Ward.
Philip Guiss Westmacott.
Frederick Randolph Wilkinson.
Christopher Kenneth Wise.
William Woods.
Albert Edward Wray.
Vaughan Lascelles Wynyard-Wright.

As Students.

Arnold Wilson Adams.
Walter Henry Barling.
Hugh Glover Bell.
Bernard Lees Bishop.
William Herbert Blythe.
Philip Seymour Brett.
Cecil George Calman.
David Sibbering Charles.
Joseph Harold Charnley.
Beng Wan Chua.
Ralph St. John Clarke.
John Purrett Clifton.
Hubert William Corke.
Edward Willford Cosserat.
James Allan Cunningham.
Arthur Robert Dawes.
William John F. Debley.
John Gordon Deedes.
Julio Pedroso de Lima.
Harold Anthony Denison.
Philip Henry Denton.
David Benjamin Devonald.
William Vere H. Duff.
Walter Eastwood.
John Claude Elmer.
Herbert Emmett.
Basil Ziani de Ferranti.
Leslie Eric Felix-Smith.
Charles Mac Fiander.
Eric Arthur H. French.
John Maughan Gibson.
Percy Leach Gill.
Basil Montgomery Gillett.
Cyril Edwin Glasspool.
Eric Harraden Glover.
Leslie Ward Goddard.
Richard Gosden.
Charles Cock Harry.

ELECTIONS—*continued.*

As Students—continued.

Frank Howarth Hayward.
Frederick George Head.
Joseph Arthur B. Hellaby.
Edgar John C. Herring.
Leslie Burgess Hobgen.
Worsley Holttum.
Frank Howard.
Alfred Millard Jenkins.
John Richard Jones.
Naunidh Rai Kapur.
Edward Thomas King.
Bertram Lakeman.
George Alexander B. Leishman.
Brian Ernest Leeson.
Reginald Artane McMahon.
Frederick William Mesinger.
Nusserwanji Ruttonji Mistry.
Walter Isaac Monkhouse.
Charles Isted Morris.
John Franklin Newton.
Peter Ostler.
William George Paddock.
Roderick Norman L. Protheroe.
Samuel Edward T. Pryce.
Arthur Hubert Pullen.
Pappu Subba Rao.
Duncan Galt Reid.
Harold Archibald Rickwood.
William Oliver Robotham.
Phani Bhusan Roy.
James Francis Slingerland.
Fred Smethurst.
Leslie Victor Smith.
Harold William S. Smyth.
John Steele.
Ernest Swinton.
Douglas Godden Teague.
Raj Narain Tiwari.
Reginald Urquhart.
Hermann Albertus Voss.
Cyril Thornton Wallis.
Richard Jeffery Webb.
Horace Bernard Webster.
Leslie Harold V. Webster.
Lionel Bertie Whitaker.
Maurice Hazel Whitehouse.

TRANSFERS.

From the class of Associate Members to that of Members :—

Daniel Adamson.
Harry Allcock.
James Grey Bell.
William Melland Booth.
William Anderson Brown.
Robert Marshall Carr.
Frederick William Carter.
Charles Day.
Evelyn Fawssett.
John George Freeman.
Benjamin Shuttleworth Hornby.
Herbert Francis Hunt.
John Henry Johnson.
Theo Kerr-Jones.
Louis Herbert King.
George Duthie Leys.
Arthur Ernest Malpas.
Raymond Gingell M. Mercer.
Donald Smeaton Munro.
Arthur Joseph Newman.
Percival Herbert Powell.
Thomas Roles.
Fitzroy Owen J. Roose.
Bernard Sankey.
Arthur Holroyd Sears.
Herbert William Sprunt.
John Edward Tapper.
William Court P. Tapper.
Edwin Abraham Uttley.
Arthur Watts.
Harry Egerton Wimperis, M.A.

TRANSFERS—*continued.*

From the class of Associates to that of Members :—

John Muir Donaldson.
Edgar Stopford Saunders.

From the class of Associates to that of Associate Members :—

Ernest Arthur Bayles.
Charies William V. Biggs.
Robert Frederick Botting.
Allan Charles Campbell.
Alfred Norman Dixey.
Surendranath Ghosh.
Harry Hughes Harrison.
Thomas Cyril Hunt.
Frank Knight Jewson.
Raphael Sanzio Mansel.
Percy Gordon Moore.
Aubrey Bertram Stratford.
John Henry M. Wakefield.

From the class of Students to that of Associate Members :—

Sidney Aitken.
Arthur John Aldridge.
James Allan.
Francis George Allen.
Edward Andreas Anderson.
John Bilsland.
John Surrage Blackmore.
Charles Arnold Brearley.
Thomas Everard Bridge.
Henry Arthur Browett.
Charles Stewart Buyers.
George Frederick Cobham.
John Douglas Crawford.
Heber Ewart Crowcroft.
John William Dodds.
William Duckworth.
Frederick Paul Dumjohn.
Alfred William Epton.
Charles George G. Faine.
Sidney Woods Farnsworth.
Ernest Henry Field.
Clarence Reginald Ford.
The Hon. Edward Fulke French.
Prosper Barnaby Frost.
Alexander Gardner.
Cyril Charles Garland.
Albert Hardy Gay.
Randolph Douglas Gifford.
Joseph Gilbert.
William Albert Gillott.
Tom Golding.
Hugh Welbourn Gregory.
Nigel Cuthbert E. Hall.
John Hargrove.
Charles Howard Harvey.
Albert Neilson Haworth.
Leslie Barnett Hewitt.
Clifford Higgins.
Edward Percy Hill.
Ernest Percival Hollis.
James Colston Kelso.
Philip Kemp.
Philip Lobel.
Reginald Frederick Long.
Alfred Lawrence Lunn.
Vernon Mallalieu.
Hugh Charles May.
George Horatio Nelson.
Kenneth Lancaster Osborne.
Reginald George Parrott.
William Pate.
Alec Stewart Paterson.
Edward Peck.
Harold Papworth Pentelow.
Reginald Cheyney Plowman.
Stanley Herbert Pook.
Henry Edward Poole.
Kenneth Preston.
Alec Walter Puttick.
Arthur Charles Rayment.
William Duckett Redfern.
Harold Willoughby Richardson.

TRANSFERS—*continued.*

From the class of Students to that of Associate Members—*continued* :—

Gilbert Colville Shadwell.
Newton Shuttleworth.
Norman Stuart Sim.
Frank Wallace Skinner.
Herbert James Skinner.
Maurice Swift.
Oliver Thornycroft.
Edmund George Townshend.
Frank Lester N. Tuck.
Thomas Seabrooke Wallis.
William Herbert Walton.
Andrew Weiss.
John William Wheeler.
Arthur Penry Williams.
Alfred Lee Wood.
Francis Vivian Wythes.

From the class of Students to that of Associates :—

James Hepburn Rickie.
John Langford D. Ridsdale.

From the class of Students to that of Graduates :—

Benjamin Abel.
Joseph William Arnott.
Daniel Dadsworth Barber.
Robert Fleming Benson.
William John I. Casewell.
Lionel Ernest Cooke.
Edward Stephen Coombes.
Dorabji Rustomji Cooper.
William Thomas Golden.
Alexander George Gow.
Percival Grice.
Leslie James Jowit.
Alban Aloysius Kilduff.
Vinayak Ganesh Kirloskar.
Nanigopal Mukerjee.
Clifford Anthony Newell.
Thomas Francis Phillips.
William Arthur Pitts.
William Rintoul.
Joseph Vincent Rugeroni.
Henry Edward Sanders.
William Alexander Scotter.
August Sild.
Arthur Albert Stone.
Richard de Fontaine Stratton.
Hugh Pilgrim P. Trimingham.
Piers Alton E. Warburton.

A paper by Mr. J. S. Peck, Member, entitled "Earthed *versus* Unearthed Neutrals on Alternating-current Systems" (see page 150), was read and discussed, and the meeting adjourned at 9.45 p.m.

EARTHED *VERSUS* UNEARTHED NEUTRALS ON ALTERNATING-CURRENT SYSTEMS.

By J. S. PECK, Member.

(*Paper received 15th September, 1912; read before* THE INSTITUTION *28th November, before the* MANCHESTER LOCAL SECTION *19th November, before the* BIRMINGHAM LOCAL SECTION *27th November, and before the* NEWCASTLE LOCAL SECTION *9th December, 1912.*)

The questions where, when, and how should the neutral point or points of an alternating-current system be earthed are highly controversial ones, and though they have been discussed a number of times before the technical societies, there are still wide differences of opinion among operating engineers as to the proper course to follow under different conditions. While it is difficult to give general rules covering all cases, there are certain conditions where the proper course to follow seems clear, and it is intended in this paper to discuss the various questions involved, and to show why under some conditions of operation it is becoming general practice to earth the neutral, while under others it may be advisable to operate with the neutral unearthed.

Three classes of apparatus or systems will be considered :—

1. Generators.
2. High-voltage transmission circuits.
3. Low-voltage distribution circuits.

Before considering these three classes separately, the general advantages and disadvantages of the earthed neutral which are common to them all will be considered.

While several advantages may be claimed for the earthed neutral, there are but two which are really of any great value. These are :—

(*a*) The limiting of the voltage between line wires and earth.
(*b*) The possibility of cutting off any wire or feeder in the event of an earth upon it.

The chief objection to earthing is the fact that the system cannot be operated with an earth on any line wire.

It is well known that in an unearthed 3-phase system, where the impedance of each phase to earth is equal, each of the three conductors assumes a potential above earth equal to 58 per cent of the voltage between wires while the neutral point is at earth potential, but any difference in the impedance causes the potential of the neutral point to shift, bringing the phase having least impedance nearer to earth potential and raising the others to a potential correspondingly higher above earth.

When one phase is earthed, it assumes earth potential, and the other phases reach full-line potential above earth. When the potential of each phase to earth is the same, the charging current to earth will be equal in all phases; but when one phase is earthed the charging current to earth is increased by 73 per cent in both of the unearthed phases, and all of this current will return to the generator through the earthed phase and will pass into the faulty wire at the earthed point.

Earthing the neutral means that this point is fixed at earth potential, and the three-line wires are fixed at a potential 58 per cent above earth.

If the neutral point is earthed through a resistance and one phase is earthed, current flows through the neutral resistance, and the drop across this resistance measures the voltage between the neutral point and earth, so that the neutral point is not fixed definitely but depends upon the amount of resistance and the current flowing through it. Thus

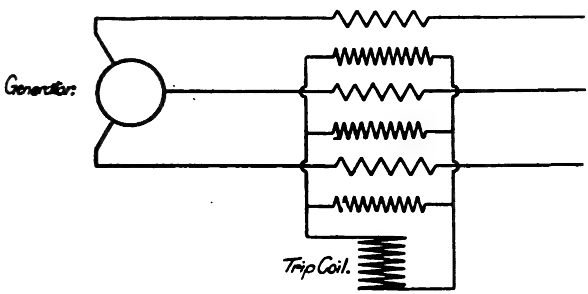

FIG. 1.—Balanced Protective Gear.

the conditions are intermediate between those with the unearthed neutral and with the neutral solidly connected.

With the earthed neutral very simple and reliable apparatus can be provided, which will cut out any feeder in case an earth occurs upon it. This apparatus may be of the Merz-Price balanced protective type, or it may be of that type which is based upon the principle that the resultant current in a 3-phase circuit is zero. Fig. 1 shows a well-known arrangement of this kind. It is evident that under normal conditions the resultant current in the secondaries of the three transformers is zero, therefore no current can flow through the trip coil; but should an earth occur on one of the wires, the circuits will be unbalanced and current will flow through the trip coil. This apparatus may be adjusted to work on a very small earth current.

Still another possibility is to put a trip coil in series with the earth connection, but this arrangement is not so satisfactory as those previously mentioned, in that it does not discriminate as to the feeder to be cut off.

Where a large cable network is supplied from common busbars

the arrangement shown in Fig. 1 may work satisfactorily without the neutral point being earthed. The reason for this is as follows :—

In the event of an earth on any wire, the charging current to earth on all of the cables on the system will be returned to the busbars through the wire which is earthed. Therefore the current balance on the earthed feeder will be upset and the circuit-breaker operated. This arrangement is, however, limited in its application, and is not so positive as when there is a current flow between earth and generator neutral.

1. Generators.

All modern 3-phase generators are insulated to stand operation without the neutral being earthed, so that no consideration of reduced first cost due to limiting the voltage to earth by earthing the neutral need enter the problem. Also the question of risk of breakdown with earthed or unearthed neutrals may be practically neglected.

When deciding upon the advisability or otherwise of earthing the neutral of a generator, consideration must be given to the service for which it is to be used. Three classes of service will be considered :—

(*a*) Generator supplying network of underground cables.
(*b*) Generator supplying overhead transmission circuits without transformers.
(*c*) Generator supplying overhead transmission line through step-up transformers.

In case (*a*) an earth on any phase of a cable is almost certain to develop into a short-circuit between phases, and on a large system the result of a short-circuit on a cable is usually to burn off a section of the cable, perhaps to damage adjacent cables, and sometimes to set up surges which may cause serious breakdowns elsewhere on the system. Since the majority of breakdowns on cables are to earth, it is therefore extremely desirable that a cable be cut off as soon as an earth develops, and as this can be done quickly and with certainty where the generator neutral is earthed, it is becoming general practice to do this in all stations of this class. Thus practically the only question to consider is whether the neutral should be earthed solidly or through a resistance, and, where there are several generators in parallel, how many of them should be earthed. The only object in inserting resistance between generator neutral and earth is to limit the rush of current which occurs whenever there is an earth on the system. If balanced protective apparatus is used, similar to that shown in Fig. 1, the resistance may be made so high as to keep down the current rush to a comparatively low value, because the protective apparatus may be set to operate with a very small unbalancing in current; but where plain overload protection is used—and this applies to the majority of cases—the resistance should be low enough to pass sufficient current to trip the breakers on

the largest feeder, but high enough to prevent damage to generator windings, and, if possible, to prevent serious burning at the point of accidental earth.

In calculating this resistance, allowance should be made for the drop in voltage on the generator due to the load on one phase, also for the resistances in neutral earth connection in the feeder and at the point of accidental earth.

In designing the resistance, ample heat capacity should be allowed, for the circuit-breakers may stick, or there may be at least temporarily a fairly high resistance at the point of accidental earth which will prevent the circuit-breaker from opening immediately, and so give time for the resistance to reach a high temperature.

In general, earthing resistances are built up of cast-iron grids mounted on porcelain insulators, and should be located in a fireproof compartment. They are usually designed to have sufficient heat capacity so that full star voltage may be maintained across them for at least 15 seconds without a temperature rise of approximately 300° C. being exceeded. The ohmic value of the resistance is determined by the current required to trip the breakers on the largest feeder.

Where it is desired to earth through a resistance, and two or more generators are operated in parallel, the simplest and best method in so far as earthing is concerned is to connect the neutral of all the generators to a common busbar which is earthed through a suitable resistance. Then, regardless of the number of generators, the value of the earth resistance will always be the same. With this arrangement switches must always be provided between each generator and the neutral busbar, otherwise a generator disconnected from the main bus and shutdown may be raised to a dangerous potential above earth, in case of an earth on any feeder. The objection to this method of connection is that in certain cases very heavy currents may circulate through the neutral connections and generator windings, and while the deleterious effect of this current has been greatly overestimated, it appears that in certain cases it has been almost impossible to operate with the neutrals connected solidly together. When the wave-forms of the different machines are exactly the same, no current can flow through the neutral connections, but when the wave-forms are different and there is a third (or multiple of three) harmonic present, current will flow through the neutral connection, the amount of which will depend upon the value of the third harmonic, the displacement angle, and upon the impedance that the generator offers to this current of triple frequency. Machines which at no load give no circulating current, may show heavy circulating currents as the load comes on, or vice versa. The amount of the circulating current also depends upon the adjustment of the field current of the different machines, and upon the angular variation in speed of the prime movers.

In certain abnormal cases the current in the neutral may amount to the full-load current of one generator or even more. It should be noted, however, that the neutral current divides equally among the three

phases of the generator, so that full-load current in the neutral is equivalent to only 33 per cent full-load current in the windings of the generator, and since the heating in the windings is equal to the sum of the squares of the main and circulating currents, the resultant heating is increased by only 11 per cent—not a serious amount even with this very high value of circulating current. With 30 per cent of full-load current in the neutral the resultant heating in the generator is increased by only 1 per cent at full load and is quite negligible.

It would seem advisable, therefore, to try connecting the generator neutrals solidly together, especially in the case of turbo-generators, where there is no angular variation in speed, and therefore less chance of circulating currents. Should satisfactory operation be impossible, then only one generator should be connected to the neutral bus, or resistance may be inserted between each generator and the neutral bus.

Of the three methods, that of connecting the neutrals solidly together is undoubtedly the simplest and best, while the method of operating with an earth on only one generator comes next in simplicity. The switching on and off of the different generators to the neutral bus may be done automatically, as at the London County Council Greenwich generating station, or it may be done by hand. While the latter method depends upon the memory of the station attendant, or upon the systematic operation of the station, the fact that the attendant may forget to connect a machine to the neutral busbar does not mean disaster, as it is only in case of a cable breakdown that trouble may occur, and it must be remembered that many stations are operating successfully with an unearthed neutral.

Where resistances are used between each generator and the neutral bus, a very considerable expense is involved in providing these resistances, and it is often difficult to find room in the station to accommodate them; also the resistance in the earth circuit is much less when there are several generators in parallel than when only one is in use.

(*b*) *Generators supplying Overhead Transmission Line without Step-up Transformers.*—In this case there are usually a comparatively small number of transmission lines, and an earth on one wire is not likely to cause a short-circuit between wires, so that with an unearthed neutral it is quite possible to run for a long period with one wire earthed. If there are not sufficient feeders to all distributing centres to make it possible to cut out a defective one and deliver full load over the others, the possibility of being able to operate with one wire earthed may be sufficient far to outweigh the only practical advantage gained by earthing the neutral, which is the ability quickly to cut off a feeder in the event of an earth upon one wire. Limiting the voltage between each wire and neutral, which can be done by earthing the neutral, is of very little importance, since the voltage is in no case very high, and the generators and the line must be insulated to withstand strains set up by lightning discharges and other abnormal conditions. It would appear, therefore, that in the majority of cases the weight of the arguments are in favour of operating with the generator neutrals unearthed.

(*c*) *Generators supplying Transformers which Step Up to a very High Voltage for Long-distance Transmission.*—In this case there are conditions involved which make it extremely desirable to earth the neutral or else to earth the low-tension side of the transformers.

If one terminal of the high-tension side of a single-phase transformer be connected to a high-tension line, and the low tension be open-circuited, the high-tension winding assumes a potential above earth equal to that of the line to which it is connected. The high-tension winding then acts as one plate of a condenser, with the low tension and core as the other plates. The low-tension winding also has static capacity to the iron, and it will therefore assume a potential above earth the value of which will depend upon the relation between its capacity to the high-

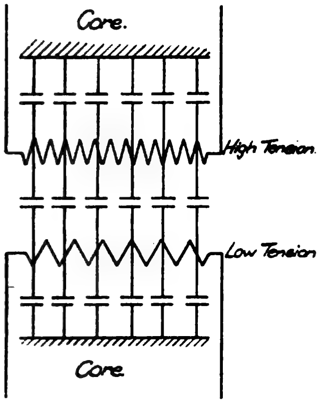

FIG. 2.—Electrostatic capacity between Coils, and between Coils and Core of a Transformer.

tension winding and its capacity to earth. In practice it may easily assume one-half the potential of the high-tension winding (see Fig. 2).

If—

K = capacity between high tension and low tension,
K_1 = capacity betwen low tension and core,
V = voltage of high-tension line,
V_1 = potential of low-tension winding above earth,

$$V_1 = V \times \frac{K}{K_1}.$$

If both terminals of the high-tension winding are connected to the high-tension lines, the resultant potential of this winding above earth will be zero, and the resultant potential of the low-tension winding above earth will also be zero. If, therefore, one terminal of a transformer be connected to or disconnected from a line before the other, the low-tension winding during this period may assume a very high potential above earth and its insulation be broken down to earth.

When the low-tension winding is connected to a generator, the generator winding also has capacity to earth, so that the capacity of the low-tension winding to earth is increased by this amount and its voltage correspondingly reduced. If the capacity of the generator-winding to earth $= K_2$, then the voltage of the low-tension circuit above earth $= V \times \frac{K}{K_1 + K_2}$. Thus, while the voltage is reduced by connecting the transformer and generator windings together, it may still be dangerously high.

When both terminals of the high-tension winding are connected to the line and one wire is earthed, the resultant voltage of the high-tension winding above earth will be $V/2$, and that of the low-tension winding will be one-half as great as before, but may still be dangerously high.

In the case of a 3-phase transformer the conditions are much the same as described above for a single-phase transformer. If one terminal of the transformer is connected to the line, the whole of the high-tension windings will be raised to full-line potential above earth, and the low-tension winding will be raised to a correspondingly high value. If the three terminals of the high-tension winding are connected to the line and one line wire be earthed, the resultant potential of the high-tension winding will be 58 per cent of the line voltage above earth, while the low tension will be 58 per cent of its former value above earth.

It is evident that when the neutral of the high-tension system is unearthed there are several possibilities of obtaining a high voltage on the generator windings, viz. in the case of an earth on one high-tension wire, or in the case of the switches in the three phases not operating simultaneously. It is not necessary that a line wire should be permanently earthed in order to raise the voltage on the low-tension circuit, as a discharge over a lightning arrester may so disturb the static balance of the system as to produce a high potential on the low-tension winding.

A case is on record where several generators in parallel were supplying a very high-voltage line through step-up transformers. The neutral point of the generators was unearthed. The lightning arresters on one high-tension phase broke down and the generator windings were raised to a high potential. One generator broke down, and the attendant, seeing smoke, cut it off the busbars. A second machine immediately broke down, and was in turn cut off. This was repeated until nearly all the generators in the station were broken down. The attendant then cut off the excitation. Had the neutral point of the generators or that of the high-tension side of the transformers been earthed, the trouble could not have occurred in the generator circuit. There is, however, a chance of similar trouble even when the high-tension neutral is earthed, provided the generator neutral is unearthed. This occurs when the insulation between the high- and low-tension windings of the transformer breaks down at one point. The low-tension

winding then assumes the potential of the high-tension winding at the point of connection, and a breakdown on the low-tension system is almost certain to follow.

With electrolytic lightning arresters it is necessary to connect the aluminium plates to the lines at frequent intervals in order to preserve the film on the plates. These plates have a high electrostatic capacity, and a considerable charging current flows through the arrester while it is connected to the line. It has been found on one system that when the arresters are being charged, disturbances are set up on the system and breakdowns have occurred on secondary circuits. It is quite possible that this trouble is due to the static unbalancing of the high-tension circuits, which in turn raises the secondary circuits to a high potential above earth.

Where it is considered undesirable to have a direct connection between low-tension windings and earth, a spark-gap may be placed in the circuit and set to break down at a pressure slightly above the normal voltage of the winding above earth.

Another method of protecting the low-tension system in the case of breakdown between the windings of the transformer is the use of earth-shields between primary and secondary windings. This system was at one time in general use in this country, but it is now scarcely ever called for. The objections to it are :—

(*a*) Increased transformer cost.
(*b*) Increased risk of transformer breakdown.
(*c*) No protection when breakdown is between leads or anywhere except between transformer coils.
(*d*) The danger that earth-shields may become displaced.
(*e*) The leads connecting earth-shields to core may be broken due to vibration or to settling of core and coils, or they may be burned off due to heavy charging current which may flow over them.
(*f*) Part of earth shield in the neighbourhood of fault may be burned away and leave primary and secondary windings connected together with no earth on the system.

While it may be argued that many of these difficulties are due to defective manufacture, and should not occur provided proper precautions are taken, there is always the possibility that they may occur, and the practical impossibility of inspecting the earth-shields after the coils are assembled makes it almost impossible to determine their condition.

Thus in the case of generators supplying a high-tension transmission line through transformers, it is highly desirable to earth the neutral point of the generators, and preferably through a rather low resistance. If it is not desired to connect permanently to earth, then a spark-gap should be used in the earth connection.

2. High-voltage Transmission Circuits.

The arguments in favour of earthing the neutral in the case of high-voltage transmission circuits are :—

(*a*) The possibility of cutting off the circuit in case of an accidental earth on any wire.
(*b*) Reduced cost of transformers and of line insulators due to limiting the voltage above earth to 58 per cent of line voltage.
(*c*) Reduced cost and closer possible setting of lightning arresters.
(*d*) The possibility of using the earth as a conductor in the event of one line wire being disabled. This requires an earth at both ends of the line.

The argument against earthing the neutral is that an earth on one wire makes it impossible to transmit over the circuit.

(*a*) Whether there is any advantage in being able to cut off a circuit in the event of an earth on one wire depends upon the number of circuits available. In general, there are seldom more than two circuits to any sub-station, and often there is but one, so that cases may often arise where, to keep up the supply, it will be absolutely necessary to operate with one wire earthed. This method of operation would be impossible with an earthed neutral. Also an earth on one wire is not likely to develop into a short-circuit between phases, so that there is no particular need for cutting off the earthed circuit.

(*b*) The higher the voltage of the transmission the greater will be the saving in first cost made by insulating the transformer for 58 per cent of the line voltage and by using line insulators for a corresponding voltage; but in general it is bad practice to adopt this expedient, for it may prove impossible to operate continuously with an earthed neutral, due to disturbances on adjacent telephone circuits which may occur under certain abnormal conditions, and in the event of an earth on one wire it may be essential to remove the neutral connection and operate with one wire earthed. In general it is poor economy to cut down insulation on either lines or transformers.

(*c*) If the system may sometimes be operated without earthed neutral the lightning arrester equipment must be suitable for this condition, so that there is no saving in first cost; but as long as the system is operating properly with earthed neutral the arresters may certainly be set for lower discharge values than when the system is unearthed.

(*d*) In the event of one wire on an earthed system going to earth, it would still be possible to transmit approximately two-thirds the full amount of power over the two remaining wires, provided the neutral points at both ends of the line were solidly earthed and one line with its corresponding transformers cut out of circuit. In this case the earth would carry full-line current, and in very few places would this be allowed on account of disturbances to neighbouring circuits.

The neutral point of a high-tension system can be obtained only by connecting the high-tension windings of the transformers in star, or by the use of an auto-transformer, and this latter method is seldom if ever used. If the high-tension windings are connected in star, then damage to one transformer disables the whole group, while with the delta connection on both windings one transformer of the group may be cut out and approximately two-thirds the capacity supplied from the remaining transformers. (This does not hold in the case of 3-phase core-type transformers, but is true for single-phase and 3-phase shell-type transformers.)

The conclusions to be drawn from the above are that in the great majority of cases continuity of service will demand that the system be operated with an unearthed neutral, and that in general the transformers should be connected in delta on both high-tension and low-tension windings.

3. Distribution Circuits.

When it comes to a study of the question of low-tension distribution circuits, the majority of the arguments presented above will apply here also, but there enters one other consideration, *i.e.* the danger to human life.

If the neutral point of, say, a 500-volt system is earthed, then the maximum potential above earth which any point of the system can reach is 290 volts. If the system is supplied from a 500-volt generator, the difference between a 500-volt and a 290-volt shock may mean the difference between life and death, or it may not, and there is certainly greater risk of shock from a system which is permanently earthed than from one which is earthed only occasionally. The higher the voltage of the generator the less will be the value of the earthed neutral in preventing danger to life due to reducing the voltage to earth.

When the distribution circuits are supplied through step-down transformers, there is always the possibility that the low-tension circuits may be raised to a very high potential, either through an earth on one wire of the high-tension system, or through a connection between high tension and low tension inside the transformer. In the former case there is great danger to low-tension insulation, and in the latter there is danger to life as well as to insulation.

Thus, where the distribution circuit is supplied direct from a generator, the advantages of the earthed neutral are the ability quickly to cut off a defective circuit and a possible reduction in risk to life, due to lower maximum voltage to earth; but where the circuit is supplied through step-down transformers from a high-voltage line, there is danger to life and apparatus if the neutral is unearthed.

Where a single-phase circuit is supplied from a single-phase transformer, the low-tension winding should be earthed, preferably at the middle point of the winding, but it is better to earth one line wire than to leave the system unearthed.

CONCLUSIONS.

1. *Generators.*—When generators supply a system of underground cables, the neutral should be earthed through a resistance. This makes it possible to isolate a cable in the event of an earth on any phase before such an earth develops into a short-circuit.

When generators supply overhead circuits, it is a question of balancing continuity of service against the ability to cut off a feeder in case of an earth upon one wire. If there are comparatively few feeders, and these of large capacity, the balance will usually be in favour of not earthing, but each case must be considered individually.

When a generator supplies transmission lines through step-up transformers, the neutral should always be earthed or connected to earth through a spark-gap, otherwise the generator windings may be raised to abnormally high potential above earth. The neutral may be earthed direct, though a comparatively low resistance will usually be preferred.

2. *High-voltage Overhead Transmission Lines.*—In this case the number of circuits supplying any particular district is ordinarily very small, and as an earth on any one wire is not likely to develop into a short-circuit between phases, considerations of continuity of service will usually determine that the high-tension system should be run without an earthed neutral and with both windings of the transformers connected in delta.

3. *Distribution Circuits.*—When the working circuits are supplied direct from a low-voltage generator, the advantage in earthing the neutral is the possibility of cutting off immediately any defective feeder, and when the necessary apparatus is provided for this purpose earthing will in general be preferred.

When the working circuit is supplied from a transformer stepping down from a high-voltage line, the neutral should always be earthed.

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DISCUSSION.

Mr. H. BRAZIL: I think that this is the kind of paper we really want: it is clear; it is entirely fair; it has no particular bias one way or the other, and it reviews the whole subject. I admire the author for putting forward the disadvantages of an apparatus of which he used to be fond, namely, the electrolytic lightning arrester. He mentions that such arresters introduce trouble because they cause surges on the system. I think if any author includes such a statement when it is not absolutely necessary, that shows how fair he is. Mr. Brazil.

I find myself mainly in agreement with the author; but there are some points upon which I should like to make a few remarks. Dealing first with systems supplying a network of underground cables, with which I have had most experience, I am entirely with Mr. Peck when he says that the neutral should undoubtedly be earthed. In the Company with which I am connected we started with the neutral insulated, but owing to the enormous rises of pressure that we used to get we had to earth the neutral. This, in conjunction with the provision of suitable lightning arresters, had a very beneficial effect, and we have retained the earth ever since. There is a very distinct advantage, as mentioned by Mr. Peck, in having the neutral earthed, owing to the fact that lightning arresters may be set to spark at a very much smaller increase above the normal pressure than is possible where the neutral is insulated; and further, lightning arresters are much more certain in their action in a system where the neutral is earthed. They are also much cheaper, because less apparatus is required. Convinced, as I am, that we ought to earth the neutral point, I am more than ever convinced that the proper thing to do is to earth it through a resistance. Why do we earth the neutral point? I think the answer is given on the first page of the paper, where it says that the advantages are, first, "The limiting of the voltage between line wires and earth," and secondly, "The possibility of cutting off any wire or feeder in the event of an earth upon it." Taking the first point, I should like to emphasize the fact that, until an earth occurs, the neutral point is just as fixed with a resistance in circuit as without a resistance; and all the advantages that you get from earthing the neutral point exist with the resistance in circuit. Further, when one phase goes to earth and current passes through the resistance I cannot see that the potential of the other phases above earth can exceed that which normally exists between phases. With regard to the other point, cutting out the feeders, why should we employ more current than is necessary to trip the automatics

Mr. Brazil. on the faulty feeder? It is clear that if we have a system with the neutral point earthed without any resistance we may get ten, twenty, or thirty times as much current as is really necessary to trip the automatics on the feeders. This, of course, is very bad, as the large current causes surges in the system, so that other breakdowns may occur elsewhere. It therefore appears that there is no really serious disadvantage in putting a resistance in the neutral earth connection; and there are very distinct advantages in doing so. Coming now to the resistance itself, I venture to suggest that iron grids are not the best type of material to use, chiefly because iron has a positive temperature coefficient, *i.e.* it increases in resistance as it heats up. If the current that passes at the moment the fault occurs is not sufficient to trip the automatics, it is clear that they will never come out, because the resistance goes on increasing in value and the current is less after a few seconds than it was at the beginning. Consequently, unless the initial current is large enough, the following currents will certainly not be so. It is necessary, therefore, to make this resistance very much smaller in ohmic value than would otherwise be desirable. First take the figure (300° C.) the author gives as the temperature to which this earthing resistance of the grid type might be allowed to attain. I am not absolutely sure, but I believe that the increase of resistance of this iron grid would be something like 50 per cent—by the way, I do not think it is pure iron. We then have to consider that the fault may occur at the end of the very longest feeder, and the resistance of that feeder itself has to be considered. Further, we have to consider the contact of the fault itself, and finally the earth return. Adding all these resistances together, it would appear that it is necessary to have a resistance which is one-half or one-third of the original value that is got by taking the voltage to earth and dividing that by the current required to trip one automatic. Take a case in point; say, an 11,000-volt system, with 6,000 volts approximately to earth, and an automatic cut-out set to operate at 250 amperes. The resistance is 6000/250, that is, 24 ohms, assuming no resistance in the feeder or the contact, or anywhere else. But since it is necessary to have one-half or one-third of that value, we have to make this iron grid resistance something like 8 ohms. From this it would appear that what is really necessary is a resistance which has a negative temperature coefficient, *i.e.* which decreases in ohmic value as it heats up. In the discussion on Mr. Rider's paper about three and a half years ago* I described such a resistance, but it was then only on paper. Since then, however, these resistances have been in service, and I think it will be of interest if I describe a resistance which has been in use for 2½ years on the Lancashire Electric Power Company's system. Fig. A shows the resistance in question. It consists of 72 fire-clay troughs, 22 in. long and 8 in. wide, filled with a special carbon powder. At the ends are carbon terminals in which flexibles are embedded: and these flexibles serve to connect the troughs together. The troughs

* *Journal of the Institution of Electrical Engineers*, vol. 43, p. 330, 1909.

are separated from each other by small pieces of asbestos millboard, and they are grouped eight sets in parallel and nine in series. This resistance will pass, on the system in question (11,000 volts between phases, 6,000 volts to earth), 200 amperes at the moment a fault occurs. One naturally asks, Does the thing work? I have written to Mr. C. D. Taite on the matter, and he tells me that he is quite satisfied with the working of the resistance, and that it has done away with a great deal of the trouble previously experienced with an iron grid resistance. He further mentioned to me that owing to the resetting of the trips on the feeders it has been necessary to reduce the initial current at the moment a fault occurs from 200 amperes, for which it was designed, to 50 or 60 amperes; but he found no difficulty in obtaining the necessary ohmic value by regrouping the troughs. It is quite easy. In fact, the Mr. Brazil.

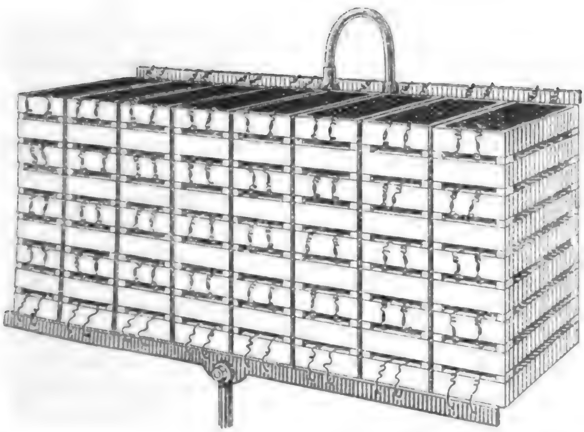

FIG. A.

resistance is like a huge box of bricks; castles can be built of different shapes and resistances obtained of different ohmic values.

Coming back to the comparison of the two resistances, I would point out that it was necessary for the iron grid resistances to be of as low a value as 8 ohms in order to work on this particular system; that would give 750 amperes when a fault occurred. The carbon powder resistance, however, had a resistance of 30 ohms, which allows only 200 amperes to flow, that is, 750 amperes in the one case and 200 amperes in the other. I venture to think that the shock to the system in the one case would be very much less than in the other, and probably there would be no trouble due to surges. Another rather interesting point is that if a fault clears itself effectively, that is to say, if the arc goes out at once, from the point of view of continuity of supply—which is the great thing that station engineers have to consider—there is no reason why the feeder should be cut off. One naturally says, is it possible to get a fault which effectively cuts itself off? I fancy so,

Mr Brazil. if the case of accidental earths in a high-tension chamber is considered. We in the early days of our supply had a very considerable amount of trouble from this cause, due to cats and rats, a number of whom were electrocuted in the high-tension chamber, much to the detriment of our supply. Taking the case before mentioned, viz. cut-outs set at 250 amperes, and a resistance passing 200 amperes, it is clear that if the fault immediately clears itself the feeder will not be cut off; but if the fault remains on, the carbon powder resistance will in a few seconds decrease in value sufficiently to allow 250 amperes to flow, so that the faulty cable will be cut off. It therefore acts as a time limit. I do not think I mentioned in describing the resistance that the latter has no screwed contact of any kind whatever; it is practically indestructible, being constructed of fireclay and carbon powder, and there is no possibility of breaking the circuit.

Dealing further with the paper, I wish to thank the author for reminding us that the current which flows between the neutral points of generators connected together is divided between the three phases. Most people are apt to forget this when they are considering the question of circulating currents; and, as the author pointed out clearly, the heating effect due to these circulating currents is really quite negligible in most cases. I therefore agree that the best possible thing is to earth the neutral points of the generators solidly to a bar and to connect this bar to earth through a resistance. If it is absolutely impossible to have the generators without some resistance between their neutral points, it is, of course, easily done, and I think the resistance might be quite small. Another real danger that Mr. Peck points out, is due to the fact that we may have a system where we transform up and where one terminal of the transformer is connected and not the other; with the result that we get a very high potential from the low-tension windings to earth. I fancy this is a matter which is not sufficiently well known. It may have accounted for a large number of breakdowns which had previously been rather difficult to explain. There is no doubt that in those cases earthing is absolutely essential.

Mr. Chamen. Mr. W. A. CHAMEN: The case in which I am interested is one where both underground cables and overhead lines are used; perhaps I have, therefore, not been quite able to follow Mr. Peck's arguments in the case of a transmission system in which only overhead lines are used. What we have had to do is to earth the neutral of one alternator only. We are not in that happy position of being able to connect the neutrals of all our alternators together. I do not know of any one at present who is in that happy position. With regard to earthing through a resistance, that question has just been receiving our most careful attention, and, although up to the present we have worked without any resistance, we have now decided that we must install one. Obviously, as the installation increases, the amount of breakdown current passing becomes more and more serious, although I suppose that by earthing the neutral point of only one alternator we do put

a limit on the amount of current which can pass. The reason why in our case we decided to earth the neutral point (which for some time was unearthed) was because we were troubled with breakdowns, not in the mains, but in the sub-station equipments in which there were some weak places that have now all been made secure. At that time we never had one breakdown alone; if there was a breakdown at all it was always a double one. Such breakdowns also did not occur in the same sub-station at the same time; we would have one at one end of the system and the other 10 or 15 miles away. This condition of things was quite intolerable. So we earthed the neutral and then succeeded in having breakdowns, at any rate, only one at a time, which was a great relief. I want to ask the author if he has considered the question of earthing one phase instead of earthing the neutral point. I know it sounds a horrible thing to suggest and it is looked upon as a sort of heresy; but I will draw his attention to the fact that in the case of overhead lines he recommends no earthing at all and then goes on to say that, in the event of a mishap, it is possible to run for some time with one phase earthed. It follows, therefore, that Mr. Peck contemplates making the insulation of the lines in that case sufficient to stand the full voltage, otherwise it will not be possible to run at all; the whole system will break down. Now I want to ask, Is the increased cost of insulation required theoretically—I put it theoretically because I do not believe that practically there is anything extra required—if we earth one phase, a serious matter? Does not Mr. Peck really think that in any case the insulation of the three phases must be sufficient to stand the possible contingency of one phase going to earth even if we do resort to earthing the neutral, but through a resistance? The point is: Must we not incur just the same cost of insulation no matter whether we earth one phase or the neutral? I am alarmed at the idea of running even an overhead circuit with what we should call a breakdown on it. Our view, although I admit it is not perhaps based on what Mr. Peck has put before us, has always been that if there is a wire down to earth, anywhere, the sooner we get the circuit off, the better. We do not consider it at all safe to run under those conditions. Perhaps Mr. Peck has in mind some systems in other parts of the world; I think that anywhere in the British Isles the rule would be that if there was a breakdown of insulation of one phase the whole circuit ought to be cut off. Mr. Chamen.

Then with regard to the sub-stations, we have in most cases in South Wales installed three single-phase transformers coupled in delta, and the secondaries are mostly wound to give 2,200 volts between phases. We have no neutral to earth. In supplies to collieries and other places it seems to me it would be a great advantage if we were allowed to earth one phase and pay attention only to the insulation of the other two. We should then be able to use automatic devices which, unless we put in an artificial neutral, we are absolutely prohibited at present from using. I regret to say that that is the

Mr. Chamen.

position under the last issued Mining Regulations, and I cannot help thinking it is a mistake. With regard to lighting transformers, we have a certain number of these in use now, the primary pressure being 11,000 volts and the secondary 400 volts, arranged to give 230 volts from each phase to neutral. We have made these transformers 3-phase star-connected, and we work with a 4-wire system. In these cases we put the neutrals solidly to earth. No trouble arises, and I quite agree with Mr. Peck that it seems to be absolutely essential from the point of view of safety to earth the neutrals solidly in such cases as those.

Mr. Partridge.

Mr. G. W. PARTRIDGE : The question of earthed *versus* unearthed neutrals is a very important one. I was going to ask exactly the same question as that asked by Mr. Chamen, namely, Why the author has not considered the question of earthing one phase entirely ? It appears to me that in earthing one phase we shall get over a great many difficulties. There would be no third harmonic in the neutral conductor when alternators were running in parallel with their neutrals connected up. Such an arrangement would reduce the cost of switchgear, as it would only be necessary to deal with two phases instead of three. I feel sure we are all agreed that running three phases everywhere in our substations and installing fuses, isolating switches, and oil switches on all three phases, to say nothing of placing each phase in a separate compartment, is a very complicated matter. If we had to deal with two phases instead of three, I think it would be a great advantage, and I should like to know what Mr. Peck thinks about that. I quite agree with his remark in regard to earth-shields for transformers. I think earth-shields are very dangerous, and I hope that all engineers who think of using them will refrain from doing so. I have had exactly the same experience as that he describes, the burning away of the earth-shield at the time of the fault, leaving the primary and secondary windings of the transformer in connection. The author has referred to the unearthed neutral in the high-voltage transmission line. I should like to ask him what would happen if a transformer breaks down and the primary makes contact with the secondary ? Also, what is there to cause the faulty transformer to be cut off ? It appears to me that the fault will remain until an earth appears on one of the other phases, and that there is the danger of charging up the secondary or low-tension circuit to a dangerous potential, and of people getting serious shocks unless the earth resistance is low enough to blow the fuse or to trip the circuit-breaker. On page 153 the author describes a method of constructing the resistance in series with the neutral of the generators. Has he considered the question of putting a spark-gap across the resistance, so that if the pressure rises across this resistance more than a predetermined amount the current jumps across the spark-gap and either trips a switch or short-circuits the resistance, putting the neutral point of the generator direct to earth ? I have had some experience in this matter myself, and such an arrangement as I have described works quite well. The author suggests that a

spark-gap might be used on the neutral of generators if it is not desired to connect the neutral permanently to earth. I suggest that, instead of using the spark-gap alone, an arrangement is made whereby the spark-gap is in series with a solenoid which causes a switch to act and puts the neutral to earth, instead of the main current going across the spark-gap itself. I find in practice that when the spark-gap alone is used the short-circuit current of the generator either melts the whole thing up solid or blows the apparatus to pieces. The author concludes his paper by saying, "when the working circuit is applied from a transformer stepping down from a high-voltage line the neutral should always be earthed." I should like to suggest an addition to this paragraph, viz. "In cases of sub-stations or transformers above a certain capacity a definite path back to the generating station should be provided." I show in Fig. B a single-phase system, as it is simpler. The diagram illustrates a single-phase generator, with one pole earthed Mr. Partridge.

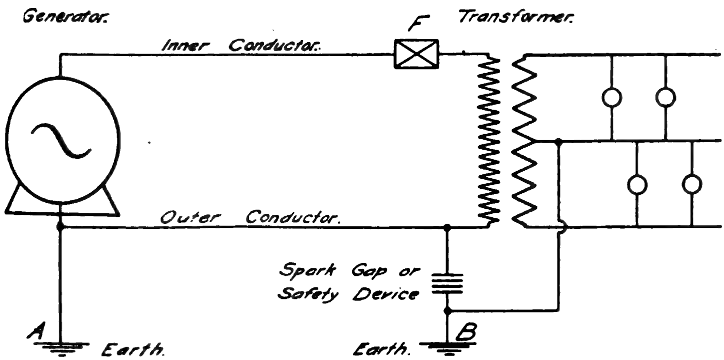

FIG. B.

at A, supplying an extra high-tension system. The transformer is shown connected to the end of the high-tension feeder. The low-tension side of the transformer supplies a 3-wire system with a neutral point earthed at B. If the transformer fails and the high-tension current gets on to the secondary winding, the earth at B will possibly be of low enough resistance to allow a sufficient current to flow back to the generating station to cause the fuse or circuit-breaker F to act and to cut off the faulty transformer if it is of small capacity. If, however, the installation is a large one, say over 500 kw., and the circuit-breakers or fuses only act with a large current, it is quite possible, if the resistance between the two earths A and B is at all high, to have the ground round the point B charged up to a very high potential, the highest potential being near the point of earthing. I have known a case in one of our sub-stations in which the whole of the floor was charged up and men received shocks from one foot to the other when walking across the floor; horses have also been killed

Mr. Partridge.

by standing near faults, where the ground had been charged up due to a fault to earth. By placing a spark-gap or safety device between the earth-plate B and the outer of our high-tension system, we provide a metallic return to the generating station, which at once causes an automatic switch or fuse to act and cut off the faulty apparatus. Such apparatus also prevents undue strain being placed on the insulation of the outer conductor of a concentric system which has its outer earthed at the generating station only.

Mr. Edgcumbe.

Mr. K. EDGCUMBE: There is one point on which I wish Mr. Peck had given us rather more information, namely, the adaptability of the two rival systems to the provision of some means for obtaining a continuous indication of the state of the insulation. I fully admit that, where it is merely a question of safety, it is quite sufficient to provide circuit-breakers which will cut off the faulty section on the occurrence of a fault, but this, it seems to me, is very like locking the stable door after the horse has been stolen; and, from the engineer's point of view, which, as Mr. Peck says, is continuity of service, it is of the greatest moment to have some means of seeing how the insulation is standing up from day to day. In some respects an insulated system lends itself more readily to this. As the author has pointed out, the potential difference between each main and the earth is dependent upon the insulation of that main, and this leads to a very simple method of determining the insulation continuously. One of the simplest methods is the well-known one of connecting a voltmeter, preferably an electrostatic voltmeter, between each main and the earth; and this is coming very largely into use. At one time engineers were much prejudiced—and rightly so—against electrostatic voltmeters; and I must confess that ten years ago they were terrible monstrosities, the climax being reached, I think, when the makers provided spark-gaps in order to protect them, apparently quite forgetting that in other parts of the installation thousands of pounds had been spent in trying to keep the two phases apart! All this, however, is now a matter of the past, and the modern electrostatic voltmeter is really a very workmanlike instrument. In the first place, it is not as a rule connected direct to the mains at all, but is, for high-tension work, generally connected through a condenser which acts inductively; the condenser is often buried in a porcelain insulator, and the voltmeter or indicator, as the case may be, is contained in a cast-iron case which is earthed, so that the whole is practically indestructible. It may be of interest, perhaps, to say that I happen to know of 200 electrostatic instruments which have been installed in the Midlands within the last two years, and up to two days ago, when I inquired, not only had they not broken down, but they had given no trouble whatever. The system is working at 5,000 volts. Given a satisfactory indicator it seems to me that where the system is insulated it is certainly wise to provide something of the kind, because the readings of such indicators can then be logged daily, thus giving the engineer an insight into how things are going. On an earthed system there are various methods of obtain-

ing a similar indication, one of the simplest being that shown by Mr. Peck in Fig. 1, the only difference being that in the common return of the three-current transformers (in series, that is, with the trip coil) an indicator is inserted which is nothing more or less than a low-reading ammeter; and this indicator shows the out-of-balance current, that is to say, the leakage passing back through the earth. I might mention also in connection with that diagram, that, whether an indicator is inserted or not, these three transformers do not usually form an additional equipment, because they generally exist already for working the overload relays on the feeder; so that practically the author's proposal of a trip coil in the circuit, or my further suggestion of adding an indicator, does not in any way increase the plant required. I certainly feel that since such indicators can be so easily added, either to an insulated or an earthed system, it is a mistaken policy not to include them. Mr. Edgcumbe.

Mr. J. SHEPHERD: I have only a few remarks to make, and they deal with my experience of 3-phase working. I think any one working with a large network of lead-covered cables would like to see as little disturbance on those cables and as little earth current passing along their lead sheaths as possible; for that reason it is certainly desirable to have some means of earthing the neutral, preferably through a resistance. Mr. Peck indicates in his paper that we might in the case of several generators solidly connect the neutrals of those generators together. I do not know if he thinks that can be done in all cases. I have looked up a good many references on the point, and I find very few cases where that has been done. In such data as I have been able to get I find that the difference in potential between the so-called neutrals of two similar machines—two 6,600-volt machines—has been as much as 500 volts, and I think with that potential difference it is certainly not desirable to connect the neutrals together. Mr. Shepherd.

Some very interesting advice was given about three years ago in the paper read by Mr. Rider * on earthing the neutrals of generators, and it has been described rather inaptly by Mr. Brazil as a paper method. I thought the details might be of interest to the members, so I have brought with me the drawings of the resistances and automatic switches, and they are now on the wall. The resistances are installed at Greenwich generating station, and have been working now for some three or four years with every satisfaction. In the discussion on Mr. Rider's paper Mr. Brazil raised some question about the cost of the apparatus. He showed us a very interesting example of his own type of resistance, and a very excellent one it was too, but he gave us no indication of its cost. I shall go a little better than that and say that the resistance which we put in at Greenwich cost £120, has a capacity of 500 amperes, and a resistance of about 3½ ohms, so that I do not think it is so very expensive after all. The automatic switch cost £45, and I do not think that is a very serious item. Remarks have been made about running a 3-phase system with one of the legs earthed. I

* *Journal of the Institution of Electrical Engineers*, vol. 43, p. 235, 1909.

Mr. Shepherd.

think this can be done with certain advantages, but I do not think it is advisable to do it if one of the legs is earthed, as it may be through an arc, to the lead sheathing; and that is what I think will occur if the neutral is not properly earthed and the circuit is protected by relays. I consider that is a strong argument for earthing the neutral.

Mr. Brazil.

Mr. BRAZIL: May I correct an impression under which Mr. Shepherd is labouring? He referred to me as if I had said something about Mr. Rider's resistance being a paper resistance. I was simply stating that my own resistance had not been made at the time when Mr. Rider read his paper. As regards cost, the resistance I have described to-night is capable of absorbing 1,200 kw. at the instant the current comes on, and 2,000 kw. at the end of one minute. I should not much mind if the resistance was left on for five minutes. The cost of it was approximately £40, so that those figures will compare very favourably with Mr. Shepherd's.

Mr. Sparks.

Mr. C. P. SPARKS: I find myself in general agreement with Mr. Peck in regard to nearly the whole paper. My experience during the last 25 years, since I was connected with Mr. Ferranti's early experiments in earthing the neutral, has been almost entirely confined to working with the neutral of 3-phase or the outer of concentric cables earthed, so that I am not really able to deal with the matter from the point of view of the unearthed neutral. This subject appears to be divided under two heads. The first is the question of the safety of the public. If we consider the question of overhead wires, the character of the United Kingdom and the dense population makes it essential that the neutral should be earthed on all overhead systems. Although, by insulating the neutral, we can maintain supply with a partial fault on one conductor, there is a serious risk to the public; and for that reason I think it essential that in every case where overhead wires are used the neutral should be earthed. The other point is with regard to security of supply. At the commencement of the paper Mr. Peck suggests the value of earthing, by limiting the voltage between the line wires and earth. I think the advantage is hardly strongly enough brought out. I agree that the generators and motors and all other apparatus should be insulated to withstand the full pressure to earth; but if we have a system with an unearthed neutral we have an unstable system, as the pressure sways about, first one point and then another being raised from zero to full potential, while if a fault occurs at one point it at once raises the pressure at another part of the system and a breakdown may occur several miles away. The security of supply is immensely improved by earthing the neutral, as the faulty section can be cut out immediately, without damage to the rest of the system. The last point is with regard to the supply to the consumer. It is generally at a low pressure of from 200 to 400 volts. While it apparently adds to the danger of the individual to have the outer earthed, the security gained by the absence of risk of the high-pressure system charging up the low-pressure system is so great that the general safety to life is improved by earthing the neutral. I am heartily in agreement with Mr. Peck on that point.

Mr. ROBERT NELSON: This paper appears to me to possess great interest from a mining point of view. I was disappointed at first to observe that Mr. Peck is of opinion that among operating engineers there is great difference of opinion upon the point raised by his paper. I am going to ask him to agree with me that in mining conditions there should be no such difference of opinion. The two systems mainly in use in mines are those in which a supply is given from a generator or from step-down transformers. I was glad in reading the paper to observe that, as regards both those systems, Mr. Peck is decisive; he recommends the earthing of the neutral. But on summarising his paper he appeared to me to qualify that conclusion in connection with supply from a generator. May I suggest to him that in underground conditions, where there is a roadway which is partially filled with timber, and where there is the condition that a sharp stone might pierce a cable and so damage it as to cause an external arc in the presence of a cloud of coal-dust, the desirability of cutting off the pressure immediately is of the first importance? The neutral points of colliery installations should be earthed so as to enable quick-acting protective devices to be used. Mr. Nelson.

Mr. A. P. TROTTER: The consensus of opinion which is being brought out at the various meetings upon this subject is of very great importance. The question of the safety of the public does not arise in connection with underground mains. It was the practice for a good many years to follow the American custom of earthing the neutral, and the first case that I heard of of a large concern with a free neutral was the Central Company's Works at Grove Road, where in June, 1901, it was suggested to work with a free neutral. That has been followed elsewhere. I am not quite sure what the date at Manchester was, but it was about the same time; and a good many other stations have been running also with a free neutral. If engineers choose to run in this manner, there may be some advantages in it in the case of underground cables, but I have not been convinced that there is any sufficient balance of advantages. When we come to overhead work, I think the conditions are different. I cannot help thinking that Mr. Peck has in mind foreign work—perhaps American work—where with dry climates and long distances the conditions may be different. It may be worth while on a long line to run with a fault. All that it amounts to is this—one wants to have a free neutral merely for the sake of running with a fault. The earth current we get on these faults is a matter on which information should be obtained. I do not know with what leakage current to earth Mr. Peck suggests we should run. I should like to know that current, because I should like to see a trip put in the neutral connection which, if an appreciable current were passing, would cut the line off. I do so not on account of those leakages which may burn through an arm or pole, but I am thinking of the wire coming down. I do not believe there is an engineer in this country who wants to go on running a 3-phase transmission line with one wire fallen to earth. I am sure every Mr. Trotter.

Mr. Trotter. engineer agrees that the sooner he shuts down under those circumstances the better. I should like therefore to see a trip of some sort devised which will shut down an overhead line with the minimum possible current to earth—I do not care whether it is reckoned in fractions of an ampere or as a percentage of the full load as long as something below the current that anybody wants to run upon is chosen. That may be done by the arrangement referred to in Mr. Peck's paper. Of course, it is done every day with the Merz-Price or the divided-conductor device; but for smaller lines something simpler is wanted, and with a resistance in the neutral circuit I do not see why we should wait for an overload or dead short-circuit. We might have a high resistance and a trip which would open the circuit on a comparatively small current. The voltage need not be tied down so firmly as on underground cables. Mr. Peck speaks of the trip not being able to discriminate, but with an overhead line I do not think there is any discrimination wanted. Single-phase working is no good if the wire is broken.

I am sorry that Mr. Partridge did not tell us rather more about working with the 3-phase generator supply and one phase earthed, because it seems that the whole of his supply from Deptford to the Brighton line is carried out on a very large scale in that way. [Mr. PARTRIDGE: The 3-phase system, quite independent of the Brighton line, is also carried out in a similar way with one phase earthed].

There there is discrimination. There is only one earthed phase with, I believe, the very important protection of a spark-gap at the far end in case the conductor gets broken, and there is discrimination of the other two phases. I am not speaking of mining; there may be disadvantages there, but for ordinary transmission systems there must be a great deal to recommend it. It would be interesting to hear the experience which has been obtained with that arrangement on the Brighton line. The only way in which this question of earthing the neutral appeals to me is on overhead line work, where, I think, the time has come when, by means of the current passing to the earth on the neutral, we ought to be able to put in a trip which will shut a line down when it falls to earth. That will greatly facilitate overhead work, as it will enable overhead wires to be used in places where at present they are not suitable.

Mr. Pearce. Mr. S. L. PEARCE: As Manchester has been referred to by Mr. Trotter, it may, perhaps, be interesting if I give a few details in connection with our experience. We started in 1901 with the unearthed neutral system, and are running to-day with a free neutral. From what I have heard I should imagine that Manchester is probably one of the largest systems running under these conditions at the present time. I suppose that the designers of the system in 1900 decided that it was better to start with the unearthed neutral system rather than to earth, as the problems in connection with this matter had not been so definitely thrashed out at that date as they have been since. The two

Mr. Pearce.

fundamental ideas obtaining at that time were, first of all, that the unearthed system was certainly safer in preventing fatalities, and secondly, that it would be possible to continue supply even if one phase became earthed. With regard to the first point, I think our experience for the first five or six years has fully borne out that assumption. We had quite a number of cases where the operators came in contact with live metal, but we never had a single fatality for the first five years. We cannot say, however, that that is the case to-day; and the explanation is, that as the system has grown, so the capacity current has increased. There is little to choose, therefore, so far as the Manchester system is concerned, between running with an earthed neutral and running with a free neutral (as is actually the case), from the point of view of the safety of human life. With regard to the question of being able to run with a fault on one phase, we did that on several occasions during the first few years, and it certainly was a substantial benefit to us. But at the present time this is not possible, and we have had to reconsider the question; in fact, we have decided to earth the neutral point, solely with the idea of limiting the current that can possibly pass. This becomes a most important question when the capacity of the station has grown to such proportions as in the case of Stuart Street Station at the present time. Plant totalling forty-five to fifty thousand kilowatts is installed; and with that power behind a fault it becomes next to an impossibility to clear a fault on an unearthed system satisfactorily. I do not mean to say that we are unable to interrupt the circuit, but it is very rough on the switch. During the last twelve months we have made use of a balanced protective system with relays, such as Mr. Peck describes at the bottom of page 151; that is to say, we work entirely on the charging current of the system. Notwithstanding the fact that the system is not earthed, we have tested these relays and have found that they will act perfectly satisfactorily whenever any out-of-balance takes place. With regard to the three possible systems of earthing, I should like to put in a plea for earthing through a high resistance and combining that with balanced protective gear of the order of, or of a similar kind to, that shown in Fig. 1. I believe that a high resistance is better than a low resistance; when I say high resistance I mean something of the order that would limit the maximum possible current to about 150 amperes. It would, thereby, first ensure that the main oil switches would clear every fault that could come on to the station even with such an amount of plant as we have now installed. And secondly, the great advantage is that this balanced protective gear can be set reasonably low, so that the fault can be cleared off the system before it has had time to attain to anything like large proportions. That is what I am proposing to do in Manchester for the future, namely, to use a high resistance in combination with balanced protective gear. If a low resistance is used, something of the order of 6 ohms, then it is possible for a current of 500 or 700 amperes to pass. Of course, the resistance must be so proportioned as to allow of sufficient current passing to trip the largest circuit-

Mr. Pearce. breakers that are installed, and this current will, I think, be found in the majority of cases, when using feeders of 0·25 sq. in. section, to be something like 500 to 700 amperes.

The author states that the majority of the breakdowns of cables are to earth. It would be interesting to know whether that is actually the experience of engineers in charge of large underground cable systems; whether it is a certainty that the cables always do go to earth first. If they do, then, of course earthing in combination with balanced protective gear will be a great safeguard. If the faults do not originate as earths, but involve the other phases first—or even supposing the cable does go to earth, but that there is a very small time limit between the earth taking place and the other phases being involved—then it seems to me that the value of earthing rather diminishes. Unquestionably on the whole, I think that engineers in charge of large undertakings will be driven to earthing their systems either through a low resistance or through a high resistance; and that of the two, for the reasons I have stated, I think the latter is to be preferred, in combination with some sort of balanced protective gear.

Dr. Garrard. Dr. C. C. GARRARD (*communicated*): With reference to the two advantages of the earthed neutral which Mr. Peck gives, the limiting of the voltage between the line wires and earth is only of practical importance on very high voltage overhead transmission lines; and even with these Mr. Peck says it is bad practice to avail oneself of the possible economy which this gives. Moreover, in his conclusions (No. 2) he decides that high-voltage overhead transmission lines are best run without an earthed neutral. The sole advantage, therefore, of the earthed neutral which we need consider, at any rate in this country, is (*b*), viz. the possibility of cutting off any wire or feeder in the event of an earth upon it. Considering the case of a generator feeding a network of underground cables when the neutral is not earthed, the author points out truly that an earth on one phase is almost certain to develop into a short-circuit between phases; and the very large current which flows under these circumstances has generally very bad effects. It appears to me that almost the same result will happen if an earth occurs on one phase with the neutral "dead-earthed," as under these circumstances a dead short-circuit of one phase will occur. If, therefore, earthing of the neutral is adopted it must be through a current-limiting resistance. With the neutrals of the generators connected solidly to a busbar, which is earthed through a resistance, Mr. Peck says that switches must always be provided between each generator and the neutral busbar. I should be glad to know what type of switch he would recommend for this purpose. I take it that a high-tension isolating link operated by a long arm would be the most suitable, as although it may have to break a high-tension circuit the current flowing would be negligible. The link, however, should be situated in the high-tension cubicles and be insulated to withstand the full voltage to earth. I agree with Mr. Peck that the simplest method is to connect the neutrals all together on to one busbar, earthing this through a resistance.

Even with turbines, however, heavy circulating triple-frequency currents may flow. A case which came under my notice was with water turbines. Efforts were made to test the generator wattmeters under inductive load by altering the relative excitation of the machines. This, however, caused large cross-currents to flow through the neutrals which were connected together, thus upsetting the tests altogether. The method of balanced protective gear illustrated in Fig. 1 is very interesting. The author says it may be adjusted to work on a very small earth current. I should be very glad if he would give us some figures in his reply illustrating this, so that a direct comparison with overload protective gear can be made. Would he also advocate the protection of underground feeders by this method alone, or would he install overload protection in addition? The author is to be congratulated on the very clear exposition he has given of the case, (*c*) on page 155, of a generator supplying transformers which step up to a very high voltage for long-distance transmission. He shows the absolute necessity of some form of earthing. I take it the same argument would apply to sub-stations in which are situated step-down transformers feeding converters. An experience which occurred to myself may be of interest in this connection. It was a sub-station fed by step-down transformers having a primary voltage of 10,000 and a secondary of 500 volts. The secondaries of these transformers, although heavily insulated, kept on breaking down to earth, and examination of the point of puncture showed clearly that it was the result of an abnormally high potential. Suspecting the cause of the trouble to be somewhat on the lines pointed out by Mr. Peck, the difficulty was obviated by putting a permanent leak on the low-tension side of the transformers by means of resistances connected between phase and earth. It was not possible to earth the neutral as the transformers were delta connected. I am not quite clear about the point mentioned on page 155, that if "both terminals of the high-tension winding are connected to the high-tension lines the resultant potential of this winding above earth will be zero." Does this not assume that the one terminal will be above earth potential by the same amount as the other terminal is below that potential? But this may not be the case if the insulation resistances, etc., of the lines connected to the terminals be different. Therefore, it would appear that a similar effect may be produced even if both terminals of the high-tension winding are connected to the lines, as the author shows does occur if one terminal be connected, or disconnected, before the other. I fully agree with the author's condemnation of earth-shields between the primary and secondary windings of transformers. I am very doubtful, however, of the utility of a spark-gap in the earth connection. For one thing it is so difficult to adjust. I would suggest that instead of the spark-gap a permanent leak from line to earth through resistance would be better if earthing of the neutral be impossible. Dr. Garrard.

Mr. B. S. HORNBY (*communicated*): I should like to ask Mr. Peck how and where he would earth the neutral point of a 3-phase electrical Mr. Hornby.

Mr. Hornby. high-tension system when two or more generating stations a considerable distance apart are connected together direct by underground cables without step-up transformers in circuit. We must, of course, take into consideration the Board of Trade regulation which states that the connection to earth must be made at one point only. Arrangements must also be made to connect the neutral point of the generators at any one of the stations to earth, for at times the plant in the remaining stations may be stopped. We might arrange for an earth at one station only, and connect the neutral points of the other stations to it by insulated cables, but this would be expensive, especially if parts of the interconnecting 3-phase cables had been laid for other purposes; and there are, of course, objections to sending alternating currents through a single underground cable of considerable length. Each station might have a separate earth, and instructions given for only one to be used at a time; but the attendants might leave the earth switches in at each station, and in that case the above-mentioned regulation would be broken. If, on the other hand, the attendants left the switches out, there would be danger of a system "designed for the neutral point earthed" breaking down; and, unless electrostatic voltmeters were connected, another Board of Trade regulation would be broken. This question has to be considered when two authorities with similar systems and earthed neutrals wish to connect, or when an authority wishes to erect new works in a different part of its district. I may mention that six or seven years ago the Board of Trade would not permit a resistance to be inserted between the neutral point of a 3-phase system and earth, except in very exceptional cases. I shall be pleased to hear that suitable resistances are now permissible.

Mr. Paton. Mr. G. K. PATON (*communicated*): The discussion shows that the question of earthing will remain open, and that the earthing of the neutral will depend very much on the designer and the conditions under which the system is to be operated. When the North Wales Power and Traction Company commenced operations in 1906, this question of whether to earth or not to earth the neutral had not been fully discussed; I doubt if it had been considered at all. After operating for a few months it was deemed advisable to earth the neutral point of the alternators which generate at 10,000 volts, 3-phase, 50 cycles, and which supply the overhead transmission lines without transformers. This was done with a view to limiting the pressure to earth on account of the climatic conditions under which the plant is operated, and which evidently had not been taken into consideration. The neutral point of each alternator was coupled direct to an earth-wire connected to a large copper earth-plate in the river-bed. A few years later, in designing the equipment for the 20,000-volt transmission line, knowing that the neutral point was earthed, we were able to use auto-transformers for stepping up the pressure from 10,000 to 20,000 volts. Two 500-k.v.a. auto-transformers were installed, and have been in use for the last five years. The neutrals of the transformers are earthed, and the alternator earth connections have been

altered to choke coils of very low resistance. It has been stated that with an unearthed system, in the event of a fault on an overhead line the system can still operate with one phase earthed until the fault is repaired. In practice, however, it is extremely difficult to locate a punctured insulator, and the fault may remain undiscovered until a fault develops on another phase and causes a proper fracture of the insulator. The result is then similar to what would have happened with an earthed system with the exception that the original fault would have been located sooner on the earthed system. Mr. Paton.

My point is that when an insulator is punctured, the fault will show on the phase indicator, but the location of the fault is another matter. This can only be done by minute examination of each insulator on the faulty line, and very seldom can such a fault be found by inspection from ground-level. On the other hand, with an earthed system in which all ironwork on the line is connected to an overhead earth-wire, the insulator would show a distinct fracture, which is easily discovered by sectioning the line and inspection. It follows, therefore, that unless there is sufficient time for inspection of a faulty line there is no advantage in having an unearthed system. It only prolongs the evil day in the event of a faulty insulator. There may be some slight advantage where there are single circuits supplying consumers; but if the system is designed to give a duplicate supply to each consumer I consider the neutral should be earthed. This applies equally to cable or overhead systems where there are duplicate services, as it is then better to have a fault located at once. Again, with overhead conductors, where the lines are exposed to atmospheric influences, an earthed system gets rid of those static charges from clouds or wind. If the system is unearthed, then pressure limiters should be used. Regarding the secondary side of the system, I believe in earthing the neutral as a protection against the extra high pressure, and in the earth-wire connected to the neutral as a means of immediately locating a faulty circuit. It also limits the voltage to earth in case of shock. My general opinion then is that all E.H.T. systems should have the neutral earthed, especially the secondary system, both for light and power. A well-designed system may be run with the neutral unearthed, but I prefer a system with duplicate services and the neutral earthed. It must also be remembered, as pointed out by Mr. Peck, that in an earthed system the voltage to earth is limited to 58 per cent of that between phases; and if the system is designed to run with the neutral earthed, the saving in the cost of insulation will be greater the higher the voltage of the system. Resistance should be inserted in the earth connection to limit the short-circuit current to a predetermined value sufficient to operate the trip gear. This limiting of the short-circuit current is essential where high-tension telephones are used with the wires run on main transmission poles. A heavy short-circuit on the H.T. system is sufficient to blow all the telephone fuses.

Discussion before the Manchester Local Section on 19th November, 1912.

Mr. Trotter.] Mr. A. P. Trotter: I have come to Manchester to-night for the purpose of learning from Mr. Peck about a very important subject. Any remarks that I make are with the hope of elucidating the matter and of drawing more information, if possible, from Mr. Peck. I have to regard this subject from the point of view of safety, and my observations must not be supposed to have any official significance. The question of earthing in mines appears to many engineers to differ from earthing above-ground. Since mining is not referred to in the paper I will not touch on that class of work. The Board of Trade Regulations permit the free neutral, that is, the unearthed neutral point. Each case must be considered on its merits. Many stations which have started with a free neutral have afterwards, like the Grove Road station of the Central Electric Supply Company, connected the neutral to earth, and I have not heard of a case where they have gone back from that practice. Those who object to earth the neutral point of high or extra-high pressure do so at their own choice. Where no accidents have occurred the method has so far proved a success, but I find almost invariably that when one accident occurs it is followed by others. From this I am inclined to think that one breakdown is likely to cause another, and I see no compensating advantage. For at high and extra-high pressures it does not seem possible to run for anytime with a fault to earth on a cable. With medium and low pressure a free neutral is permitted by the Regulations, but, except in mines, where the conditions are different, there is hardly any doubt that earthing the neutral point is the best practice. No advantage appears to be gained by earthing the neutral of each generator; it suffices to earth one, and not each in succession as is done at Greenwich. The question of earthing the neutral in the case of overhead wire transmission and distribution must be considered separately. An earth on an overhead line may be due to a failure or breakage of an insulator, causing a leak, or a wire may break and fall to the ground. In a dry climate it may be possible to continue the supply with a line lying on an arm or hanging against a wooden pole, but in this country the result is generally damage to the line as well as stoppage of the supply. Nearly all engineers agree that if an overhead line falls from an insulator and lies on an arm or against a pole it is better to shut down at once and to repair; and I think all are agreed that it is of no use trying to run on when a wire has broken.

Mr. Peck says that it is possible "to put a trip coil in series with the earth connection, but . . . it does not discriminate as to the feeder to be cut off." If it is risky to run with an appreciable leak on an overhead line, and useless to run where one wire is broken, discrimination does not appear to be of any importance. Such trips have generally been arranged for overloads or dead earths, but might we not go

further and trip on a leakage current? The higher the working voltage the greater will be the current to earth. A 6,000-volt line is much safer in this respect than a 2,000- or 3,000-volt line. Unless anybody wants to run with a leak of, say, a quarter of an ampere, or unless it can be shown that harmless leaks of that magnitude are to be found in practical working, the trip could be set for such a current. It would be well to arrange a time limit of, say, one or two seconds, so that a bird pecking at a wire may be killed and disposed of without opening the switch. In conclusion, I should like to say that I consider the time has come when those who are engaged in overhead transmission should look for some method which will ensure that if a wire has fallen to the ground it shall be cut off at once. If also dangerous leaks to stay-wires, etc., can be prevented, overhead work could be used much more extensively than at present. Mr. Trotter.

Mr. W. B. WOODHOUSE: Where a number of generators are in use my view is that their neutrals should not be connected together, the neutral point of one machine only being earthed. In principle, the earthing of the neutral point of a system seems sound, but many of the advantages are based on the assumption that the earth is of negligible resistance. If a fault to earth occurs at a point distant from the station this may not be the case, and the protection expected may not be realized. There are other difficulties with overhead lines on the earthed system. Some people in this country still use the earth as the return for telegraph circuits, and there have been complaints of disturbances merely due to connection of the power system to earth. Even though the system is perfectly balanced, trouble has been experienced from the capacity current in the system. If one line conductor could be earthed all the way along it would get over a good many troubles. Mr. Woodhouse.

Mr. R. BLACKMORE: About six months ago, at Stalybridge, we had serious trouble, due to the fact of connecting the neutral of all our alternators direct to the earth plate. An earth developed on a cable in one of the sub-stations about two miles from the generating station, and the rush of current to earth apparently caused surging, the insulation of two of the alternators failed, and the machines were seriously damaged. After this experience we decided to insert a resistance between the neutral point and the earth-plate. This resistance can carry 300 amperes for a period of 10 seconds, and will allow sufficient current to flow to trip any of the feeder switches instantaneously. Switchgear was arranged so that one alternator could be earthed at any one time, so far as the turbo-alternators were concerned, but the earth-wires of the reciprocating sets were left solidly connected. We found, however, that the interchange of current between the 500-kw. reciprocating sets and the turbo-alternators was large enough to heat up dangerously the earthing resistance. Mr. Blackmore.

Mr. A. E. McKENZIE: The Manchester high-tension system has been running for over 10½ years with the neutral insulated. I have known of several instances where we have run for as long as one hour with one phase earthed before the faulty feeder has been located Mr. McKenzie

Mr. McKenzie. and disconnected. But that possibility does not exist to-day. The high-tension system has extended so rapidly that when an earth on one phase occurs it almost immediately causes a breakdown adjacent to the first earth or on some other part of the system, and two faults actually result instead of only one. That state of things has continued for perhaps a year so, and we are now considering the advisability of earthing the neutral point. It seems to me that there is undoubtedly a good deal to be said for earthing the neutral point, especially in those cases, as at Manchester, where the capacity of the generating plant is increasing rapidly year by year. It is only a matter of time before the load that the circuit-breakers will be called upon to break is in excess of that with which they will satisfactorily deal. If one has a fault in the vicinity of a station, and between 30,000 and 40,000 kw. of plant actually connected to the busbars, a very great power indeed has to be interrupted at the feeder switches, even if the latter are set to operate instantaneously, as they are at Manchester. The actual method of connecting the neutral to earth is one that requires a very great deal of consideration. I certainly should not think of earthing the neutral except through a resistance. Supposing the latter were designed to carry the current required to clear the largest circuit-breaker on the system, it would have to be of enormous proportions, because the 0·25 sq. in. feeders are set to operate with a current of 600 amperes.

Another reason why we have hesitated to make the alteration is because of the enormous cost involved, as every piece of apparatus on our system would have to be fitted with three series transformers instead of two as at present. I think we should perhaps be better served by having a resistance which would only pass a very small current instead of 600 amperes, and have balanced protection on all the feeders, similar to that illustrated in Fig. 1 of Mr. Peck's paper. I should just like to say that, in my opinion, Fig. 1 is somewhat misleading. It appears to me that if generators were protected as there shown, and a fault occurred to earth on one phase of a feeder, all the generators would be immediately thrown off the busbars, if the neutral points of the generators are earthed. I may be wrong, and I shall be glad if Mr. Peck will in that case correct me.

Referring to page 150, "The possibility of cutting off any wire or feeder in the event of an earth upon it," as Mr. Peck says later on in his paper, is not confined to a system having an earthed neutral; in fact, several years ago we fitted two of our feeders with the Ferranti-Field protective devices, which are similar in theory to that shown in Fig. 1, but since then we have not had any faults on these two feeders. In the early days it was rather difficult to locate the fault on any particular feeder, although we had an ammeter on each phase, because of the fault current being out of phase with the load current.

Mr. Watson. Mr. S. J. Watson: I think the question of earthing really wants approaching from two points of view; and, moreover, they are points of view which in many ways are in direct conflict with each other. If we consider only the safety of the public, there is no question, I think,

that earthing is desirable; on the other hand, if we are to study the larger question of continuity of supply, one is bound to say that the less we earth the better. Take, for instance, the Board of Trade Regulations concerning the earthing of the neutral on 3-phase, 4-wire, low-tension distribution and of the middle wire on 3-wire continuous-current distribution; I do not think any of us would earth either the neutral or the middle wire unless he had to, because it must be borne in mind that, when one main is earthed, a fault on any of the other mains means the cutting off of the supply, the one thing which supply authorities try above all to avoid. When Mr. Trotter puts forward a suggestion that some device should be got out that would entirely cut off an e.h.t. overhead line when a fault of one-quarter of an ampere occurs, I am at issue with him from the continuity point of view. A fault of this amount on a 6,000-volt line is not worth talking about, and if we can continue to use the line until another is brought into use we ought to continue to supply consumers through it. These are views which I am quite certain will be accepted by supply engineers. On the general question of earthing the neutral point on e.h.t. 3-phase supply I am one of those who do not earth at all. I cannot say that I have had very long experience of unearthed systems, but certainly so far there has been no trouble of any kind during the two years the system has been in operation. I am rather inclined to think that some of the difficulties which have occurred in connection with the earthing of the neutral on e.h.t. systems has been due in the past to faulty generator construction. Many of us know of cases where a serious short-circuit on a feeder has badly damaged the end windings of the generators through insufficient attention having been given to the bracketing of the windings. Incidental to this question of damage arises another point of some importance. If a bad short-circuit occurs in connection with a continuous-current supply it is very rare indeed for any serious damage to result to the generating plant. Most of us, I think, have experienced short-circuits on 400- to 500-volt shunt-wound machines excited from their own supply, the machines simply becoming demagnetized through the action of the armature-current on the field, and no damage being done; but in the case of alternators we have quite another problem. The alternator has a separate exciter, so that a short-circuit on the alternator does not affect the exciter, which latter continues to maintain the field of the alternator at full excitation. We therefore get currents from the alternator for an appreciable time, the load representing in many cases four, five, or six times the full-load output. Mr. Watson

There is just another point in connection with this matter which perhaps indirectly bears on the question, and that is, the present design of alternators. The matter has been raised on previous occasions, but no change is yet being made in designs; and as e.h.t. alternating-current generation is likely to become much more extensive in the future, I think the question is well worth attention. It is quite a simple matter to design an alternator which will not give much more

Mr. Watson. than its full-load output on a short-circuit. Such machines have certain disadvantages, such as poor regulating qualities, but they could be automatically controlled and would also be cheaper to build; it is therefore a moot point whether the advantages would not balance the disadvantages. I should not be at all surprised if future alternators are designed so that it will be quite impossible to obtain the tremendously large currents which with the present design are responsible for some of the difficulties. Several speakers have referred to the protective devices used on a ring system of e.h.t. mains, which undoubtedly has many advantages; but Mr. Trotter very rightly pointed out that it is not many who can arrange a ring system on the lines of the Newcastle-upon-Tyne Electric Supply Company. I should say that not more than half a dozen undertakings in the country have such a system. The method usually adopted is to use overload trips with some form of time element; and on the whole I think they carry out the duty required of them in a very excellent way. I can only say again that, in my opinion, the question of continuity of supply and of earthing are at issue, and it requires a most careful consideration of the subject in order to arrive at a proper mean between the two.

Mr. Cramp. Mr. Wm. Cramp: I have but little experience of this matter, yet there is one question which I should like to ask Mr. Peck. Reference has been made to the cost of resistances and to the disadvantages attending their use. It seems to me that the main object of having a resistance between the neutral point and earth is to limit the rush of current that can take place. Now a choking coil can easily be designed to take the place of this resistance; and such a coil would not only have the properties of a resistance, but it would be cheaper to construct, it would occupy less space, and it would be particularly valuable in limiting the high-frequency currents. Mr. Peck may reply that surging would result, owing to there then being capacity in series with inductance. I think, however, that if he will work out the choking coil that would be necessary for any given system he will find the proportions to be such as would not result in any violent surging being at all possible. It would be interesting to know whether this arrangement has ever been thought of or tried.

Mr. Wedmore. Mr. E. B. Wedmore: I am particularly glad to see in print the explanation of the reason why troubles need not be anticipated, as a rule, from current circulating between the mid-points of generators earthed on a common busbar, as a good deal of misapprehension still exists in this connection. I have not yet met with a case where it has proved necessary to employ more than one earthing busbar, but perhaps Stalybridge furnishes such a case. [After the meeting I learned that the turbo-generators at Stalybridge are so much larger than the earlier machines that it will be practicable to obtain satisfactory results by earthing only the turbo-generators, so that here again only a single earthed busbar will be necessary.]

The arguments for earthing are becoming more important year by year. The second reason given is increasing in weight, as improve-

ments are made from year to year in the protective apparatus available for isolating a faulty section of the system. I believe I was the first to suggest the employment on e.h.t. systems of the protective device illustrated in Fig. 1, and it is now in use in a large number of installations, under my recommendation. The diagram illustrating the principle does not bring out the full merits of the arrangement. The current transformers shown may be employed to excite time-limit overload devices, which will take care of all ordinary load conditions, including momentary heavy overloads. The trip coil in the common return is excited only on leakage occurring, and may be set to operate instantaneously on a fraction of normal load current. We thus have the benefit of a time-limit device so long as the feeder is sound, and the benefit of an instantaneous device with a low setting immediately a fault develops. This enables faults to be removed without causing general disturbance of the supply system. Mr. Wedmore.

Referring to the reasons given for earthing, I should have added yet a third reason, perhaps equal in importance to those offered, namely, that a system operating with its mid-point earthed is less liable to static troubles. There is a large amount of evidence in favour of this proposition, although the explanation of the matter is not fully clear. There is, however, one kind of static disturbance which admits of a ready explanation, and of which the existence has been well proven. A so-called insulated system is really a system earthed through a star-connected capacity, there being capacity between each phase and earth. The capacity of any pole to earth is in shunt to any fault which occurs on that pole, and in case of an arcing fault we have the familiar high-frequency conditions of the humming arc. Whilst a cable fault is not so likely to possess the characteristics of an open arc, it is difficult to avoid the conclusion that in any case of breakdown on an insulated system involving an open arc, this high-frequency disturbance must be present, sending waves with steep front searching for weak points all over the system.

This trouble is not associated with normal switching operations, but with the development of faults on the system. It is not clear to me that the troubles referred to by Mr. McKenzie at Manchester are not due to the accumulated effect of static disturbances. Even where the mid-point of the system is earthed it is possible to get this phenomenon. It would occur in case the earth connection could not pass a current appreciably larger than could be absorbed by the capacity of the system. It is, therefore, most important to avoid this feature in earthing, and the following somewhat empirical rules will be of service to this end:—In practice, one should not apply a small testing transformer to a cable or system of cables having a capacity current comparable with the current which the transformer can pass in case of a fault to earth. Reactive devices for obtaining an artificial neutral point must also be designed to pass a current heavy as compared with the capacity current of the system. This feature must also be considered in the earthing of generators, and any arrangement avoided

Mr. Wedmore.

which might result in breaking the earth connection at the time there was an arcing fault to earth. Such an arrangement would obviously leave the system in a condition most favourable for the setting up of the high-frequency disturbance above referred to. This feature bears on the design of earthing resistances, as in case such a resistance burns out when one pole has gone to earth the conditions are analogous to those above described. In the past it has been the practice to design such resistances with a margin of safety of 10 to 15 seconds, this being ample to allow time for the operation of any automatic devices relied on to clear the fault. One has to remember, however, that automatic devices do not always operate, and in recent years we have recommended the employment of earthing resistances with a two-minute rating, in order to avoid failure of the resistance. This rating provides a greater margin than at first appears, as a heavy arc will burn a big hole in two minutes, thus greatly cutting down the current and extending the effective life of the earthing unit. The arrangement, in practice, gives ample time for an attendant to take steps to clear the fault. I am generally in agreement with the summary of conclusions given on page 160, but I do not see why the employment of delta-connected transformers on overhead transmission lines is advocated. It is true that such transformers can generally be operated on open delta in case of failure on one phase, but with modern transformers this feature is of minor importance. The employment of a star winding on the high-voltage side reduces the pressure across the windings on each leg, and thus enables the designer to furnish a cheaper transformer with a higher factor of safety than can be obtained with a delta-connected winding. If the employment of a star-delta connection at this point on the system is granted, the rules for earthing the star point are the same as those given for generators, and the whole of the matter can then be summed up in one rule, viz. it is generally desirable to earth the mid-point unless considerations of economy necessitate the employment of a system where an insulated mid-point is necessary in order to maintain supply. Whilst one should not take advantage of the increased factor of safety in such transformers, or in generators, and reduce the insulation, I do not quite agree with Mr. Peck's suggestion that the increased factor can be practically neglected. The possibility of nearly doubling the factor of safety appeals to me as a valuable feature, and a good argument in favour of earthing the mid-point of e.h.t. generators.

It is interesting to ascertain what occurs on an insulated system in the event of one pole going to earth. On smaller systems than that at Manchester my records indicate that the fault develops in the first place almost invariably as a fault to earth. This fault may remain on the system, as a rule, until it has been located, which may take perhaps half an hour. Within this period, in a small percentage of cases, the fault develops into a short-circuit between phases at the point of first occurrence, and in 25 per cent of cases the fault develops on another phase before it can be removed. The majority of faults occur on the

Mr. Wedmore.

apparatus connected to the main, and not on the main itself. It is interesting to have the testimony from Manchester that, when the system had developed extensively, the capacity current to earth was such as to cause almost instantaneously the development of a dead short-circuit between phases at the point of first occurrence, or elsewhere on the system. It would be interesting to know whether the general experience is that the majority of faults occur outside the cable system.

Mr. Faye-Hansen.

Mr. K. FAYE-HANSEN: On page 151 Mr. Peck refers to different kinds of balanced protective gear. Besides the Merz-Price protective gear and the protective gear shown in Fig. 1 there exist other balanced protective gears, as, for instance, those for parallel feeders, mentioned in Mr. Harlow's and my paper * on "Merz-Price Protective Gear," and a new protective gear proposed by Messrs. Merz & Hunter, in which the conductors are divided into two equal parts inside each cable for the purpose of using a protective gear similar to that for parallel feeders. It is likely that further proposals for new balanced protective gears will be made, all of which will require an earthed neutral for operating instantaneously in case of a fault to earth, at least for all ordinary-sized networks. For underground networks supplied through transformers, the same reasons for earthing the neutral of the transformer hold good, as Mr. Peck has given for the earthing of the neutral of generators under similar conditions. Regarding the earthing of the generators used on small overhead systems, I think we should consider the danger of other parts of the system breaking down if run with one side earthed for any considerable period. Under ordinary circumstances there is only about one-half the net voltage to earth, and notwithstanding that the insulation may have been designed for the full line voltage there is always a great danger that weaknesses will develop so that the insulation will break down if subjected to the full line voltage, *i.e.* double the normal voltage to earth. If this takes place we may get a complete shutdown instead of, with an earthed neutral, only one consumer being cut off, or usually one consumer being supplied through one feeder only instead of through two. The reasons that Mr. Peck has put forward for earthing the neutral of generators working on overhead transmission lines through transformers, will hold good for alternators connected to underground transmission lines through step-up transformers. In this case it may be of interest to know that it can happen that the low-tension winding of the transformer will get a much higher voltage to earth than one-half of the high-tension voltage if not connected to earth or to other windings on the low-tension side. This is due to the fact that in many cases the capacity between the high-tension and low-tension windings is very much larger than the capacity between the low-tension winding and earth.

Mr. Peck states that if both terminals of the high-tension winding are connected to the high-tension line, the resultant potential of the low-tension winding above earth will be zero. This is only true if the

* *Journal of the Institution of Electrical Engineers*, vol. 46, p. 671, 1910.

Mr. Faye-Hansen. potential of the high-tension windings is symmetrical to earth, *i.e.* the neutral of the high-tension winding is earthed, and when the arrangement of the transformer windings is such that the capacity between the two halves of the high-tension and low-tension winding is the same. Though this usually will be the case, a fair number of transformers have been designed where the high-tension and low-tension windings are not placed symmetrically to each other in this way, so that the danger of a high potential between the low-tension winding and earth can exist, even if there is no disturbance on the high-tension side.

In regard to the earthing of high-tension transmission circuits, I should like to know if telephone troubles have been experienced in case of earthed neutrals, and whether these troubles have been rectified simply by removing the earth from the neutral. If the generators give a sine wave form, and the arrangement of the transmission line, etc., is symmetrical, such troubles should not occur unless the neutral of the transmission line is earthed at more than one point. There can, however, be no doubt that there have been cases of telephone troubles with unearthed systems as soon as one line has gone to earth, thus making telephone communication impossible just at the moment when it is most important for the power station to be able to communicate with the different switch stations and sub-stations. In case of such high-voltage overhead lines, where there is a breakdown to earth and it is tried to run with one side earthed, the danger of further breakdowns on other parts of the line is very great, due to the causes previously referred to for low-tension overhead lines.

As to earthing by means of an auto-transformer, this is not very often done. I know, however, of between half a dozen and a dozen cases where auto-transformers have been used for such purposes in conjunction with delta-wound main transformers or delta-wound generators. They have been of the interconnected star type, which were first proposed by Mr. Peck. They allow at the same time the advantage of using delta/delta single-phase transformers, one of which may be cut out during trouble, and of an earthed neutral.

Mr. Hollingsworth. Mr. E. M. HOLLINGSWORTH: For the past three years we have been operating, in St. Helens, a 3-phase 6,000-volt system with the neutral unearthed, and so far we have not experienced any trouble. The spark-gaps have come into action on several occasions, and in one instance I am certain we should have had a shutdown if the neutral had been earthed. Before deciding as to earthing or otherwise I made many inquiries and visited several undertakings, and found that quite a number were running with a free neutral. At one of the London stations I was informed that they had recently, owing to cable troubles, found it necessary to earth the neutral. They afterwards stated, however, that the greater part of their cable system had not been intended for operating with the neutral unearthed.

At the last meeting of the Municipal Electrical Association Mr. F.

Ayton, of Ipswich, read a paper on the question of maintaining the continuity of supply, in which he stated he had come to the conclusion that greater reliability was ensured by not earthing the neutral. Mr. Ayton was, no doubt, looking at the question from the point of view of a medium-sized station. It seems to me that the size of the station, and the number and arrangement of the feeders, should be taken into consideration in deciding this matter. In the case of a large station where the plant capacity is very considerable, it does seem desirable to cut out a faulty feeder immediately, for in all probability there will be a duplicate supply available, or, at the worst, only a very small portion of the total area of supply would be affected. In the case of a smaller station, however, the failure of a feeder would in all probability affect the greater portion, or perhaps the whole, of the supply ; and for this reason I am in favour of running with a free neutral, since the possibility of being able to continue operating with one phase earthed, if only for a short time, is, in my opinion, an advantage that outweighs all the disadvantages. Mr Hollingsworth.

With regard to earthing or insulating the neutral of a low-tension system, I take it that Mr. Peck is referring to private installations. Public supply authorities have no option in the matter, for under the Board of Trade Regulations the neutral point of a 3- or 4-wire system must be earthed.

Mr. R. G. Cunliffe : In the first paragraph of his paper Mr. Peck asks, "Where, when, and how should the neutral point or points be earthed ?" The paper does not deal with the methods of earthing or their considerations, and the subject is difficult, the term "earth connection" being a very loose one. Large potential differences may be maintained between different points in the earth, and it is obvious that, since the potential disturbances are mainly confined to the immediate neighbourhood of the earthed points, the bulk of the earth remaining at a constant potential, the correct term to use when discussing earth potential is the term "normal earth potential." Mr. Cunliffe.

Mr. Peck refers to the neutral point being at earth potential. It is at normal earth potential only when there is no flow of current ; the conditions change entirely as soon as a leakage occurs. The reason why a breakdown on one phase of a 3-phase cable develops into a fault between phases is that almost the whole of the phase voltage is concentrated at the faulty point, and the insulating material around that point is, owing to the immense concentration of energy, almost instantly destroyed, resulting in a short-circuit between phases. Earth connections should be made to several plates sunk, close together, at a considerable depth and surrounded by suitable conducting earth always kept in a moist condition.

Mr. W. Bolton Shaw : With regard to the term that Mr. Peck uses—"solidly connected"—I take it that he means to distinguish between earthing with and without a resistance ; but it might be taken to imply that he precludes the use of any automatic protective device between the neutral point of the generator and the neutral busbar. It Mr. Shaw.

Mr. Shaw. seems to me that an overload protective device of this kind ought always to be provided because, in the event of a short-circuit between any of the three phases and earth at the frame of the machine, or on the leads between the machine and the switchboard, there is no control over this current unless the neutral lead can be disconnected from earth. In the event of such a short-circuit occurring it may continue until it burns out the whole of the generator phase windings between the neutral point and the fault, whereas if an overload device is provided it will open the earth connection and prevent further damage to the generator windings than the local burning at the fault. The setting of this device should of course be sufficiently high, either in tripping current or in time lag, to prevent its opening with any external short-circuit to earth that may occur on the system.

With regard to the use of resistance between the neutral and earth, my own view is that for medium- and high-pressure systems it is a mistake to put resistance in at all. The resistance in the contact between the earth-plate and the earth itself is always an appreciable amount, and this added to the resistance of the fault itself is sufficient, in the event of a short-circuit to earth, to limit the fault current without adding a resistance in the neutral. The object of earthing the neutral is to ensure a fault to earth developing into a sufficiently heavy short-circuit to bring out the circuit-breaker; and the placing of a resistance in the earth connection is a step in the wrong direction.

Mr. Trotter mentioned the case of a wire on an overhead transmission coming down; there is no doubt that with an earthed neutral this is a source of danger. A wire may come down by breaking away from the insulator, and may then either fall on the cross-arm or sag down under it and swing against the side of the post. In either case the post may become a source of danger. Or, again, the wire may actually break and one or both ends fall to the ground; in which case there is not only danger to life from the wire itself, but also from the ground in the immediate neighbourhood. This danger can be largely obviated by having the cross-arms of iron or steel, as is usually the case, and bonding them to an overhead earth-wire carried the whole length of the line and earthed at both ends. If horns are provided at the ends of the arms a wire breaking loose from an insulator will be compelled to lie on the cross-arm, and so produce a heavy short-circuit to earth, bringing out the circuit-breakers. To cope with a broken wire, bars can be fixed fore and aft of the posts, parallel to, but in front of and slightly higher than the cross-arms to which they are electrically connected. A broken wire hanging down to earth rests on the bar beneath it, and so produces a short-circuit to earth, bringing out the circuit-breaker.

Professor Marchant. Professor E. W. MARCHANT: Referring to Fig. 1, on page 151, I should like to know if, with that apparatus, there is not trouble due to triple-frequency currents operating the trip. If the neutral points of the generators are earthed and the trip coils carry the circulating currents, these currents will be certain to release the trip. The circuit which carries the circulating currents is one of small impedance for triple-

frequency currents, since with an ordinary 3-phase generator the inductance of the stator, when it carries triple-frequency currents, is due only to the leakage reactance of the armature coils. Professor Marchant.

Discussion before the Birmingham Local Section on 27th November, 1912.

Mr. R. A. Chattock: I have carefully read Mr. Peck's paper, and cannot find in it any fresh reasons for earthing the neutral point beyond those that have already been put forward. I agree that in the case of pressures of 10,000 volts and upwards it is probably beneficial to earth, but below this voltage I do not consider that it is necessary, unless experience with a particular undertaking justifies it. In Birmingham, on our 5,000-volt, 3-phase, 25-cycle system, we have not yet found the slightest necessity for earthing, and we do not propose to do so as long as the conditions remain as satisfactory as they are at present. Mr. Peck said in his paper that the chief objection to earthing is the fact that "the system cannot be operated with an earth on any line wire." I consider that this objection is exceedingly important. While I admit that an earth on one phase generally produces a complementary earth on another phase, resulting in a short-circuit, yet I have known cases in which an earth on one phase has developed, and it has been possible to maintain the supply for some considerable period until this earth has been located and rectified. With an earthed neutral under such conditions a section of the supply would, of course, have been automatically cut off. Maintenance of supply is one of the chief points that supply undertakings have to consider, and any arrangement that tends to carry this out should always receive first consideration. Mr. Chattock.

Mr. Peck in his paper says that it is becoming general practice to earth the neutral in all stations supplying 3-phase networks; and, dismissing the unearthed systems in this way, he proceeds to discuss those in which the neutral is earthed. I think that this is hardly fair to the unearthed side of the problem, as, in my opinion, there are many points in favour of the latter. The first point, of course, is the possible maintenance of the supply with one phase earthed. The second is the greater safety to human life in case of accident. With an earthed system, if a man touches one of the phases he is bound to be killed, but more than one case has been known in which, on an unearthed system, a man has accidentally touched one of the phases, and although he has been very seriously injured he has not been killed. The protection of the attendants against fatal accidents is such an important point that this alone should carry great weight in comparing the two systems. The third point is the complication and the unsatisfactory nature of the arrangements used for earthing. Mr. Peck describes two or three methods of earthing, and advises that one should be tried; and that if this is found to be unsatisfactory then another should be tried. This does not look as if the methods adopted are at all standardized, or that, indeed, sufficient experience has been obtained in operating them. If

Mr. Chattock. the generator neutrals are connected solidly together, there appears to be liability of large currents circulating in the windings. Mr. Peck mentions 30 per cent of full-load current causing an increase in the resultant heating in the generator of only 1 per cent. But he admits that in certain abnormal cases the current in the neutral may amount to the full-load current of one generator, or even more. The effect of such currents superimposed upon the wattless currents which circulate in the generator windings under conditions of low power-factor must be very serious on the heating of the generators, and must cut down the useful capacity of the machines. These circulating currents apparently depend upon the wave-form of the alternators; and in a commercial undertaking that has to purchase its machinery from different manufacturers it is highly probable that the wave-forms of machines designed by different firms will be dissimilar, the result being that it is impossible to prophesy what quantity of circulating current will have to be dealt with under the condition described above. The alternative is to connect one generator neutral to earth, and this, of course, has to depend either upon the attendant in charge or upon the very complicated apparatus that is installed at the London County Council's power station at Greenwich. This apparatus was described by Mr. Rider in his paper before the Institution some years ago, and, whilst being very ingenious, it is so complicated in its action that under prolonged use there is no doubt trouble would be experienced. Again, with one machine coupled to earth, in case of a fault on one phase it is quite likely that the overload circuit-breakers on this particular machine will be opened at the generating station, and the system will then be running unearthed just at the time when further trouble is most likely to appear, it being well known that when one earth appears on a system complementary earths are likely to occur, due to the strain that is thrown on any weak places in the insulation. Can Mr. Peck give us some idea of the size of resistance that would be required in a large undertaking ? I am under the impression that considerable space would be taken up by it, and this, of course, is not always easy to arrange for in existing power stations. In Birmingham we have designed our high-tension cables to stand safely the full voltage to earth on any phase, rather than with the idea of cutting down the insulation, as is possible with an earthed system. I consider that economy of this kind is far from advisable, and the results obtained in Birmingham have fully justified this provision. Very few breakdowns to earth of any kind have been experienced, and these have always occurred in connection with broken insulators in the switchgear, or coils in generators. I consider, therefore, that for a system using pressures up to 10,000 volts it is certainly better to allow a large factor of safety in the insulation and to run with an unearthed neutral.

Mr. Forrest. Mr. F. FORREST : The general conclusions to be drawn from reading this paper simply amount to this, that in operating any 3-phase system, if it is found advantageous to earth the neutral, then by all means earth it, but if there is nothing tangible to be gained by doing

so, then don't earth it. In considering the earthing of the neutral of any 3-phase transmission system the factor of safety of its insulation to earth should largely determine this point. By the factor of safety of the insulation is meant the ratio between the normal working voltage to earth (in all systems, whether earthed or not, this is $1/\sqrt{3}$ of the voltage between phases) and the voltage to earth which would cause breakdown when continuously applied under the worst atmospheric conditions likely to be experienced. In the case of bare overhead transmission lines working at pressures above 50,000 volts between phases, this factor of safety is usually about 2·5. In order that this factor of safety may be maintained, and also in order to save a large additional capital expenditure for insulators, such transmission lines should always be operated with an earthed neutral. In this country, where voltages are comparatively low and underground systems prevail, the factor of safety is usually much higher than 2·5, and the question of earthing will largely be decided by other considerations. One of the chief points in favour of earthing the neutral, and one which the author has not made nearly enough of, is the question of lightning or pressure arresters. If full use is to be made of these they should be set to spark over at a voltage 25 per cent in excess of the normal working pressure to earth, so as to clear the system of all pressure surges; but if the neutral is unearthed they cannot be set with safety lower than 25 per cent in excess of the full voltage between phases. In other words, with an unearthed neutral the arresters must be set so high that they are never likely to operate at all. This is one point in favour of earthing; but there are plenty of arguments of equal weight against earthing, so that in considering this important question no definite law can be universally applied, and each system should be run in the manner which, on the whole, is found to suit it best. Mr. Forrest.

Mr. S. A. MAHOOD: The subject of this paper is of great interest to power supply engineers, amongst whom there appears to be some difference of opinion as to the relative advantages of earthed and unearthed neutrals. The conclusions arrived at by the author are not very convincing and, I fear, of not much assistance in deciding the question, this being left to the judgment of each central station engineer. My experience with a 3-phase, 5,500-volt, 25-cycle system in which the neutral is earthed "solid" without any resistance has been quite satisfactory in actual practice for the last eight years. We have had no complications due to the way in which the system is earthed, and I see no reason why we should make any change. Mr. Mahood.

Dr. W. E. SUMPNER: The author employs the word "balance" in a new sense. A 3-phase circuit is usually described as balanced when the three line currents are equal. The author has given a new meaning to the term, and it is rather a pity he has not suggested a new word to denote the condition of a 3-phase circuit when, from some cause such as leakage, the sum of the instantaneous values of the three line currents Dr. Sumpner.

Dr. Sumpner.

differs from zero. The paper is a concise and valuable summary of engineering experience and opinion on the subject of earthing the neutral. The fact that there are wide differences of opinion among operating engineers on this subject indicates that many circumstances have to be taken into consideration, and the conclusion arrived at depends chiefly on which of these considerations the engineer looks upon as of dominant importance. Personally, I favour the view that the neutral should be earthed in all cases, except where it can be clearly shown that there is a great disadvantage in doing so. This opinion is based on general considerations of simplicity. A 3-phase circuit really consists of three circuits interlinked, except when the neutral is earthed, when the circuits are essentially separate. In the latter case, a defect in one circuit is not likely to affect the others, though it may prove more serious in the circuit in which the defect occurs. On the whole it seems preferable to localize the trouble, especially in view of possible dangers arising in high-tension systems from what are known rather vaguely as surges. These are likely to arise whenever a sudden change occurs in the circuit conditions: and they may give rise to much higher potentials than those indicated by a simple theory such as that mentioned by Mr. Peck in connection with Fig. 2. The formula given (in which there appears to be a misprint; the denominator in the first formula should be K_1+K, and in the second K_1+K_2+K) only indicates a potential for the insulated conductor which is intermediate between that of the high-tension main and earth. Probably the explanation of such an experience is that, due to a sudden surge, a much higher potential than that of the mains was momentarily impressed on the windings. This may have caused a puncture of the insulation, converted afterwards into a breakdown by the ordinary line potentials. At one time almost the only formula needed for electrical engineering was Ohm's law. This was afterwards developed into the impedance laws of alternate-current circuits, including such cases as that of Fig. 2. But nowadays, with high-tension work, engineers are face to face with what may be called the lightning-flash stage of theory; a theory which as yet no one has been able to make very simple, but in which we have to consider phenomena akin to lightning discharges and to wireless telegraphy. The ordinary formula of steady flow, such as the one given by the author, is not sufficient.

Mr. Taylor.

Mr. A. M. TAYLOR: I think that the author has not proved the necessity for earthing the neutral on a low-voltage system, such as that at Birmingham. For a system that is not at present operating with the neutral earthed, and is not equipped with the arrangement shown in Fig. 1, there are two courses open if it is decided to earth the neutral; either (1) to put in an earthing resistance sufficiently heavy to pass enough current to operate the feeder overload trip gear, or (2) to equip each feeder with the arrangement shown in Fig. 1, and to put in new and more delicate trip gear and an earthing resistance of smaller current capacity. The first course is quite out of the range

of practical politics. Take, for instance, the case with which I am acquainted. The present overload setting of the circuit-breakers is 400 amperes, and this will, before long, be doubled on certain trunk mains. The overload breakers have just been equipped with a time lag of 1½ seconds on the trunk mains. The resistance, therefore, would have to carry 1,120 k.v.a. for, say, 2 seconds, or 2,240 k.v.a. if for 800 amperes. But if five trunks were in parallel, it would have to carry 5,600 k.v.a. (or 11,200 k.v.a.) for, say, 0·2 second, while the reverse-current breakers at the sub-station end were clearing. The resistance would probably take up the space of one generating unit and would be costly. Consider, also, the serious interruption to supply that results from cutting off even one feeder carrying 400 amperes at 5,000 volts. If the neutral is earthed this interruption is deliberately invited for even a trifling fault. Now in most cases the faults do not occur in the cable but on consumers' premises. Hence it is much better to try to devise means to cut off the faulty consumer rather than the whole feeder. Consider, now, the other alternative—to install apparatus as in Fig. 1. This, again, assumes that the whole feeder has to be cut off for the sake of a fault that may be quite unimportant. The more sensitive the apparatus the more this trouble is exaggerated. Mr. Chattock has already spoken of a case where the fault was on the system for over an hour, and was cleared without affecting any one but the consumer concerned. I know of other cases. Moreover, the arrangement shown in Fig. 1 gives no protection against short-circuits between phases. To work with the arrangement of Fig. 1, but having no earth whatever, as alluded to incidentally by the author, seems to me to labour under the disadvantage that it is only possible on very large systems having a capacity current of perhaps 100 amperes and many feeders. Even then there is a tendency—though to a less degree of course—for the relays on *all* the feeders to operate, and not only on that feeder on which the fault has developed; and the reliability may be further affected by the location of capacity round the system independent of the cable capacity. The objection to having a whole feeder cut off for the sake of a trifling fault would still hold, as compared with running with the neutral unearthed. Mr. Taylor.

Dr. T. F. WALL: I think this paper contains a great deal of suggestive information. There is, however, a statement on page 156 which I do not understand, viz. "When both terminals of the high-tension winding are connected to the line and one wire is earthed, the resultant voltage of the high-tension winding above earth will be V/2, and that of the low-tension winding will be one-half as great as before . . . ." It seems to me that if one considers a single-phase system with voltage 2V between the lines, the potential difference between either line and earth will be V if the insulation of each line is equally good; and this I understand to be the sense in which the author intends the voltage V to be taken. Hence if one line were connected to the high-tension winding of a transformer, that winding would assume a potential V, referred to earth potential, if the capacity of the transformer were small Dr. Wall.

Dr. Wall. compared with the capacity of the line. The potential difference between the low-tension winding and earth would then be given by the corrected formula on page 155. If, now, both high-tension lines were connected to the high-tension winding and one line were earthed, the other line would assume a potential of 2V, referred to earth potential. As regards the low-tension winding the capacity currents would tend to send one end to earth and would, therefore, tend to give the other end a potential of twice the value it had in the previous case. The actual potential of the low-tension winding to earth would probably depend on the tranformation ratio of the transformer.

I wish to ask the author whether he has any data as to the magnitude of the equivalent capacities shown in Fig. 2; and whether it is likely that the capacity currents would ever cause an appreciable amount of heating.

Mr. Wilmshurst. Mr. T. P. WILMSHURST : It is rather surprising that the author has given us no information as to the financial aspect of the question. With an earthed neutral on a star-connected system, the voltage between any phase and earth cannot exceed 58 per cent of the line voltage, and in consequence a smaller dielectric thickness may be used. The difference in cost between a cable built to use with an earthed neutral and one built for non-earthing is about 7 per cent ; and this might amount to several thousand pounds on a large system. I wish that the author would give his opinion whether an unearthed system is worth an additional expenditure of, say, 7 to 10 per cent in the cost of the cables.

DISCUSSION BEFORE THE NEWCASTLE LOCAL SECTION ON 9TH DECEMBER, 1912.

Mr. Mountain. Mr. W. C. MOUNTAIN : I think the paper can be divided into sections, *i.e.* first the case of large power stations, and secondly of smaller private installations, such as collieries. I consider, therefore, it would be better to deal with the main subject of the paper, which is in connection with power station work. My experience has been chiefly with private installations, and I am in favour of unearthed neutrals ; but if by earthing the neutral the difficulties can be reduced, by all means do so. From the colliery engineer's point of view, however, I am not convinced that for ordinary purposes it is desirable to earth the neutral. I can only picture to my mind what would happen in a colliery with a big pumping installation, etc., operating on a system with an earthed neutral, and fitted with protective devices, if something went wrong and the supply was cut off. The pandemonium which would ensue would be much worse than that caused by an earth on an unearthed system when the fault could be attended to at the first opportunity. I have always hesitated to put automatic apparatus underground, and I think a leakage indicator is the most desirable thing to install.

Mr. Vernier. Mr. C. VERNIER : I am glad to see that Mr. Peck is on the whole in favour of earthing. There remain, however, two cases in which he

Mr. Vernier.

prefers to leave the neutral unearthed, and both relate to overhead lines. I do not altogether agree with him in regard to those cases; I am a firm believer in earthing the neutral everywhere, and most certainly in the case of high-tension overhead lines under the conditions existing in this country. He has given very good reasons why we should earth on underground cable systems, namely, that cables generally break down to earth first, and it is necessary to be able to cut off a faulty cable before a short-circuit occurs between phases. It is very seldom that a fault arises between phases on an earthed system; but when it does, it is impossible not to know of it, because there is such a serious shock on the system that the switchgear is usually damaged. Obviously it depends upon the size of the system whether we are likely to be able to clear a fault before a second one occurs. I was reading a short time ago of an instance in America where forty feeders had to be switched off one after another in locating an earth on one phase; and this occupied two hours. I do not think anyone would say there was any time lost in this instance; but during the whole of that period there existed the risk of a very serious shut-down occurring, should an earth develop on another phase. On a large supply system like that operating in this district, the probability is that some hundreds of switches might have to be operated for locating and clearing a fault in this manner. Some of the conclusions of the author do not strictly apply to many systems in this country, and elsewhere, where it is common practice to operate mixed systems of overhead and underground cables, and where obviously we cannot very well differentiate between earthing one system and not the other. I think that, generally speaking, leaving the neutral unearthed is a condition more applicable to small systems, and especially to private installations, where the whole of the plant is under the control of the engineer, who can then weigh one risk against another. For large systems I see nothing for it but to earth the neutral; and those undertakings which are running satisfactorily at the present time without an earth connection will eventually find it an advantage to earth when the system reaches a certain size.

On page 158 the author refers to high-voltage transmission; by this I presume he means those long transmission lines in America operating at very high voltages. He mentions having to disconnect the neutral in certain cases in order to make telephone circuits workable. I cannot see that we are any better off by doing this, with an earth on one phase, as the disturbances on telephone circuits are almost entirely due to electrostatic induction; with one phase earthed the whole electrostatic balance of the system is upset. With an earthed neutral, the disturbances on telephone wires are due in some cases to a third harmonic, set up by hysteresis in star-connected transformers, which has the effect of raising and lowering the voltage on the three lines simultaneously to a value of about 40 per cent of the line voltage. The whole line therefore acts as a single-phase line, and causes disturbances in the telephone circuit due to electrostatic induction. The author has

Mr. Vernier. not attempted to suggest leaving the neutral unearthed on distribution circuits. There is no doubt that we in this country attach very great importance to the safety of the public, with the result that we have been remarkably free from serious accidents. This cannot be said of some other countries where practice differs from ours; so I think that there can be no question that on distribution work the neutral point should be earthed. The author refers to the reduced cost of insulation possible where the neutral is not earthed; for moderate pressures up to, say, 50,000 volts, I agree with him and do not think it is worth while. Although we earth the neutral, we do not consider it advisable to reduce the amount of insulation to earth, even on 20,000-volt cables. The fact that cables usually break down to earth makes this extra insulation of special value in withstanding surges and other stresses which tend to break down the insulation to earth. I think this is largely responsible for the very successful operation of mixed systems consisting of overhead lines and underground cables in series in this district. For very high voltage lines, however, say between 80 and 120 kilovolts, it may be worth while to cut down the insulation to earth, especially where corona effects are present to take care of pressure rises. I notice in the report of the discussion at Manchester a suggestion that a high-tension overhead line should not necessarily be cut off in the case of a fault so as to maintain the continuity of supply. It is also stated that this view is accepted by supply engineers. I certainly disagree most strongly with the suggestion. It savours of balancing the cost of a duplicate supply against danger to human life; whatever may be the views held upon this matter in other countries, there is no doubt as to which is considered most important with us. There is already quite sufficient prejudice against overhead work, without introducing any doubt regarding questions of safety. The latest development in protective gear, viz. split-conductor protection, promises to be of very great importance in this respect, as not only can it be made to operate on small earth faults, but it operates in the most positive manner when a conductor breaks.

Professor Thornton. Professor W. M. THORNTON: I think it is well for this question to be reopened from time to time, for though it has been discussed on frequent occasions there has been no finality with regard to it, and only extended experience can give that. With regard to the voltage of the system referred to on page 151, the electrostatic state of the earth's surface is never at zero voltage: it may even be millions of volts in thunderstorms. The author means the line voltage of the system above earth. Reference is made to the fact that the resultant current in a 3-phase system is zero. I do not know why that should be so often mentioned, for there is no electrical circuit where the sum of the currents is not zero. I should like to ask what is the smallest earth current at which the system shown in Fig. 1 would operate. [Mr. P. V. HUNTER: With 1 per cent of the line current; but a delicate relay must be used.]

It is undoubtedly interesting to note that in the author's opinion

Professor Thornton.

the difference of risk of breakdown with the generator neutral earthed and with it unearthed can be practically neglected. The opinion is still held amongst mining men that earthing the neutral is putting a premium on breakdowns. With regard to surges, I would suggest that the real oscillations which break down the insulation of a system originate in the flame of the arc between phases, by reason of its capacity. Incandescent metallic vapour is an excellent example of a conducting dielectric; and with such a medium in a condenser the capacity is relatively very large. We have then a condenser with appreciable capacity in series with the line; and such a system can resonate with great violence. If arcs are permitted to develop, the risk of breakdowns on other parts of the system is very greatly increased. There is one method of earthing the neutral which does not seem to have been referred to in the paper, *i.e.* through choking coils. On page 152 the author states that practically the only question to consider is whether the neutral should be earthed solidly through a resistance. During the discussion on Mr. Wood's paper read before this Section a suggestion was made to earth through a choking coil. Certainly from the point of view of theory a choking coil is the very thing one wants, but it is probably limited in use by considerations of cost.

On page 153 heavy circulating currents are referred to. I have made oscillograph tests in works and power stations, and I have never found two machines having exactly the same wave-form on load; any machine may have a very different current wave-form when running in parallel at low excitation from what it has on full load. The capacity effect referred to in Fig. 2 seems to be coming more into general discussion. It may not be clear to everybody why the potential of the low-potential winding above earth should be equal to V K. That this must be so, however, will be clear on considering the fact that the same quantity of electricity must pass through both high and low-voltage insulation in series, as capacity current; and since $Q = KV$, the product of the voltage on the insulation and the capacity to the core must be the same in both cases. The summary on page 157 of objections to earth-shields is very interesting from Mr. Peck's former experience on this point.

Mr. Baxter has raised the question of "Potential Front." May I be permitted to explain what appears to me to be the physical fact at the back of this somewhat obscure phrase? At the moment of switching on pressure to one end of a wire we have the conditions of full pressure at one end, no pressure at the other. The pressure from the wire to the surrounding insulation is then equalized along the wire at the velocity of "wireless" waves in space. If there is no load at the far end the only factors are the electrostatic capacity and the impedance of the line; and at any point in the line where either of these changes in value there is a reflected electric wave, that is, a wave of electric stress in the medium around the wire. When there is resistance in circuit at the far end the probability of resonance is diminished; the oscillations are less "free" and more "forced"

Professor Thornton.

in type; and the influence of motor load once established can be expressed in terms of its equivalent resistance. But the state of things in front of the electric wave as it advances along the wire is not the same as when regular rhythmic flow is established. The current does not reach its full value instantaneously; there is first a thin trickle —then the full tide rapidly rising. With large wires and no inductance the retardation of the wave front is small and unimportant; but where the line wire enters the windings of a motor which have very great inductance, only an exceedingly small displacement wave current is forced through first under the line pressure, followed in a small but infinitesimal fraction of a second by the full state of flow. But Heaviside has shown that to force under these conditions an infinitely thin filament of current through a system the electrical gradient in the insulation rises to an infinite value; so that what we really mean by a potential front is the increase of the electrical stress in the insulation as the wave front advances along the line, caused by this wave front encountering inductance. Suppose that a round shaft carried a fly-wheel, and that a twist was applied with infinite rapidity to the shaft circumference; it is easy to see that the stress on the outer layers would be very much greater at the moment of application, before the stress was equalized over the section, than when a steady stage was reached; and the greater the inertia of the fly-wheel the greater the stress at the shaft circumference. Also, even if the applied twist alternated periodically, the circumferential stress in the shaft would never be so great as at the moment of instantaneous application, unless this coincided exactly with zero.

Mr. Baxter.

Mr. W. BAXTER: Mr. Peck, in his conclusions on page 160, seems to think that there is no reason for earthing the neutral on high-tension transmission lines. Members will remember the recent case in Scotland where two people were killed: I believe that the Company is now putting in protective gear, and earthing the neutral. I wonder if the condenser effect referred to on page 155 is connected in any way with what has been called "potential front."

Mr. Hunter.

Mr. P. V. HUNTER: What experience I have had with unearthed neutrals was gained in the early days of the Durham Collieries system, where the tendency was for trouble to occur at more than one place at the same time, *i.e.* if a breakdown occurred at one point, similar breakdowns occurred at two or three other points simultaneously. I put that down to oscillations caused by free energy, liberated in the circuits when one phase went to earth, which has to dissipate itself by moving about at a very high speed. It eventually finds one or more weak spots. I do not see any real advantage in being able to run unearthed, and it tends rather to lengthen the time occupied by repair work. An earth on one phase takes time to locate if the system is at all extensive; should another earth occur meanwhile on another phase, two faults exist on the system at the same time, probably at widely different points, and may cause a large shutdown. On the other hand, with an earthed neutral the faulty feeder is located

Mr. Hunter.

immediately by the protective gear. In the old days, without protective gear, it took as long to find the faulty feeder as it did to repair the fault. The conditions are similar with an earth on one phase with an unearthed neutral. I have known several cases where high voltages have occurred on quite small installations with unearthed neutral, which I have attributed to free energy liberated by an earth on one phase. In the case of a colliery having a 6,000-volt supply which was transformed to 600 volts and which worked with an unearthed neutral, paper spark-gaps were inserted between the transformer neutrals and earth in order to comply with the Board of Trade regulations. These gaps can be set to break down very consistently at any desired voltage by varying the paper. I found that although no fault had occurred in the transformer the paper had been pierced, showing that a pressure of more than 500 volts had existed; whereas theoretically not more than 350 volts could have existed. Greater thicknesses of paper were inserted, and it was found that with paper discs which required a pressure of 1,200 volts to pierce, sparking-over still took place. A 2,000-volt disc was not pierced. I am not quite clear whether this voltage is accounted for by Mr. Peck's explanation; namely, that capacity effects, as shown in Fig. 2 of the paper or due to a variable resistance earth on one of the phases down the pit, cause oscillations of free energy. With the ordinary 3-pole oil switch this effect mentioned by Mr. Peck would only last for a very small fraction of a second; I am inclined therefore to think that the effect Mr. Peck mentions does not ordinarily occur to any serious extent in ordinary switching operations on high-tension systems. I think, however, they may possibly occur as a serious additional complication when faults are automatically isolated on a secondary system having an unearthed neutral. Mr. Peck really only supports the unearthed neutral in one case, that of a very high-voltage, heavy-power, overhead transmission line; and he supports it not because he thinks it best technically, but for continuity of service. I think we may say these conditions do not apply to this country. A heavy-power high-voltage transmission line in this country would not be run without an alternative supply in case of breakdown; in fact, the Board of Trade rightly limit non-duplicate supplies to 1,000 kw. Mr. Peck's reasoning does not apply therefore to this country; but in America consumers are prepared to sacrifice reliability for cheapness, and will put up with the occasional shutdown that must necessarily occur with a supply given by a single line. The danger to the public which exists when a live wire lies on the ground is also not considered such a serious matter in America; and in fact since many of the lines pass through thinly populated country, over private rights of way, the danger does not necessarily exist to the same extent as in this country.

The suggestion of earthing the neutral through a choke coil has been raised; the real objection is the cost, on account of the fact that the choke coil must not be run at saturation point. With the iron circuit

Mr. Hunter. saturated, the reactance voltage of the choke would have a very peaked wave-form, a very undesirable effect to introduce into a fault to earth. To avoid this it would be necessary to use a very expensive choke coil. A resistance to withstand a heavy rush of current for a short time can be built cheaply, and is not liable to break down. I quite agree with Dr. Thornton in regard to circulating currents. The arrangement of neutral busbar and one resistance, which Mr. Peck suggests, was not adopted in the large power stations in this district because of circulating currents in the neutral. In several cases Mr. Peck suggests the use of spark-gaps; but these should be used very cautiously, as an ordinary horn-gap without a series resistance will readily produce high-voltage oscillations. The spark-gap in general use here is one in which two plates are very close together, in fact with only a very thin piece of insulating material between them. When the insulating material is punctured by the pressure the two plates weld, whereas an ordinary horn-gap does not weld. Mr. Baxter raised the question of potential front, and I must say that I have never seen any really convincing explanation. I know of one case in which sparking has occurred across terminals on the same phase of a 20,000-volt boosting transformer, the sparking distance being of the order of 8 to 10 in. Each winding of the booster was in series with one core of a 20,000-volt underground feeder, and ordinarily the pressure between two terminals on the same phase would not exceed 2,000 volts. Such sparking may be explained by potential front when switching the cable and booster together.

Mr. Hunt. Mr. F. O. Hunt: In the discussion of Mr. W. W. Wood's paper some time ago I referred to a protective device. It was an apparatus brought out by Mr. M. B. Field, which worked on the same principle as that shown in the diagram on page 151, in that it depended on the sum of the currents being zero. It makes use of the fact that if a 3-core cable be surrounded by a magnetic path, the flux in that path will be zero only so long as the sum of the currents is zero. It can therefore be applied directly to an existing cable. It is practically the same idea, only it balances the magnetic fields instead of the currents. As regards the question of using choke coils for earth connection, I should like to raise a warning. The whole capacity of the network would be really in series with the choke coil. Might not this result in the formation of a resonant circuit with very low resistance?

Discussion at Middlesbrough on 17th December, 1912.

Mr. Christianson. Mr. W. A. Christianson: A class of service not considered by the author, but of great interest to us in this district, is that of generators supplying an interconnected network from points widely separate, transformers being sometimes utilized. The Board of Trade Rules at present limit earthing to one point; and spark-gaps are generally provided at other points in order to bring the neutral to earth potential in the event of a possible removal of the earth connection. A case might be conceived in which a fault to earth might separate the system into

two or more parts due to automatic opening of section switches. Serious trouble might then be expected upon paralleling the sections together, with the fault still on one section and the earthed neutral on the other. In a case where it is found necessary to earth only one generator at a time owing to trouble with neutral circulating currents, and in event of a fault to earth on the system, there would be a considerable drop in voltage on the faulty phase which would not occur if several generators were earthed. The former arrangement would apparently not clear a faulty feeder with the same certainty as the latter arrangement; but no trouble is generally experienced in this respect. The author has not mentioned the use of choking coils in connection with the method of earthing generator neutrals. An interesting arrangement was referred to some time ago involving the use of a common neutral busbar and a common non-inductive resistance to earth, but with a choking coil between each generator neutral and the neutral busbar. The choking coil offered three times the impedance to triple-frequency circulating currents that it did to normal-frequency earth-fault currents. At first sight the arrangement would seem a good one; but I am not aware of its use, or indeed of a case where choking coils are used alone in any important system. Choking-coil arrangements have generally not found much favour. In connection with the voltage induced on the low-tension side of a transformer due to electrostatic capacity, how is this likely to be affected with different types of windings? Mr. Christianson.

Mr. R. M. LONGMAN: I think that an explanation should be given of what is meant by "earthed." The general practice is to earth the lead covering of all cables; and from tests I have made, the lead of a cable seems to be a better earth than any earth-plate. I have made tests under various conditions, and however the cables are laid—whether in troughing on the draw-in system, or laid solid—the lead covering is a better earth than any that can be made with plates. In a case that I knew of some years ago about 100 yards of two 3-core lead-covered cables had just been laid in dry weather in wood troughing filled in with bitumen or pitch, and the resistance of the lead covering to earth was only about 0·1 ohm, which is remarkably low, much below that of any two earth-plates. On a system of high-tension lead-covered trunk mains drawn into earthenware piping and earthed in each manhole, these being spaced about 100 yards apart, the resistance between such plates and earth was tested after disconnecting them entirely from the cables. The lowest value I think was 10 ohms, and the average about 20 ohms from plate to plate. Some of these were in very wet manholes—in fact, water was lying at the bottom. The plates were laid under the concrete bottom of the manhole, and when some were taken up they were found to be resting on only four points, with a hollow on the underside; hence the bad contact. That means that the lead is "live" when a fault occurs; there is thus a danger that if the neutral is earthed there would be burning of the lead at some points. On small cables that has occurred; in fact, it is liable to strip Mr. Longman.

Mr. Longman.

the lead at points. In spite of connecting the lead covering of a cable to a sub-station earth bar the fault current will not return to the power station altogether by way of the lead covering. Tests made on a seven-mile length of cable proved this conclusively.

One of the best ways of earthing the lead covering of a cable is to get some old iron rails and lay them in the ground alongside the troughing, bonding them to the lead covering; or else to drive some pipes or rods into the ground instead of the cast-iron earth-plates generally used. A series of tests was made of various types of earth-plates laid in different ways, but no conclusive results were reached. In the case of an overhead line each pole must be earthed. The earth-wires of some poles were taken down, divided into six, and spread out in star fashion in the ground. The resistance from one pole to another, averaged for about 20 poles, was 20 ohms. Nearly all the drop of pressure occurs within a few inches of the earth-wire or plate; it is therefore advisable to spread the latter out, and even to carry the insulated lead well under the surface so as to avoid any great potential gradient on the surface. The practice of connecting generator neutrals solidly together, whether earthed or not, is very debatable. I do not think it is at all the correct thing. Circulating currents do result. From some tests made on machines of similar design the circulating current in the neutral was found to depend largely on the power-factor of each machine; in fact, it was decided to put a power-factor indicator on each machine so as to operate them at as nearly the same power-factor as possible. Even if the heating effect is not considerable I believe that there is a very considerable demagnetizing effect, which is most important. The better plan seems to be only to earth one machine at a time.

Mr. Brazil has referred to a carbon powder resistance. That should be satisfactory; but I think there is more danger of fire with it than with iron grids, owing to the presence of air or gases which carbon always occludes.

Wherever a high-tension system is stepped down to a low pressure I think the neutral of the low-tension system should always be earthed, both on account of the static effects shown on page 155, and also on account of safety, as in case of a breakdown of the high-tension to the low-tension windings there is a great danger to the insulation of the low-tension side. The same remark would also apply to 440- or 500-volt distribution. For instance, when transforming from a high to a medium pressure, say from 11,000 to 3,000 volts, if the low-tension side is connected in star, and if there are several transformers with similar characteristics, the neutrals could be connected together, but should have a spark-gap to earth. On all subsidiary circuits where there is a step down in pressure I think there should either be a reliable spark-gap set to go direct to earth when there is a breakdown from the high-tension to low-tension windings, or an earthed point on the low-tension side. Personally I think it is better to have the earth. On a circuit fed only by transformers there

should be little chance of circulating currents with several neutrals earthed; but with a generator in the circuit with the neutral earthed no other neutral should be. There is undoubtedly great variation of opinion on this question; but there is one point which I think makes a lot of difference, viz. the size of the system that is being operated. It would be quite right to run a small system unearthed, but I am inclined to think that on the whole the larger systems ought to have an earthed point, as there are generally two ways of supplying any particular point or consumer. The method of current balancing shown on page 152 is hardly practicable. The settings would be very fine and the relays would be tripping on the capacity current. Mr. Longman.

Mr. L. M. JOCKEL: With regard to the earthing of generators, in practice we cannot always put down the latest types of turbo-alternators with non-salient rotors and specially designed compensated windings, so as to obtain a theoretically perfect sine wave under almost all conditions of load. There are also many cases where a turbo-alternator must be installed to work in conjunction with existing reciprocating sets, or perhaps an earlier type of turbo-alternator; in which case very serious considerations may arise with an earthed neutral. The higher harmonics are generally present in the E.M.F. wave, and I believe the third harmonic is usually the most important. The triple-frequency circulating currents are of course affected by the excitation, and it is seldom possible and sometimes even inconvenient in practice to get equal power-factors on all the generators running. I think it would, perhaps, be better in such cases to have an ammeter in circuit with the neutral connection of each generator so that the excitation could be adjusted to give the minimum value of circulating current. The author mentions only the heating effect of these circulating currents; but another effect which is perhaps of importance, and which has already been mentioned by Mr. Longman, is the demagnetizing effect of the higher harmonic circulating currents. It is, of course, well known that the sum of the currents in a star-wound generator at any instant is equal to zero, but it does not follow that the sum of the higher harmonic circulating currents at any instant is equal to zero. They may flow towards the neutral in the opposite direction to the load currents in the phase windings, and consequently have a demagnetizing effect on the stator windings, which, of course, means extra energy required for excitation, and a lowering of the power-factor of the machine. My first experience of 3-phase work was in connection with a transmission system with an unearthed neutral. We relied to a certain extent on three electrostatic voltmeters which were mounted on the control board. These were connected between each phase and earth. However, I am inclined to think they were really more ornamental than useful. When faults came on we had not time to run and look at the voltmeters before the fault was cleared. I should like to ask the author if he thinks that the fact of the neutral being insulated would have any bearing on the many troubles we had on the transmission cables. From subsequent experience with an earthed neutral it would Mr. Jockel.

Mr. Jockel. appear that it is perhaps advantageous in the case of underground cables.

Mr. Thompson. Mr. P. S. THOMPSON: On the north-east coast the case is quite proved for earthing the neutrals of generators supplying direct to a system without the interposition of transformers. I think that a neutral bar earthed through a single resistance has much to commend it, as settings of overload relays can then be arranged with some certainty. There is, of course, the disadvantage of circulating currents, on which perhaps undue emphasis has been laid, as these can be largely obviated by the provision of choking coils between the neutral points and the bar. A usual arrangement is for each generator to have its own neutral resistance to earth. When a number of generators are running under those conditions, it is quite probable that a fault to earth will be cleared quite quickly; in fact, soon enough to avoid development into a short-circuit, as feared by the author. With only one generator on the bars, however, it is probable that a fault to earth will last sufficiently long to set up very serious disturbances all over the system. Mr. Christianson mentioned the case of a network supplied from a number of sources, viz., direct, through step-up transformers, and also through step-down transformers. It is quite conceivable that, with the neutral earthed at only one point, section switches may in case of a severe fault operate in such a way as to separate off this particular point from the rest of the network, leaving the remaining portion of the system with an earth on one phase. Very considerable trouble would result when the section with neutral earthed was again paralleled with the remainder of the network. I should like the author to say how he would deal with such a case—whether by spark-gaps, as I think he mentions, or in some other way. It is, of course, obvious that a complicated system such as that mentioned could not be directly earthed at more than one point. With regard to high-voltage transmission schemes, while we recognize that there is a certain amount of prejudice against the use of pole lines, we also recognize that from a commercial point of view they are a necessity. Further, one cannot get away from the fact that if they were so arranged that an earth on one phase would cut out the line there could be no reasonable objection to their greater use. I therefore think that it would be an advantage from a development point of view if it were the practice so to construct pole lines that they would automatically cut themselves out as soon as anything went wrong.

Mr. Marshall. Mr. A. H. W. MARSHALL: The question of what constitutes a good earth has a considerable bearing upon the subject which we are discussing. My impression is that the old Board of Trade Regulation for tramways, which calls for a resistance, between two plates buried 60 ft. apart, low enough to permit of the passage of 2 amperes at 4 volts, is rarely found possible of application. This shows the difficulty which exists. Mr. Peck seems to think that an earth on a cable usually develops into a short-circuit; but my experience is that with 3-core, extra-high-pressure paper cables a fault to earth does not spread. I

do not want that statement, however, to reduce the value of his argument in favour of earthing the neutral of cable networks. In regard to the general question, the advantage of being able to operate with an earth on one phase is a very doubtful one, and with the exception perhaps of small, isolated, overhead systems is not sufficient to compensate for the disadvantages which exist. It is well known that with a free neutral the fluctuation of the voltages when any disturbance occurs, with the consequent static unbalancing, is much greater than in the case of the earthed neutral. This is specially true with an arcing fault, such as may be set up by a broken insulator: an earth on one phase is only too likely to break down the insulation on the other phases somewhere else, and, from what I have seen, invariably does so. As time goes on, and networks become larger and more interconnected, the need for discriminating protective gear will become greater, and this will lead to the development and use of instantaneous tripping gear, requiring the neutral to be earthed. I am inclined to think that the ultimate solution of the problem will be to run with an earthed neutral in every case. Mr. Marshall.

Mr. J. S. PECK (*in reply*): Mr. Brazil has referred to electrolytic lightning arresters. I did not intend in my paper to give the idea that the electrolytic arrester was apt to set up dangerous surges in the system; in fact the particular trouble to which I referred might have been caused by a simple resistance or a simple capacity as easily as by an electrolytic arrester. However, it has been found that there is a possibility of minor surges being produced on a system when the arresters are being charged, for the purpose of renewing the film; but as far as can be found out, these surges have never been responsible for any trouble. Nevertheless, in order to ensure that no damage shall result, it is now customary to connect a resistance in series with the arresters at the time of charging. Mr. Peck.

I know nothing about the merits or demerits of Mr. Brazil's graphite resistance, but resistances of a graphitic type have not always given satisfactory operation, so that personally I should prefer the cast grid type, the characteristics of which are well known and can be replied upon.

As to the question of temperature coefficient, I believe the matter can be argued either way, but I do not think it is one of great importance in any case. I consider that Mr. Brazil's figure for the increase in resistance of cast-iron grids is rather high, as the temperature coefficient of cast iron is practically negligible.

Mr. Chamen has asked whether the earthing of one generator out of a number will not limit the current. With one generator earthed, the current will certainly be less than where several generators of the same size are connected in parallel and earthed, provided the limiting factor in both cases is not the resistance in the earth circuit; but even with one generator an extremely heavy current may flow, which in some cases might damage its windings.

Mr. Chamen has also referred to the possibility of permanently

Mr. Peck. earthing one wire of a 3-phase system when the neutral is inaccessible. On a low-voltage system I see no objection to this; but the higher the voltage the greater is the risk. The objections are:—

(*a*) Two of the wires are raised to a potential 73 per cent higher than if the neutral were earthed.
(*b*) The charging current on two of the wires is increased 73 per cent.
(*c*) There may be a slight unbalancing in voltage due to the difference in the charging current of the three wires.

The above points must be carefully considered before deciding to earth one wire; and it should be remembered that there is a decided difference between running continuously with one wire earthed and running under the same conditions for a short period in case of emergency.

Where the transformers are connected in delta it is often possible to get at the middle point of the windings. If the middle point of the winding of one of the delta-connected transformers is earthed, then one of the phases is maintained permanently at a potential above earth equal to 87 per cent of the voltage between wires, while the other two phases are maintained at a potential above earth equal to 52 per cent of that between wires.

Mr. Partridge also raised the question of running with one wire of a 3-phase system permanently earthed; and he cited as an advantage of this method the saving in switchgear. It might be pointed out, however, that the voltage strains to earth on a 6,000-volt plant with one wire earthed are the same as those on a 11,000-volt plant with the neutral earthed; whereas at the higher voltage only one-third of the amount of copper is required as at the lower voltage.

It was further asked what would happen in a high-voltage transmission line with unearthed neutral should the high-tension winding of the transformer break down to the low-tension winding. When I referred to a transmission system with an unearthed neutral, I meant that the neutral on the high-tension side of the transformers was unearthed; but I strongly recommended that in all cases the low-tension side of the transformers should be unearthed. In this case the low-tension winding could not be raised above its normal voltage, even should a breach occur between the high- and low-tension windings.

The suggestion made by Mr. Partridge of using a spark-gap in shunt to the resistance appears to be a good one, for there is always some danger of overheating or burning out the earth resistance in the event of circuit-breakers failing to operate. The further suggestion made of connecting a spark-gap between earth plates and return conductor may be useful in certain cases, especially where a satisfactory earth cannot be obtained.

Mr. Nelson strongly advocates earthing the neutral in colliery plants in order that the supply may be cut off quickly in the event of damage to the cable. I quite agree with him as to the desirability of earthing

on underground systems, but I very much question whether it is desirable to earth the neutral point of every low-voltage system. Take, for example, the case of a large engineering works supplied at 220 volts, the cables running in all directions about the plant. Earths, more or less complete, occur at frequent intervals and are cleared as soon as there is an opportunity, but in the meantime the system must be run with one end of the wires earthed, as it might cause very serious loss and inconvenience to shut down that section of the works in which the defective wire was located. Mr. Peck.

Mr. Trotter agrees with me as to the advisability of earthing the neutral on an underground cable system, but he can see no advantage in running with a free neutral on an overhead system. There may be some justification in his arguments with reference to this country, where voltages are low and distances short; but the *Journal* is read in other countries where conditions are different, and I think it very desirable not to confine our papers or discussions to British conditions alone. On many transmission systems there are circuits several hundred miles long which cannot be inspected every day, but over which continuity of service is essential. The possibility of operating under emergency conditions with an earth on one wire due to a punctured insulator, or other similar trouble, may be of the greatest importance. Of course such a condition of operation would not be permitted to continue longer than necessary. In order to prevent a heavy current passing to earth at the defective point it would only be necessary to earth the defective wire at the generating station; the system would then be equivalent to that at Deptford, which appears to meet Mr. Trotter's approval; and I cannot see that under these conditions there would be any risk whatever to the public.

Mr. Edgcumbe referred to static indicators for showing the condition of the insulation of the circuit. I quite agree with him that the modern static voltmeter or static earth protector is quite a satisfactory instrument for low-voltage circuits, *i.e.* circuits not exceeding 10,000 to 15,000 volts; but on very high voltage circuits it has not proved so satisfactory. An ammeter connected between apparatus and earth is useful, but it does not indicate on what feeder or on what wire the earth occurs. In the case of transformers which supply a single feeder, the ammeter in the connection between the transformer neutral and earth would of course be discriminating as far as the particular group of transformers was concerned, but it would not indicate on which one of the three wires the fault had occurred.

Dr. Garrard wishes to know what kind of a switch should be placed in the earth connection. I would recommend for this an ordinary high-voltage isolating switch mounted, as he suggests, in the high-tension cubicles, though in some cases a remote-controlled oil switch may be preferred. He also wishes to know how close the discriminating devices shown in Fig. 1 of my paper can be set. There is no difficulty in setting these to operate with an unbalanced current of 5 per cent of the normal full-load current; and by using special transformers and

Mr. Peck. relays it is possible to set them considerably closer than this. Overload protective devices in addition to the discriminating devices should be used. When I stated on page 155 that if both terminals of the transformer were connected to the high-tension line the resultant potential of this line above earth would be zero, I meant that this would be the case on a symmetrical system, as I had explained elsewhere that if the impedance of each wire to earth was not equal the system would not be symmetrical.

Mr. Paton is in favour of earthing overhead circuits as well as underground cables. His argument is based on the difficulty of locating a defective insulator where it has broken down on an unearthed system. In certain cases this is true, while in others the insulator is badly shattered and can be picked out without trouble. Where there are duplicate lines supplying all customers, there is much more to be said in favour of earthing than where single lines are used. In the latter case it may be extremely desirable to operate with a punctured insulator till the week-end. One of the other wires can then be earthed, when the defective insulator will probably be shattered so that it can be easily picked out. The whole question seems to rest on the fact that without an earthed neutral two breakdowns are required to put the system out of commission, while with the earthed neutral a single breakdown will do so.

I am interested to hear that Mr. Wedmore was the first to suggest the arrangement shown in Fig. 1 of the paper. He issues a warning against the use of choke coils in earth circuits and the use of testing transformers of such proportions that they can produce resonance in the circuit; also against the use of too high a resistance in the earth circuit. It is evident that the higher the resistance the more nearly does the system approach to the condition of an unearthed system.

Mr. Faye-Hansen asks whether any telephone troubles have occurred due to earthing the neutral point. I know of one case where very serious telephone troubles occurred when the neutral was earthed, but which disappeared entirely when the neutral was disconnected from earth: but I do not know whether the neutral was earthed at both the generating station and the sub-station.

I do not agree with Mr. Shaw as to the necessity for a circuit-breaker in the neutral connection, because it will always be possible to open the field circuit of a generator in case of trouble, and this would be a far more effective means of stopping the trouble than the opening of the earth circuit.

Regarding Professor Marchant's query as to the effect of triple-frequency currents on the device shown in Fig. 1 of the paper, there will be no trouble of this kind unless there are at least two separate stations with their neutrals earthed and with feeders supplying common sub-station busbars as shown in Fig. C, or unless the arrangement shown in Fig. 1 is placed between the generators and the busbars.

I am glad to see that Mr. Watson supports my argument regarding continuity of supply with an unearthed neutral, but I cannot agree

Mr. Peck.

with him in his statement that the unearthed neutral is more dangerous to human life; for, with the neutral earthed, a man standing on earth cannot touch any live wire without getting a dynamic current through his body that is much more likely to prove fatal than is the charging current which he might receive when touching one wire of an unearthed system. Undoubtedly, present practice favours the building of alternators with poorer regulation than formerly and providing them with automatic regulators; but even these machines give enormous rushes of current at the instant of short-circuit. I am afraid Mr. Watson has confused continuous short-circuit current with instantaneous short-

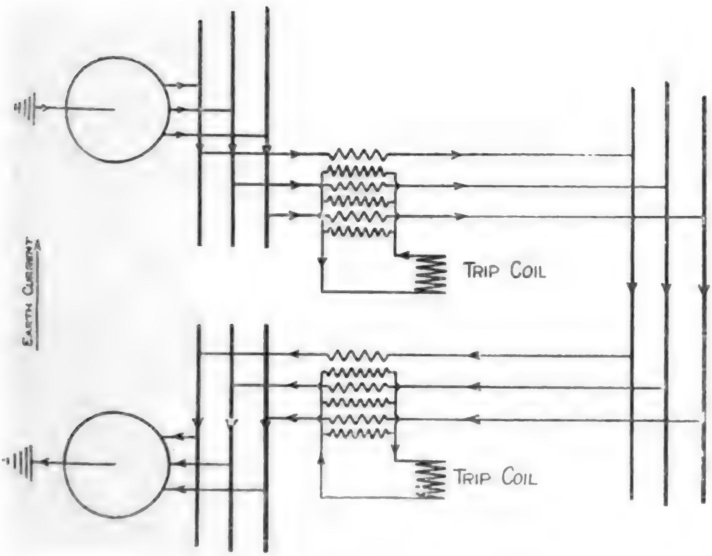

FIG. C.—Diagram showing Direction of triple-frequency Circulating Currents between two Generating Stations.

circuit current. This matter has been dealt with by Mr. Miles Walker in the *Journal*.*

Messrs. Pearce and McKenzie have given some very useful information regarding the system at Manchester, and I believe the decision to earth the neutral point through a resistance is one that the engineers of all very large systems will eventually come to as their systems grow and as the amount of cable, and therefore the charging current, on the systems increase. Also as cables grow older there is a greater tendency for breakdowns to occur.

Mr. Pearce asks whether faults do actually start to earth rather than between phases. While I have had no actual experience myself,

* *Journal of the Institution of Electrical Engineers*, vol. 45, p. 295, 1910.

Mr. Peck. I have always understood that in the great majority of cases the trouble started with a fault to earth.

Mr. Chattock apparently has been able to operate a large underground cable system with a free neutral with very satisfactory results. Also there appears no reason why he should not continue to do so as long as he has no breakdowns on his cables. But as his system grows larger and the capacity current increases, he will no longer be able to operate with an earth on one phase without its developing into a short-circuit between phases. Also as his cables grow older, troubles will be more likely to occur; and eventually I think he will find it of great advantage to earth the neutral and to provide discriminating devices on at least the heaviest of his feeders.

Mr. Taylor bases his arguments against earthing upon the undesirability of cutting off a very heavy feeder upon the occurrence of a trivial fault. By trivial fault he must mean one which passes sufficient current to earth to trip the breakers; but is it possible to continue running with such a fault upon a heavy current cable? I believe that in very few cases it is possible to do so without the trivial fault developing into a very serious one; and if this is true, then the sooner the faulty cable can be isolated the better for the system.

When the arrangement shown in Fig. 1 of the paper is used there seems no very good reason for not earthing the neutral. The device would certainly not be as reliable with a free neutral as when it is earthed; but I believe it has given satisfaction in at least one large plant. A charging current as large as 100 amperes would not be required to permit the device to operate, as a relay can be set for a very small unbalancing in current. It will work best when there are a large number of feeders; but it should work in the case of two feeders, provided they are not connected in parallel at both ends.

In reply to Dr. Wall's question, I think there is no danger from heating due to condenser currents between primary and secondary windings, even in very high-voltage transformers, provided the insulation is properly ventilated.

Mr. Wilmshurst has asked whether it is advisable to pay 7 to 10 per cent more for cables for the sake of having them insulated for full voltage instead of for star voltage to earth. It is the author's opinion that money expended in this way is likely to prove a sound investment.

Mr. Hunter cites the case of a 6000/600-volt transformer in which a spark-gap indicated pressures of 1,200 volts to earth. I do not think it is necessary to attribute this to surges, as it is more easily explained by the condenser action between the high-tension and low-tension windings. When reference was made in the paper to spark-gaps between winding and earth, gaps similar to those described by Mr. Hunter were referred to, *i.e.* those which are welded together by the passage of current.

Mr. Christianson has asked how the electrostatic capacity between the high-tension and low-tension windings and between the low-tension winding and the iron varies with the different types of transformers.

Mr. Peck.

The difference will usually not be very great between core-type and shell-type transformers with the common arrangement of coils; but in general the higher the voltage of the transformer the greater the capacity from the low-tension winding to iron as compared with that from the high-tension to the low-tension winding. Therefore there will be a greater voltage drop between high-tension and low-tension windings than between the low-tension winding and the iron.

Mr. Hornby, Mr. Christianson, and Mr. Thompson have asked what should be done in the case of two generating stations which are located

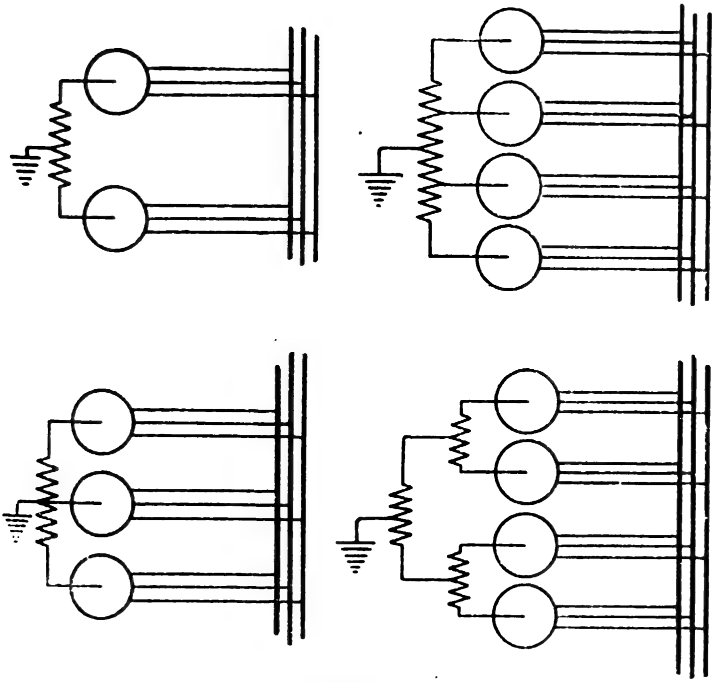

FIG. D.—Methods of connecting Auto-transformers to prevent Circulating Currents between Generator Neutrals.

some distance apart, and where the Board of Trade Rules prohibit the earthing of the neutral point at both stations. I am informed that the only reason the Board of Trade object to earthing more than one point of any system is for fear of damage due to earth currents between the two earthed points; and that if it can be shown that this current causes no disturbance on telegraph or telephone circuits, there will be no objection to earthing the neutral point at each station. I do not think that any trouble is to be feared from earth currents between the stations, provided fairly high resistances are placed between neutral point and earth. There is always the possibility, however, of connect-

Mr. Peck. ing the neutral point of one station to that of the other by means of insulated cable, in which case any circulating current would flow through the cable and not through the earth.

If discriminating devices of the kind shown in Fig. 1 of the paper are used on a system having two independent generating stations, each with earthed neutral, it is necessary to limit to a low value the circulating currents, otherwise these currents may cause the discriminating relays to operate. Fig. C shows such a system, where the arrows represent the direction of the flow of the triple-frequency circulating current at some particular instant. It is evident that the resultant of the three primary currents is not zero, but that they act as a single-phase current, and that if of any great magnitude the relays may be operated.

Several speakers have asked about the advisability of earthing through choke coils instead of through resistances. The principal objection to the use of a choke coil is its high cost. I myself do not attach great importance to the fear of resonance in the case of any commercial choke coil, though it is, in general, wise to avoid placing reactance in series with capacity. Where it is desired to connect several generators to earth, there are certain combinations of auto-transformers which may be used to prevent circulating currents, but which will not limit the flow of current in the earthed circuit. Fig. D shows some of the possible combinations. It is evident that the auto-transformer will act as a powerful choke coil against circulating currents from machine to machine, but will offer a practically non-inductive path for currents to flow to earth. When machines are cut out of circuit it may be necessary to change the position of the earth connection on the auto-transformer or to cut out the transformer altogether.

The discussion appears to have brought out the following points :—

1. There is a decided difference of opinion regarding the advisability of earthing the neutral point, even in the case of cable systems.
2. Small cable systems seem to operate satisfactorily with a free neutral, and it is possible on such systems to operate for some considerable time with an earth on one wire of a 3-core cable.
3. On large and extensive cable systems it is advisable to earth the neutral through a resistance and to provide discriminating devices on at least the heaviest of the feeders.
4. When two or more generating stations are worked in parallel, some means must be found for earthing at all stations, which will limit the amount of the circulating earth currents to a value which is unobjectionable.
5. The low-tension side of all high-voltage transformers should be earthed.

THE TURBO-CONVERTER; A HIGH-SPEED DIRECT-CURRENT GENERATING UNIT.

By F. CREEDY, Associate Member.

(*Paper first received 12th March, and in final form 7th September,* 1912; *read before the* MANCHESTER LOCAL SECTION *5th November, and before the* SCOTTISH LOCAL SECTION *10th December,* 1912.)

INTRODUCTION.

The present paper describes a device which differs in some respects from standard mechanical practice in electrical matters, and it is written to suggest that a deviation from this practice may sometimes be warranted by results.

Electrical engineers are naturally conservative in such matters, but it is the writer's belief that a great deal is lost in some cases by too rigid adhesion to the standard practice. In some cases very great electrical complications are introduced in order to avoid a slight departure from traditional mechanical practice. This is well illustrated by the history of the electric locomotive where very great electrical sacrifices, and also sacrifices of convenience and accessibility, were made in order to produce a locomotive free from gears and coupling-rods. When it was finally produced, it was found to be undesirable except for light work, as the well-known New York Central accident showed.

A still better example, of course, is the present type of direct-current turbo-generator, and the present paper describes an attempt to supersede such a mechanical monstrosity as the high-speed commutator of the ordinary direct-current turbo-generator by a construction which, though differing from current practice, is far less fundamentally objectionable.

It is hoped that the present paper may stimulate discussion as to forms differing from standard practice and their uses, as if the principle is once established that the standard two-bearing mechanical design is not the only one permissible, and that all others will not be summarily ruled out, great possibilities are opened out for the use of electrical gears of all kinds, besides the one described, which is an attempt to solve a very difficult problem by means of an electrical gear.

The fundamental difficulty, of course, in the design of a direct-current turbo-generator, is the collection of the current from the rapidly revolving commutator; and for this in spite of the engineering skill

lavished on the subject there would appear to be no remedy, except reducing the rate of revolution of the commutator. In order to do this, Mr. H. M. Hobart has proposed the use of an alternating-current generator driving a rotary converter of the ordinary type; but this proposal has not often been adopted in this country, presumably through fear of the cost. Another plan that has had a certain measure of success is the use of double helical machine-cut gears, which are now obtainable in sizes suitable for transmitting large powers.

The present paper describes a new method devised by the writer, which may be regarded, to some extent, as a combination of the two mentioned above in that an alternating-current generator of the induction type feeding a rotary converter is employed, the induction generator being used at the same time as a species of electromagnetic gear. By this means it is possible to reduce materially the size of both the rotary converter and the alternating-current generator, since while in Hobart's proposal it is necessary to have both capable of delivering the full output of the system, in the writer's plan, the output of the system is the sum of the outputs of the component parts, each of which therefore need only have half the capacity of the set.

The turbo-converter, then, in the form in which it has so far been developed by the writer, consists of an induction generator combined into one machine with a rotary converter, one member (preferably the primary) being mounted on the converter shaft and revolving with it, and the other, usually the squirrel-cage rotor, being mounted on the turbine shaft. Fig. 1 shows a diagram of connections of the device.

By mounting the generator primary on the converter shaft, instead of having it stationary as in Hobart's proposal, we make use of the driving torque required by the induction generator, or, in other words, the resistance which its rotor opposes to being revolved by the turbine, in order to drive the converter, which is thereby caused to generate direct current in addition to its function as a converter.

Let us take, by way of example, a 4-pole generator and 4-pole converter. Let the converter be running at 1,500 revs. per minute, say, and let the 3-phase induction generator be connected to 3-phase tappings on the converter armature through the hollow shaft.

At 1,500 revs. per minute 3-phase currents at 50 cycles will flow through the tappings on the converter armature. These tappings are so connected that the revolving field of the induction generator rotates the same way as the converter armature. In a 4-pole machine with 50-cycle excitation the revolving field will also go at 1,500 revs. per minute relative to the primary winding which produces it. Hence the total speed of the revolving field will be 3,000 revs. per minute, the sum of its speed relative to its primary and that of the primary itself. The squirrel-cage rotor, and, therefore, of course, the turbine, will go at approximately the same speed as the field. Hence we have obtained an apparatus in which the generator only runs at a fraction of the speed of the turbine.

A little reflection will make it clear that in an apparatus mounted

as described, since the driving torque of the induction generator also drives the converter as a direct-current generator, the torque exerted between rotor and stator of the induction generator must be identically equal to that between armature and field of the converter. They are, in fact, the same torque exerted at different points.

The input of power into any generator, of course, is :—

Power input = torque × relative speed of inductor and induced parts.

If, for the moment, we neglect the losses the power input and output will be the same.

We saw above that the torque of the two elements of the turbo-converter was identically the same. If both have the same number of

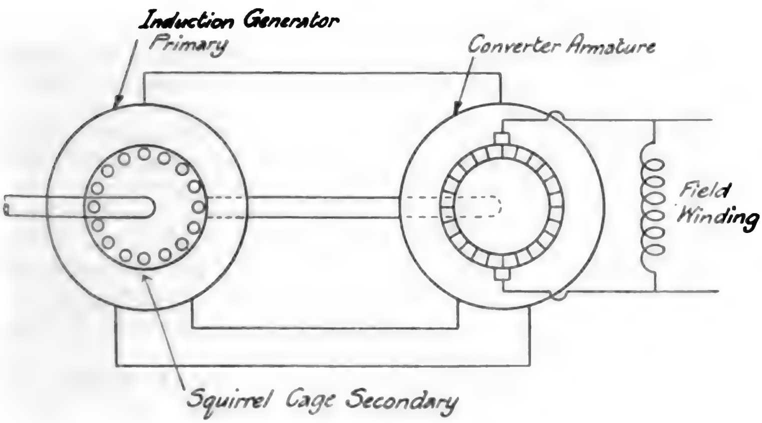

FIG. 1.

poles, the relative speed of inductor and induced parts will be the same in each, and hence each will generate the same amount of power. In addition to its function as a direct-current generator the converter, of course, changes the power of the induction generator, equal in amount to its own, into a direct-current form, and the total power flows out of the commutator of the set.

The reader will not fail to remark the analogy between the present apparatus and the well-known and very successful motor converter of Bragstad and La Cour. There are, of course, fundamental differences between the principles of action of the two machines, but the analogy is sufficient to lead us to hope that the newer machine may find a sphere of usefulness similar to that of the older.

Before passing on to a more detailed consideration of the characteristics of such a set, it may be as well to explain the reason for the use of an induction rather than a synchronous generator, which would

appear at first sight to be equally applicable. These reasons are few, but conclusive :—

1. If a synchronous generator were employed it would be necessary to synchronize the two revolving elements every time the set was started.

2. Owing to the presence of two sources of magnetization in the set—one in the converter and one in the generator field—it would be possible for the set to "hunt" if overloaded, and for it to fall out of step if accidentally short-circuited and be incapable of picking up again.

3. Collector rings would be required on the high-speed element to excite the field.

Characteristics of a Turbo-converter Set.

The converter portion of such a set will be distinguished from an ordinary rotary converter by a number of peculiarities of design.

1. Owing to its function as direct-current generator, it will require more copper on the armature than a standard converter—exactly the same amount, in fact, as the converter portion of a motor converter.

2. As the field of the converter has to perform the double function of magnetizing both converter and induction generator, it will necessarily be somewhat heavier than that of an ordinary converter.

The magnetizing current of the induction generator, in fact, differs 90° in phase from the working current, and circulates in the converter armature in such a position as directly to demagnetize the field of the converter. This field, therefore, must be supplied with an extra number of ampere-turns sufficient to counterbalance the magnetizing current of the induction generator.

In order to calculate the increase in the strength of the converter field required, we may proceed as follows :—

Express the reluctance of the induction generator by means of an effective air-gap in a manner well understood by electrical designers. Multiply this by the ratio of the air-gap magnetic density chosen for the induction generator to that of the converter.

Excitation must be provided for the resultant reluctance of the converter calculated as above.

Expressing the above calculation algebraically, it appears as follows :—

Let—

δ_c be the converter effective air-gap.
δ_g be the generator air-gap.
β_c be the converter gap density.
β_g be the generator gap density.

Then—

Resultant effective air-gap $\delta_c + \delta_g \beta_g / \beta_c$.

The effect of the induction generator on the saturation curve of the converter is shown in Fig. 6.

3. The drop in speed of a turbo-converter set from no load to full load would at first sight appear to be considerable, as the "slip" of the induction generator is added to the drop in speed of the turbine.

However, if the ratio of speeds of the turbine and converter are, say, 2 : 1, the effect of the slip on the speed of the converter is reduced in the same ratio, so that the resultant drop in speed is much less than might have been anticipated. This is best seen by a short algebraical investigation.

Let—

k_0 = the speed of the turbine.
k_1 = the synchronous speed of the induction generator.
s = the slip.
P_1 = the number of poles of the converter.
P_2 = the number of poles of the induction generator.

Then we have—

Converter speed $= k_0 - s - k_1$.

Frequency generated by converter $= (k_0 - s - k_1) \times \frac{P_1}{120}$.

Synchronous speed of induction generator

$$= \text{frequency of supply} \times \frac{120}{P_2},$$

or—

$$k_1 = (k_0 - s - k_1) \frac{P_1}{120} \times \frac{120}{P_2}.$$

Transposing and cancelling—

$$k_1 (P_1 + P_2) = (k_0 - s) P_1,$$

or—

$$k_1 = (k_0 - s) \frac{P_1}{P_1 + P_2}.$$

Substituting this value for k in the equation for the converter speed, we get—

$$\text{Converter speed} = (k_0 - s) \frac{P_1}{P_1 + P_2}.$$

If we neglect the slip for a moment this equation shows us that the general rule for finding the "gear ratio" of a set having any numbers of poles P_1 and P_2 is as follows :—

"Divide the number of poles on the induction generator by the sum of those on the converter and the induction generator, and the result will be the ratio of the converter speed to that of the turbine on no load."

In order to illustrate the influence of the slip on the converter speed let us take the rather extreme case of a bipolar induction generator coupled to an 8-pole converter giving a "gear ratio" of 5 : 1. If the

turbine runs at 3,650 revs. per minute, say, the no-load speed of the converter will be 730 revs. per minute. Suppose, now, the slip of the induction generator is 150 revs. per minute, or somewhat over 5 per cent. The full-load speed of the converter by the rule given above will be one-fifth of 3,500, or 700. Thus the converter has only dropped from 730 revs. per minute to 700 revs. per minute, *i.e.* 30 revs. per minute, or about 4 per cent, while the induction generator has slipped 150 revs. per minute.

There should be no difficulty in a machine of any size in reducing the slip of the squirrel-cage induction generator to 2 per cent, so that its effect on the converter speed will be very slight.

It has been thought desirable to discuss the subject of slip in the induction generator quite fully, as otherwise it might appear to have a seriously injurious effect on the regulation of the set. However, the above investigation shows that while the series field of such a set requires to be a little stronger than in an ordinary generator in order to offset this, yet the effect of the slip is of quite small magnitude.

4. Such a set can very readily be employed as a 3-wire set. All that is necessary is that the induction generator be connected in "star" and a tapping led from the neutral point to a collector ring, the brushes on which are connected to the neutral wire of the system. A test of the experimental machine as a 3-wire machine is shown in Fig. 8.

5. *Starting.*—It is, of course, obvious that the turbo-converter cannot be started from the steam-turbine end without auxiliary means. The steam turbine and squirrel-cage rotor attached will start up alone, while the converter portion remains stationary without showing any tendency to start. It is necessary to bring the converter up to a sufficient speed to generate enough voltage to excite the induction generator before the primary and secondary of the latter can get into step.

Methods of starting may be divided into two classes—electrical and mechanical.

Let us take electrical methods first.

1. If a supply of direct current is available, the converter may be started up as a direct-current motor, when it will bring the turbine up with it to its rated speed of twice or more that of the converter. Steam can then be admitted, and the set will be ready for load. This is by far the best plan where applicable, but of course it requires an appreciable supply of electric power to enable it to be used.

2. Another plan requiring only a very small supply of power is the following:—

Means are provided for causing a direct current to circulate in one of the phases of the induction generator during the starting period. The generator so excited acts as an electric clutch, which will bring both elements up at the same speed, and may be automatically thrown out of action when the converter reaches the desired speed.

Coming now to the mechanical methods:—

1. One of the best mechanical methods is that illustrated in Fig. 2. By means of an auxiliary idler shaft the turbine is belted on to the converter by the use of pulleys, which may conveniently be arranged to give the same velocity ratio as the induction generator.

On starting the turbine, the converter is brought up to its correct speed by the agency of the belts, and as soon as the field switch is closed will be ready for load. The belts may then be run on to loose pulleys and the idler shaft stopped. This may be done by hand or automatically by means of a solenoid operated by the voltage across the converter brushes.

Gearing could be used instead of belts in the above method, but would probably be less satisfactory.

2. An auxiliary turbine might be used to start the converter.

3. Primary and secondary of the induction generator might be coupled by a centrifugal clutch releasing when the converter reached its rated speed.

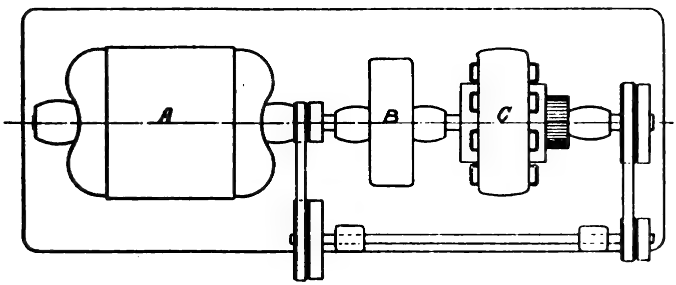

FIG. 2.—Starting Arrangement for Turbo-converter.

4. They may be coupled by a clutch operating when the torque between primary and secondary exceeds a certain value. A rudimentary instance of such an appliance is the device occasionally useful for experimental purposes, wherein primary and secondary are tied together by means of a predetermined number of thicknesses of thread.

When the machine reaches an appropriate speed, the torque between the two becomes sufficient to burst the thread and the two portions of the generator fall into step. When we come to criticize these methods with a view to picking out one for practical application, we find that all of them except the first electrical and the first mechanical methods are subject to the following criticism. So long as everything is in perfect order all of them will operate all right, but if the clutches or switches required failed to operate as expected, the converter would run the risk of being raised much above its rated speed, the result of which would very likely be disastrous. Means could probably be found to prevent this, but on the whole it seems better to adopt a method where this criticism cannot arise.

The first electrical method described is not applicable in the absence of a considerable supply of electrical power, hence we come to the conclusion that the best method of starting is by means of belts and an idler shaft as described.

Mechanical Design of the Turbo-Converter.

Two different forms of mechanical construction are possible for the turbo-converter. Firstly, the type illustrated in the drawings and

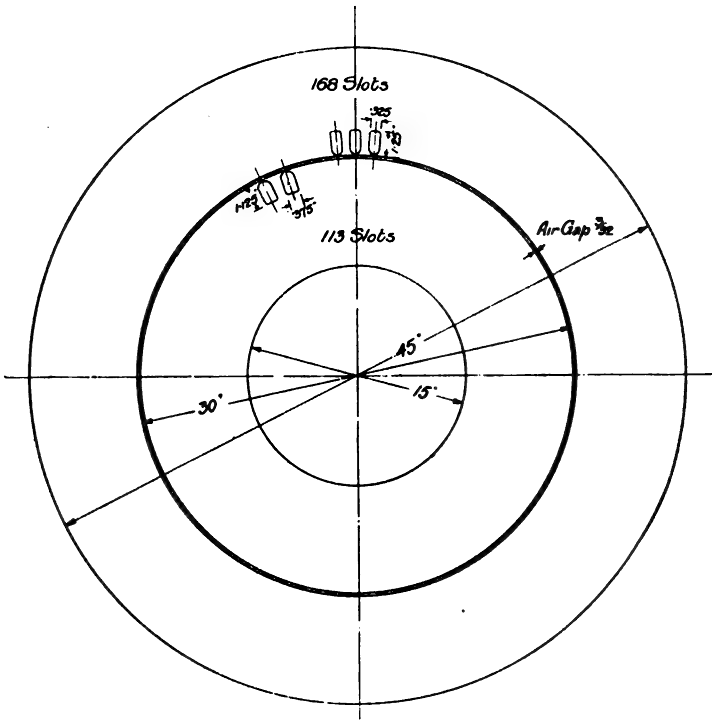

Fig. 3.—Punching for 500-kw. Induction Generator.

photographs, which may conveniently be called the "flywheel" type, or secondly that sketched in Fig. 6, which may be called the "spinner" type, as the mechanical construction is the same as that of the multiple speed induction motor made by Mavor and Coulson of Glasgow, and called by them the "spinner" motor. In this type the squirrel-cage rotor forms the inner element, and is driven direct by the turbine. Surrounding this is a second element, the "spinner" capable of free rotation and also supported by means of ball bearings or the like on the same bearing pedestals in which the squirrel-cage rotor runs. This

element bears the primary winding of the induction generator on its inside surface and the armature winding of the converter on its outside surface, being fitted with a commutator and brushes in the usual way.

Outside of this again is the field ring of the converter, carrying the pole-pieces and their windings, etc.

The author, however, has hitherto devoted his attention chiefly to the other or "fly-wheel" type of design, perhaps chiefly because it permits of the use of a standard design of converter.

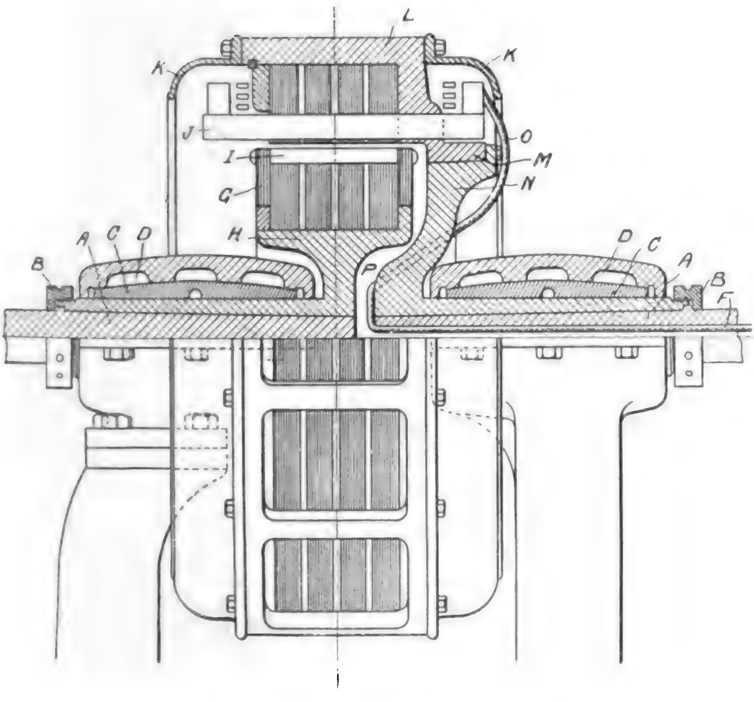

FIG. 4.

The cardinal feature of this, of course, is that both primary and secondary are "overhung," or, in other words, supported by a single bearing.

A description is given below of the design adopted by the writer to render this construction unobjectionable. The most essential feature of any single-bearing construction is to reduce the overhang of the centre of gravity of the overhung mass beyond the bearing-nose as much as possible.

It will be found in every case that the design adopted enables

us to reduce this overhang to a matter of a few inches beyond the bearing nose even in the most extreme cases, and for sizes of 500 k.w. In order to do this, an induction generator design of large diameter and short length must be adopted, of course. In order to give definitiveness to the description of such a design, I give below particulars of the electrical and mechanical design for the induction generator of a 500–600-kw. set.

PARTICULARS OF INDUCTION GENERATOR FOR 500-KW. 500-VOLT TURBO-CONVERTER SET.

Number of poles of induction generator	4
Number of poles of converter	8
Ratio of turbine to converter speed	3 : 1
Turbine speed (assumed) ...	1,800 revs. per minute
Converter speed (approximately)	600 revs. per minute
Relative speed of primary and secondary of induction generator	1,200 revs. per minute
Frequency of current in induction generator	40 cycles
Kilowatt capacity of induction generator ...	333 kw.
Connection	6-phase star
Full-load current per phase	400
External diameter	45 in.
External diameter (primary)	45 in.
Internal diameter (primary)	30 in.
Air-gap (single)	$\frac{3}{32}$ in.
External diameter (secondary)	$29\frac{13}{16}$ in.
Internal diameter (secondary)	15 in.
Number of primary slots...	168
Number of secondary slots	113
Useful dimensions of primary slot ...	$1{\cdot}25 \times 0{\cdot}325$ in.
Useful dimensions of secondary slot ...	$1{\cdot}125 \times 0{\cdot}375$ in.
Number of bars per slot (primary)	1
Number of bars per slot (secondary)	1
Size of primary bar	$1{\cdot}125 \times 0{\cdot}2$ in.
Size of secondary bar	$1 \times 0{\cdot}325$ in.
Net core-length	10 in.
Gross core-length	$11\frac{1}{2}$ in.
Number of vent ducts	3
Width	$\frac{1}{2}$ in.
Leakage coefficient (calculated by Hellmund's formula)	0·02
Full load efficiency	94 per cent
Full load power factor	92 „
Peripheral speed of primary (outside diameter)	7,000 ft./min.
Peripheral speed of primary (gap diameter)	4,650 „
Peripheral speed secondary (gap diameter)	14,000 „
Peripheral speed secondary (inside diameter)	7,000 „

It is clear from the peripheral speeds and diameters given that we need have no anxiety as regards the effects of centrifugal force on the primary. It is merely necessary to take care that the primary end connections are well anchored in any convenient way.

The peripheral speeds of the secondary are also quite moderate. A squirrel-cage rotor is, of course, ideal for high-speed designs. As there is only one bar per slot which can be pushed through from the end there is no difficulty in thickening the overhanging lip of the slot sufficiently to take care of the centrifugal force of the bar.

The only point which need cause us any anxiety is the short-circuiting ring of the squirrel cage. This cannot be made of steel, as it must consist of a high conductivity non-magnetic material. In this connection I wish to draw attention to the properties of aluminium for high-speed work. It will be found, on calculation, that for all high-speed work aluminium gives a factor of safety only inferior to that of good steel on account of its extreme lightness. Probably some of the new light alloys are still better in this respect. At present prices,

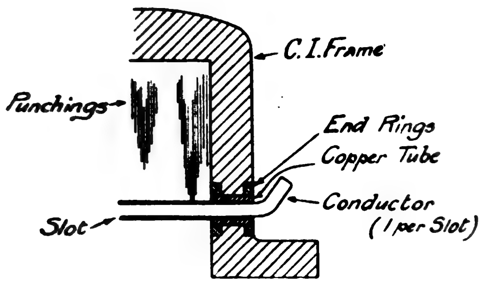

FIG. 5.

moreover, aluminium is, volume for volume, cheaper than any other material except cast iron and an ordinary grade of steel, again on account of its lightness.

As regards conductivity, it is a commonplace that an aluminium high-tension line is cheaper than a copper one of the same conductivity, so we may feel reassured on this head.

Hence, if we adopt the end-ring construction shown in Fig. 4, in which the end ring is made of rolled aluminium sheet, we have an ample factor of safety against centrifugal force.

Fig. 4 is a sketch, approximately to scale, showing the mechanical design of the induction generator whose dimensions are given above. It will be seen that the centre line of the secondary only overhangs the bearing nose by little more than an inch, while the slowly revolving primary is overhung by approximately 9 in. We may estimate the weight of this, including everything, as not over 3,500 lb., so that with an overhang of only 9 in. and a speed of 600 revs. per minute we are well within the range of flywheel practice.

The cardinal features of the design used to obtain these results are two in number.

1. The primary frame is made of a non-magnetic material in which a number of holes are cast corresponding to the stator slots. Through these holes the stator bars are brought, and so the stator winding is

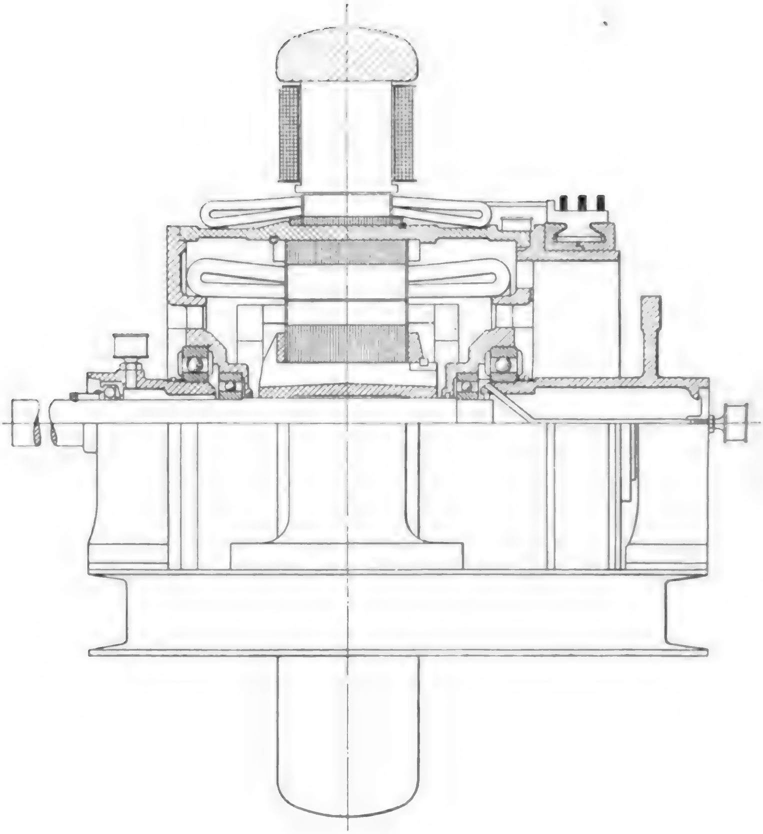

FIG. 6.

kept outside the frame. An alternative construction not requiring the use of a non-magnetic frame is shown in Fig. 5.

By this means we obtain the following advantages:—

(*a*) The overhang of the primary is reduced very much, as we do not have to allow space for the end connections inside the frame.

(*b*) The frame abuts solidly on the punchings all round, and there is no hollow space to cause mechanical weakness.

(c) There is ample room for the end connections, and they do not have to be cramped in any way. Any engineer who, like the writer, has had to design end connections for a cramped space, will know how great an advantage this is.

(d) There is ample ventilation for the end connections.

2. The other important feature of the design is the arrangement whereby the joint between the two hubs on which the primary and

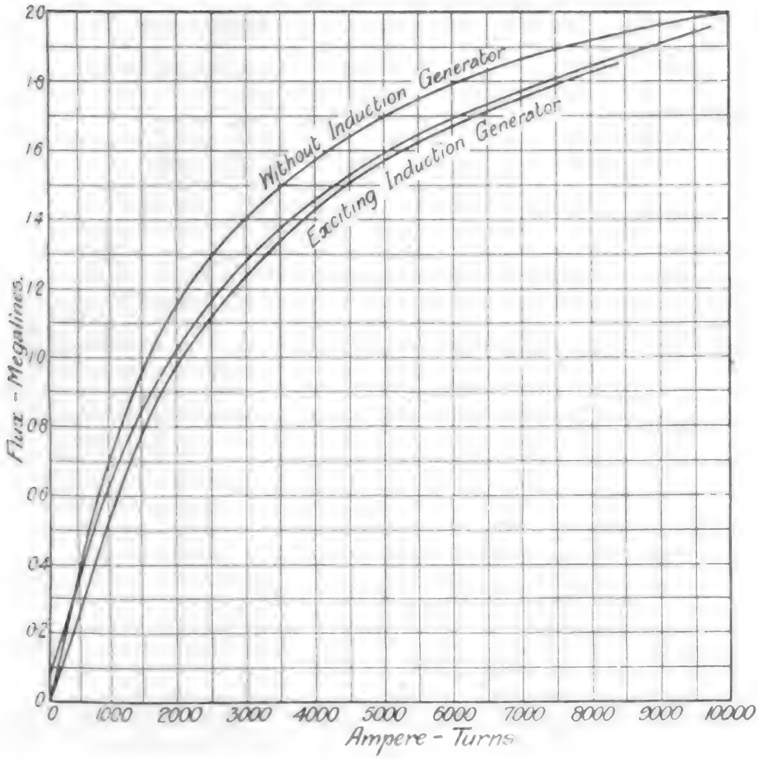

FIG. 7.—Turbo-converter (No-load Saturation Curves).

secondary of the induction generator are built up is made inside the bearing instead of on the projecting part of the shafts in the usual way. This enables us to have a quite ample and rigid bearing surface, and yet only have a shaft extension of about 3 in. in the set considered. The distance between the ends of the two bearing brasses is only about 11 in., scarcely more than it would be with an ordinary flange coupling.

The machine, in general, consists of two forced-lubrication, water-cooled bearings of large size supporting respectively one end of the

turbine and the secondary of the induction generator, and one end of the converter and the primary of the induction generator. Mounted on the turbine shaft by a very long taper fit and keyway is a steel hub on which the secondary of the induction generator is built up. This is built up of punchings in the usual way, the end rings of the squirrel-cage winding being of aluminium in which holes are punched for the bars, these being afterwards riveted over on the outside. The primary is built up within a frame provided with holes on one side through which the insulated stator bars pass. This frame is mounted by a taper fit of ample area and keyway on another steel hub similar

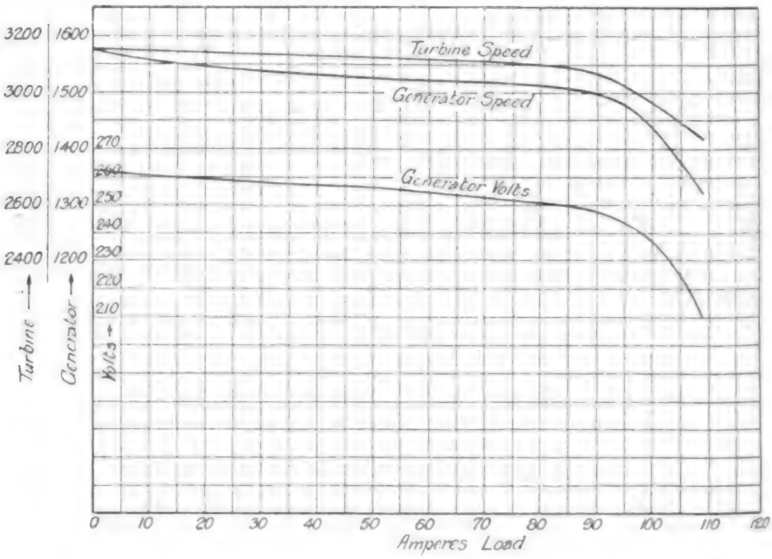

FIG. 8.—Load and Speed Curves as Shunt Generator (no Compounding).

in general design to that used for the secondary, which is again mounted by a taper fit within the bearing on the converter shaft. In order to save the space taken up by a nut on the front end, these taper fits are arranged to tighten up from the back as shown.

The leads are brought from the primary winding through the hollow shaft to the converter.

Finally, it will be of interest to describe the tests made on the experimental set mentioned above.

Fig. 7 shows the no-load saturation curve of the turbo-converter both with and without the induction generator connected, whence the demagnetising action of the latter will be clear.

Fig. 8 shows load and speed curves for the machine as shunt generator. In these curves the turbine speed is shown to one-half the

scale of the generator speed. Since half the speed of the turbine is the synchronous speed of the generator the difference between the turbine and generator speeds read off on the generator speed scale is the slip in revolutions per minute of the induction generator.

It will be seen that the load was limited by the capacity of the turbine, which began to fall rapidly in speed above 90 amperes load.

Fig. 9 shows curves of the set arranged as a 3-wire machine in the manner described above. In the machine as built no collector

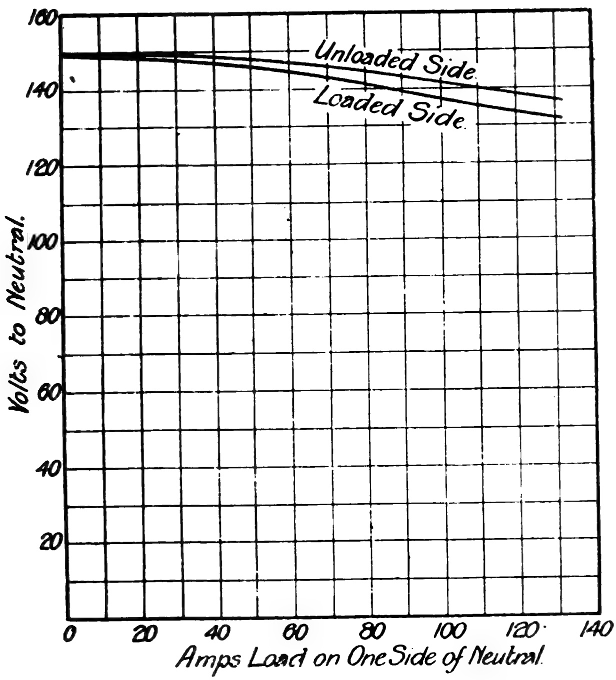

FIG. 9.—Load Curve as Three-wire Machine (no Compounding).

ring was provided for the neutral of the induction generator, and hence for the purpose of the test it was earthed and a lead taken from a stationary part of the frame for the neutral wire.

Load was then applied to one side of the system only, the other side being left entirely unloaded, with the results shown in the curve.

It will be seen that the maximum difference in voltage between the two sides corresponding to full-load current on one side and no load on the other was 3½ per cent.

The total voltage fell somewhat as one side was loaded, but this, of course, could be corrected by ordinary compounding coils.

Fig. 10 shows a number of retardation curves taken on the set under various conditions. Among other points, one of the most interesting is the following:—

Curves 1 and 2 are curves taken with the field off and the machine therefore unexcited. One, however, was taken with the secondary in place, and the other with the machine removed from its base and the secondary therefore out. The curves show that the retardation is quite

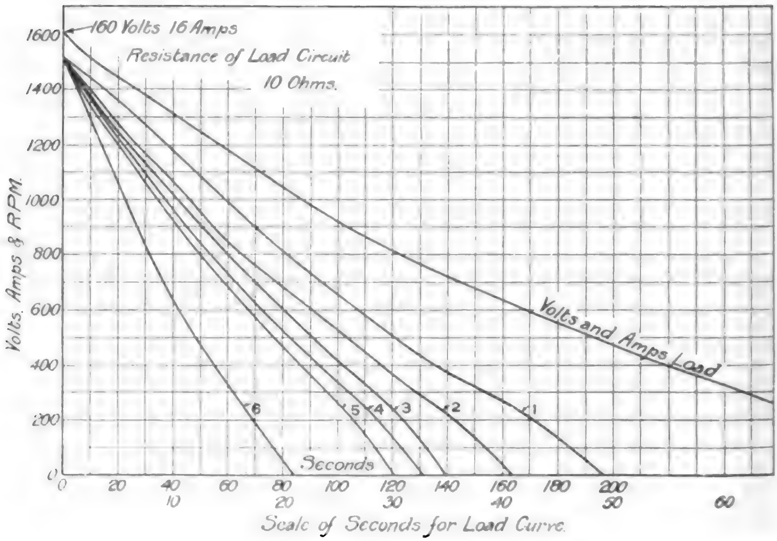

FIG. 10.—Retardation Curves.

appreciably less when the rotor is in place, a result which can only be ascribed to diminished air friction.

DISCUSSION BEFORE THE MANCHESTER LOCAL SECTION ON 5TH NOVEMBER, 1912.

Professor Walker.

Professor MILES WALKER: One of the good points about this apparatus is the fact that we have in the squirrel-cage rotor a perfectly simple mechanical construction for the high-speed rotating part of the generator. In addition to that we have the lower speed of the commutating part. I am inclined to join issue with Mr. Creedy as to whether it is possible to make a continuous-current generator run at a very high speed. In my own experience we can run commutators at something like 10,000 or 11,000 ft. a minute and keep the number of revolutions at 2,000 to 3,000 per minute without any difficulty whatever. The difficulties with turbo-generators in the past have arisen not so much from the high speed of the commutators as from the fact that

Professor Walker.

sufficient provision was not made for expansion and contraction; difficulties of that kind have been met with not only in turbo-generators but in ordinary continuous-current machines. They arise from constructing a commutator of a number of copper bars supported entirely on insulating material. When warmed up, the bars press upon their supports with very great force, so that when cool there may be a certain amount of looseness and some movement of the bars relatively to one another.

I admit that where a commutator is running at a high speed any roughness which may arise from this cause is more pronounced than it is on a very low-speed commutator where the inequalities can be followed much more easily. But if the commutator is running perfectly true—and a radical face commutator can be made to run perfectly true for years—then the mere fact that it is running at 3,000 revs. per minute and has a peripheral speed of 10,000 ft. per minute is absolutely no argument against it. That is best proved by an inspection of machines which can be found all over the country.

Mr. Creedy really takes a very easy case, that of a 600-kw. 500-volt machine giving 1,200 amperes, and he runs his turbine at only 1,800 revs. per minute. One would hardly call that a turbo-generator. There are in this country many machines delivering thousands of amperes and running at speeds of 2,500 revs. per minute; there also are machines delivering more than 1,000 amperes and running at 3,000 revs. per minute, machines which are running year in, year out, from Monday morning to Saturday afternoon. They simply run like an ordinary low-speed machine without any special attention being required by the brushes. I can show Mr. Creedy a brush which has been used for more than 18,000 hours—practically three years—on a commutator running at 2,500 revs. per minute, and the brush has only worn half an inch. On that same machine at the end of three years nearly all the brushes that we installed for the first run were still in use. It therefore gives an altogether false impression to say that a high-speed commutator cannot be built: it can be built, and it is built most successfully.

It is very important that this point should be established; it is not right that Mr. Creedy's remarks on this matter should go out to the world unchallenged; I therefore take the liberty of quoting one or two cases in point. In a certain works in Sheffield—a very important steel works where they use a large amount of continuous current—the engineer responsible for the equipment investigated very cautiously to see if high-speed commutators could be made to run satisfactorily with carbon brushes. Although there was some evidence of trouble from expansion and contraction he was finally convinced that these troubles could be met as in low-speed machines. He first ordered two 500-kw. turbo sets running at 2,500 revs. per minute, and each delivering 2,200 amperes. After trying these for more than a year he ordered a 1,000-kw. set, consisting of two 500-kw. generators in tandem, running at the same speed. This set supplies over 4,000 amperes continuously from

Professor Walker.

Monday morning to Saturday afternoon, and gives no more trouble than a low-speed generator. The Cunard Company—who are also very careful with regard to the kind of machinery they connect to their steam turbines—have looked into this matter. They have seen the machines at Sheffield and other parts of the country, and they have decided to use radial-type commutator, continuous-current turbo machines on their ships. The main reason for their decision is that they are satisfied that the reliability is great and the upkeep small. In face of these facts one cannot say that satisfactory high-speed continuous-current machines cannot be built; they can be built, and they are built. There are machines running here in Manchester, built by Messrs. Siemens Brothers, which are running in a thoroughly satisfactory manner.

Let us see what Mr. Creedy proposes instead. He has a machine which only runs at 1,800 revs. per minute. This is far too low a speed for a 600-kw. steam turbine. The size quoted by Mr. Creedy is—diameter 30 in., and length 11½ in. If we take the square of the diameter, and multiply by the length, we get about 10,400 cub. in. In the particular machine I have mentioned—the 500-kw. machine that has given such great satisfaction in Sheffield—D^2L is only 6,900: that is to say, in the turbo part alone of Mr. Creedy's machine the material is 50 per cent more than in a continuous-current generator. Not only is the generator cheaper, but the turbine is also cheaper, and the operation of the machine is very satisfactory. It is not true to say that the converter part of Mr. Creedy's machine may be made very small for its output; it cannot be made as small as an ordinary rotary converter. In an ordinary rotary converter the copper losses are only about 0·3 of the losses in a similar continuous-current machine delivering the same current; whereas in Mr. Creedy's machine they are about 0·75, so that more copper must be provided in the armature. The field winding also must be bigger than in the rotary converter, because it has not only to magnetize the field of the machine itself, but it has also to provide a wattless current for energizing the turbo-generator.

If Mr. Creedy can design on his principle a satisfactory 500-kw. machine running at 6,000 revs. per minute so as to give the turbine designer a good opening for a cheap machine, then one could see some advantage over the ordinary generator, which one does not care to run at higher speeds than 3,000 revs. per minute. Or it may be that for very large outputs the type of electrical reduction gear may be of service.

Whatever machine it is desired to build, whether an induction motor or a continuous-current generator, it is always possible to rotate the stator; if the stator is rotated at the same speed as the rotor the output is doubled. It may be that Mr. Creedy is right in putting his copper and iron on a spinner or overhung spider, and in that way doubling the output of his material. The future will prove whether engineers will come round to spinning the stators of their machines.

Although Mr. Creedy has said that the construction shown in Fig. 4

is a better construction than that in Fig. 6, I do not quite agree with him. I think, notwithstanding some little difficulty in the mechanical arrangement shown in Fig. 6, the whole construction illustrated there is more satisfactory than that in Fig. 4. In order to support the stator he proposes to use a non-magnetic support for the iron punchings and copper coils; that will be an exceedingly expensive design, because it has to be very thick in some parts, and it will also have eddy currents generated in it where the coils come through the slot. It may be possible to consider Fig. 6 as showing a construction for saving material; but whether the purchaser can be prevailed upon to pay a rather lower price and put up with the troubles that may arise from a mechanical construction to which we are not well accustomed is another matter. Professor Walker.

Mr. J. S. PECK: However much we may criticize Mr. Creedy's turbo-converter as a commercial machine it is certainly very interesting; but I do not believe that it is the final solution of the problem. Mr. Creedy refers to the commutator of a continuous-current turbo-generator as a mechanical monstrosity; what, then, would he call the arrangement shown in Fig. 2? I can hardly believe any engineer would accept such a machine if he had to operate it. First, there is a generator with both the field and armature rotating. This machine is coupled through a jack shaft with belts and fast and loose pulleys to another machine. The combination looks to me to be perfectly hopeless as a practical machine. Mr. Peck.

The commutator of a continuous-current turbo-generator is not an ideal mechanical construction; but there are, as Professor Walker said, many machines which are working and giving perfect satisfaction. Trouble comes when it is used for the very large sizes of very high-speed machines. The tendency on the part of the turbine designer is to force the speed of his machines higher and higher, so that I believe the final solution of the problem of driving large continuous-current generators will be found in the use of gears, or in the use of alternators and rotary converters. The rotary converter and alternator have been used quite extensively in America, while the gear drive seems to be coming into favour everywhere.

With a synchronous machine driving a rotary converter it is not necessary to synchronize; but when the two machines are connected together electrically, and started up together, the rotary converter comes up in step with the generator provided the fields of both machines are excited. Even if synchronizing were necessary, however, it would not make the arrangement impossible—synchronizing is done every day.

Returning to the turbo-converter, the use of non-magnetic material for supporting the primary laminations appears to me as it did to Professor Walker. The construction is very weak, and if the arrangement shown in Fig. 5 were used I cannot see that it would avoid the electrical difficulties. If non-magnetic material is used to bush the openings through which the armature conductors pass there would still be heavy

Mr. Peck. currents set up in the magnetic material; and the use of these bushes would, of course, decrease the strength of the whole frame, since it increases the amount of material that has to be cut away, so that there would be very little left to support the frame.

The problem of carrying heavy leads through the shaft is also one which would have to be considered; and while it could probably be arranged without great difficulty it is one of the numerous problems affecting the mechanical construction. Professor Walker has already pointed out that the continuous-current machine must really be much larger than a rotary converter having half the output.

There is one other point, Mr. Creedy has chosen a speed reduction of two to one; if he uses some other ratio in order to get a higher turbine speed or a lower speed on the continuous-current generator, then the claim that the capacity of each machine is only one-half the total capacity of the set is no longer true, even theoretically.

Mr. Cramp. Mr. W. CRAMP: There is one point which has not been touched upon by previous speakers, I refer to Mr. Creedy's suggestion that electrical engineers are very conservative in adopting new ideas. I think it is only right to call attention to the fact that of all engineers in the country, or in the world probably, electrical engineers are the most open to welcome new ideas. It would really be more true to say that if there is any tendency at all in the electrical engineering world it is rather in the direction opposite to conservatism. Every manufacturer is so concerned to put on the market something different from his neighbour, and something that is largely new, that we have no standard at all in this country. As an instance, look at the general principles involved in this very machine. At least twenty-five years ago a very large generator was put down in Berlin by Siemens with a ring armature and overhung bearings similar in principle to that shown by Mr. Creedy; I believe that machine is still running. Again, Dr. Ferranti told me at least fifteen years ago that one of his first generators was designed with both the field and armature rotating, and that he still believed in that principle if it were not for the difficulties to be overcome in the matter of bearings. Again, a German company put on the market some ten years ago a line of small motors having ring armatures in which both the inside and the outside of the ring were utilized, and, as a result, the armature had to overhang the bearing.

Both the well-known "Boucherot" motor and the "Spinner" motor have had a certain amount of success, though both depend upon the principles of which Mr. Creedy is afraid in this machine, so that it does not necessarily follow that the new machine is condemned from the mechanical standpoint.

On page 218 there is a point which may prove a difficulty to other readers of this paper. Mr. Creedy refers to a machine running at 3,650 revs. per minute at no load. He then says "the full-load speed of the converter by the rule given above will be one-fifth of 3,500, or 700." The induction generator has slowed down with load 150 revs.

per minute, and yet on well-known principles an induction generator ought to run above synchronous speed to give any power. The explanation appears to be, that though the actual speed of the rotor decreases, its speed relatively to the stator has increased, because the latter is running in the same direction but faster. I should like Mr. Creedy to confirm this. Mr. Cramp.

With regard to the arrangement shown in Fig. 2, it seems to me that there will be mechanical troubles with the belt drive. The pulley upon the turbine shaft, for instance, can hardly be less than 1 ft. or 18 in. in diameter, and it may even be greater; but if the turbine is run at 3,000 revs. per minute the belt speed will be of the order of 10,000 ft. per minute, which is excessive.

Then on page 223 the use of aluminium is referred to. Mr. Creedy says that, "volume for volume aluminium is cheaper than any other material except cast iron." Now it is not clear what he means by "volume for volume"; does he mean volume for volume with regard to the comparative heat, or comparative losses; or does he mean volume for volume as regards actual cost? I shall be glad if he will give an explanation of what the volume is for copper and aluminium, both from the point of view of cost and of heat. Professor Walker made a comparison between the dimensions of the machine mentioned on page 222, and those of a rotary converter for the same output. He gave the D^2L values as 10,000 and 6,000 respectively, but he did not allow for the fact that his rotary ran at a much higher speed, so that the comparison was not fair.

There is one other point to which I would call attention; by the principles of the machine enunciated on page 215, it would appear that the power developed by each part of the combination will be identical. But this is only true if the speed of the converter be one-half that of the generator. In any case, the power to be developed by either half changes with the ratio of its speed to that of the other half.

Mr. G. W. WORRALL: The troubles in the case of a continuous-current turbo-generator are not entirely at the commutator, but between the commutator and the armature. The commutator connections are apt to break, and dust also lodges under the armature bars behind the commutator. The latter trouble is very difficult to avoid, as the dust is hardened by centrifugal force into a compact mass across which the current may leak to the shaft and eventually "earth" the armature. The conductors and insulation are so packed together that usually the only repair possible is a complete rewind, which is a costly item. If the speed could be kept below 1,000 revs. per minute such troubles would not occur. I do not think Mr. Creedy has made himself quite clear at the bottom of page 214 with regard to the value of the torque; I hope he will explain this more fully. Mr. Worrall.

Mr. A. E. MCKENZIE: I think we must all agree, from the fairly numerous examples we have before us, that the high speed continuous-current generator with high-speed commutator is possible, and we cannot turn our eyes from the fact. Even supposing, however, that Mr. McKenzie.

Mr. McKenzie. this were not so, my contention is that the high-speed turbine driving through single-reduction gear a continuous-current generator is preferable to the arrangement that has been shown. I have in my mind a 500-h.p. turbine which has been running for over twelve months almost continuously at 7,500 revs. per minute, driving a pump through single-reduction gearing at 750 revs. per minute; if it is the aim of most turbine builders to get high speed, then surely a speed of 7,500 revs. per minute is sufficient. The gearing after working continuously for over twelve months shows really no sign of wear at all; it is exactly the same as when put in. This is a case which shows that it is possible to get a high-speed gear to-day that is perfectly satisfactory.

I will state another case. A firm of engineers has offered a friend of mine a turbine which will run at 3,500 revs. per minute; it is of 3,000 h.p., and will drive a 1,800-kw. continuous-current generator through a single reduction gear at 1,000 revs. per minute. That is a definite proposal which has been put forward to my friend, and I have no doubt that it can be made a perfectly satisfactory job.

Now to consider the machine the author has put forward. Those of us who have been unfortunate enough to deal with large units which have been subjected to very heavy short-circuits will not look with very great favour upon the mechanical design of the stator. Previous speakers have referred to it, and the author himself has commented upon its weakness; I certainly should not like to be the first to install a machine of that type. I know from painful experience that the stresses are so great that we should certainly have trouble with the overhanging stator which the author has illustrated. Of course we all must admit that it is worth while to try to reduce the speed of the continuous-current generator; but in view of the successful machines available at the present day, I do not think it is absolutely essential.

Mr. Frith. Mr. J. FRITH: I know of a geared turbine which drives a rolling mill through double helical gearing, the speed being reduced from 2,000 to 70 revs. per minute.

Mr. Cooper. Mr. A. G. COOPER: With regard to turbines and gearing, the De Laval turbines are all driven at a high speed with reduction gearing, and in most cases two dynamos are used, which are driven in opposite directions by means of spur wheels that gear with a central pinion on the turbine shaft. This equalizes the thrust and makes a very good job. The only objection is that they make a terrific noise owing to the gearing running at high speeds. They are working in small sizes up to 16,000 revs. per minute. I believe that several machines of a fairly large size have been built upon this principle.

DISCUSSION BEFORE THE SCOTTISH LOCAL SECTION ON 10TH DECEMBER, 1912.

Mr. Robertson. Mr. J. A. ROBERTSON: Compared with the turbo-alternator and rotary converter Mr. Creedy's machine appears somewhat complicated. I should like to know its overall efficiency. I do not agree that the direct-current turbo-generator is a mechanical monstrosity. That

statement would have been true ten or twelve years ago, but designs are different now. I have recently seen a 1,000-kw. machine which has been in use for two years and shows practically no more commutator wear than an ordinary engine-driven generator of the same output. The mechanical design of Mr. Creedy's machine appears to present serious difficulties; and these would be still greater in larger sizes, say 2,000 kw. Another point is the use which could be made of such a machine if the mechanical difficulties were overcome. The tendency is to concentrate plant in large stations and transmit multiphase current at high pressure to sub-stations. Direct current in large amounts is rarely required at the generating station, so that there seems to be little need for such a machine except in the case of isolated plants. The efficiency of rotary sub-station plant is remarkably good, and in three sub-stations with an aggregate capacity of 2,500 kw. we have obtained over twelve months' working an overall efficiency of 91 per cent, inclusive of transformer losses. Taking into account the necessarily high cost of Mr. Creedy's turbo-converter and the complication in design, it is difficult to see where its advantages would arise as compared with a turbo-alternator and a rotary converter. The balancing curve on page 227 is certainly very good and better than I have been able to obtain by the usual arrangement of connecting the neutral point of a star-wound transformer to the middle wire of a continuous-current system. Quite recently I have installed rotary converters with series-wound reversible boosters mounted on an extension of the rotary shaft, and I must say the system works satisfactorily for ordinary variations in voltage; but there is a difficulty in running such a set in parallel with a steam-driven or motor-driven balancer. Mr. Robertson.

Mr. F. A. Newington: I agree with the author that the high-speed commutator is a mechanical monstrosity. It is an unmechanical mixture of mica and other things, and this is a difficulty still not overcome in high-speed continuous-current machines. As long as it is necessary to work with mica or other compressible material for insulation it seems unlikely that we shall be able to get a really rigid construction of commutators. It is only after actual experience that one could say anything definite about the working of the electrical gear described, but I think for large sizes it might be more reliable and convenient than mechanical gear. I do not see how a machine can be obtained with cut gear running at 3,000 or 4,000 revs. per minute which would not wear after running for some time; and if wear did take place it would make such a noise that it would be impossible to run the gear. My experience of small gears is that they are a continual source of trouble. I disagree with Mr. Robertson about there being no need for continuous-current plant in large power-supply stations, but it seems that we shall be compelled to adopt alternating current simply because of commutator troubles. Mr. Creedy's design is ingenious, but I cannot say whether the overhanging arrangement would cause trouble. The question of cost would also have to be carefully considered. Mr. Newington.

Mr. Bunting.

Mr. J. H. BUNTING : I should like to know if this type of machine has been constructed of any appreciable size, and also whether any of these machines have been put into commercial use.

Mr. Spence.

Mr. W. L. SPENCE : Nearly every one who has worked at mechanical and electrical problems has at one time or another considered the spinner idea. From my experience there is little likelihood of that type ever being generally adopted. I foresee the greatest difficulties with the bearings, and I think that it would be almost impossible to ensure both good lubrication and complete oil tightness; the latter, in view of the position inside the windings, is absolutely essential.

Mr. Mavor.

Mr. SAM MAVOR : The mechanical features of Fig. 4 seem to me not so good as the spinner form shown in Fig. 6. My firm have been in communication with very experienced builders of large marine turbines on the Clyde, with special reference to the mechanical features involved in the spinner motor, especially of large sizes, and these engineers have expressed the view that there is no insuperable difficulty in the mechanical design of the spinner motor in large sizes. They considered that in their turbine practice they had overcome difficulties much greater, and so far as the mechanical features were concerned they saw no special difficulty in the spinning design.

Mr. McWhirter.

Mr. W. MCWHIRTER : I have seen a good deal of trouble with high-speed commutators, and after considering the efforts which have been made during recent years to get over those troubles by means of steel rings and mica bushes, etc., there are good reasons for differences of opinion as to the operation of high-speed continuous-current machines. No doubt a generator of such a design as Mr. Creedy suggests would be an important departure; but the building of it in large sizes is a problem which might require exceptional treatment.

Mr. Creedy.

Mr. F. CREEDY (*in reply*) : I do not dispute in the least that the high-speed commutator is possible, or that it can be constructed satisfactorily. I also agree with Professor Miles Walker in saying that the difficulty does not arise directly from the fact that the commutator is rotating at a high speed, but from the bad effects that the high speed produces indirectly. Admittedly these commutators heat excessively, or else the excessive expansion and contraction of which Professor Walker speaks would not take place; and the cause of the heating is quite clear. If we have two commutators, one running at 3,000 ft. per minute and the other at 9,000 ft. per minute, each carrying the same current, the high-speed one will give rise to exactly three times as much friction loss as the low-speed commutator, other things being equal. No design whatsoever, radial or otherwise, is of the slightest avail to modify this fact. If this is not a point against the high-speed commutator, I do not know what is.

I think Mr. Cramp has already replied to Professor Miles Walker's remark about the value of D^2L in the two types of machine. It will be sufficient for me to say that Professor Walker has taken a machine running at 3,000 revs. per minute and compared it with a machine

running at only 1,800 revs. per minute ; and he then complains that the value of D^2L in the latter machine is greater than in the former. Mr. Creedy.

I think I have been slightly misunderstood as regards the apparatus shown in Fig. 2 of my paper. This has no pretension whatever to represent a practical design. It is simply intended to show the principle of the method of starting that I prefer, namely, by means of some kind of gearing which brings the generator on no load up to its normal speed before it is excited or coupled magnetically to the turbine. I simply chose belting as the simplest kind of gear : any other kind would satisfy me just as well.

With regard to Mr. Peck's criticisms of the details of my proposed design ; some of these may be quite justifiable, others may be more a matter for individual opinion. In any case, they would naturally arise in the drawing-office stage of an actual design, when they could be settled one way or the other, so that I need not defend them at present when we are chiefly concerned with fundamental principles.

It may be useful, however, to explain a little more fully the construction proposed in Fig. 5 to obviate the use of non-magnetic material. In this construction the holes through which the conductors pass are bushed by copper tubes, which are short-circuited by means of an end ring on the inside and outside of the iron frame ; these tubes carry a system of currents equal and opposite to those in the active conductors. They therefore completely annul the magnetic effect of the currents in the latter.

As regards the losses in these tubes, if we suppose the frame is 2 in. thick the additional loss will be equivalent to that produced by adding 2 in., or a little more, to the length of each of the active conductors ; this is on the assumption that the current density in the tube is the same as that in the conductor. To obtain room for these tubes, I see no difficulty in off-setting the holes so as to put them in two layers instead of in one. This, however, is again only a suggestion, and on full consideration we might prefer to carry the frame outside the conductors altogether, and so avoid the holes.

Mr. Cramp has given us some details of machines with over-hung rotor, which are very welcome.

As regards the difficulty of accounting for the drop in speed of the converter under load, I think this is best explained by remembering that a drop in speed of the converter and the attached primary of the induction generator means a greater difference in speed between the primary and the secondary which is attached to the turbine and which is supposed to go at a constant speed. Thus under load the speed of the induction-generator primary increases relatively to the secondary, though it decreases relatively to a fixed frame.

Another difficulty which seems to have given trouble is to account for the fact that the torque of the turbine is the same as that of the induction generator and of the converter, since the turbine transmits twice as much power as that dealt with by either of the electrical elements. It is enabled to transmit twice as much power with the

Mr. Creedy. same torque, of course, because it runs at twice the speed. For instance, if T be the torque of the turbine, induction generator, and converter, being the same for all three, while the turbine speed is, say, 3,000 revs. per minute, the relative speed of the two portions of the induction generator is 1,500 revs. per minute, which is likewise the relative speed of the two elements of the converter. Equate the power transmitted by the turbine shaft to the sum of the power converted by the induction generator and of the power converted by the converter, and we get the equation—

$$T \times 3000 = T \times 1500 + T \times 1500,$$

an equation which is obviously correct.

The relative prices I gave for aluminium and other materials referred simply to prices per cubic inch.

I am glad that Mr. Mackenzie has raised the question as to what happens during short-circuiting, as it enables me to mention a point which I might otherwise have forgotten. The induction generator has a definite over-load capacity, and cannot transmit more than a certain maximum torque without falling out of step and coming to rest, whereupon the whole machine ceases to generate current while the turbine goes on running on no-load. This maximum torque can be adjusted by the designer to have any desired value, say, 2½ times full load, in which case the machine cannot under any circumstances whatever be subjected to more than this load. From this point of view the induction generator is equivalent to a slipping clutch, such as is sometimes used as a safety device. Sudden short-circuits, such as do so much damage in alternating-current generators, are not to be feared in continuous-current apparatus, as the self-induction of the latter can be made high enough absolutely to prevent such occurrences, since self-induction is quite harmless in continuous-current circuits.

I should like to know what happens to a geared turbine on a short-circuit, sudden or otherwise; nothing less than stripping of the pinion, I should imagine.

The consensus of opinion at the Manchester meeting, with the exception of Professor Miles Walker, seems to be in favour of some change from the present type of continuous-current turbo-generator. Some speakers, for example, Mr. Peck, favour the alternating-current generator and a rotary converter; others a geared turbine; while I have endeavoured to bring forward a third proposal, which is a form of electrical gearing. Which of these forms will be adopted depends on many factors. I have shown the advantages of electrical gearing over mechanical gearing, both in enabling us to reduce the size of the generator and in providing a slipping clutch and other safeguards which a mechanical gear cannot supply. For this reason, and because of the low speed of the commutator, I feel entitled to claim that this device is more reliable—and not less reliable—than the others which may be proposed.

I here propose to give a brief comparison between my device and helical gearing :— Mr. Creedy

Helical Gear.	*Turbo-converter.*
Efficiency, 95 per cent.	Efficiency, 92 per cent.
Capacity of generator equal to capacity of turbine, less losses.	Capacity of converter, say 60 per cent or 70 per cent that of turbine.
Losses in generator, say 8 per cent capacity of turbine.	Losses in converter, say 5 per cent or 6 per cent of turbine, on account of reduced power handled by converter.
Effect of short-circuit is to strip pinion.	Automatic protection against short-circuits by slipping clutch action.

To explain the above statements a little more fully, I may say that the efficiency of the gearing is a little higher than that of the induction generator ; but this is compensated for by the reduced losses in the continuous-current portion of the apparatus. In addition to this we have the safeguards mentioned above against trouble on short-circuit ; safeguards which do not exist with the gearing.

As regards cost, I think it very doubtful whether the gearing in large sizes, judging by such examples as I have seen of the Melville-Macalpine gear, would not come out considerably more costly than the induction generator portion of the present device ; while, of course, there is in addition a net saving on the continuous-current generator.

It is a mistake to think of these gears as merely a pair of standard spur gears, such as are used on tramway motors ; they are enormously more elaborate than anything of this kind.

One of the chief advantages of the set over an alternating-current generator and rotary converter is the fact that the sum of the outputs of the two parts is the output of the set, while in the rotary converter and alternating-current generator each element must have the full capacity of the set.

The arguments brought by several speakers against the use of this device apply to every kind of continuous-current machine. It must be admitted that if continuous-current apparatus could be done away with, it would be very desirable. But until this takes place the justification for this type of machine is just as great as that for any other type of continuous-current machine.

I am interested to hear from Mr. Mavor that turbine builders consider there would be no insurmountable difficulties in carrying out the "spinner" idea. This, perhaps, lends point to the statement at the beginning of my paper that electrical engineers are a little too conservative in mechanical matters, and consider certain devices to be impossible, although such devices seem quite simple to engineers whose whole attention is given to mechanical matters.

THE CONTROL OF METERS, PUBLIC LAMPS, AND OTHER APPARATUS FROM THE CENTRAL STATION.

By W. DUDDELL, F.R.S., A. H. DYKES, and H. W. HANDCOCK, Members.

(*Paper first received 26th September, and in final form 23rd November,* 1912.)

Except in certain isolated cases where special mains have been laid for the purpose of connecting or disconnecting remote portions of the distributing network, the problem of the control of distant apparatus from a central point on an electric light or power system is one that has been neglected to a singular degree, or possibly it would be more correct to say that it has, after merely superficial examination, been dismissed as impracticable.

Careful search shows that attempts at its solution have been made along three main lines :—

(*a*) By reversing the polarity of the mains when required.
(*b*) By running separate mains or pilot wires back to the station.
(*c*) By earthing one pole at the station and at the distant apparatus, and operating the latter by means of currents through the earth.

None of these attempts, however, have led to a satisfactory solution.

If, in the first case, the object of the distant control is to alter the registration of a meter, it is not exactly a practicable proposition to reverse the polarity of the system at different periods to make the meter register at different rates, having regard to the fact that it is not unusual to find arc lamps and storage batteries connected to the circuit.

The second proposal is to lay separate wires from the station to pick up every point where it is required to control a switch or relay for the thousand and one purposes for which they would be useful, but it is not practicable in the case of existing stations to break up roads everywhere for the sake of putting in additional wires, besides which there are other objections too numerous to mention here.

The third proposal is to use one of the existing light or power leads as the lead for the current to operate an ordinary relay or switch and

the earth as the return. This system, which is much more practical from the point of view of expense, does not at first sight appear to be very feasible in everyday work when one considers the ever-varying conditions that obtain as regards earthing.

Perhaps at present the best-known substitute for distant control is the local clock-work switch which can be set to switch the current on and off, or make any other change at any predetermined time.

A large number of different patterns have been put on the market and are in extensive use. The principal disadvantages are: high capital cost, cost of upkeep, the necessity for continually adjusting them to suit the varying hours of darkness, and the trouble in winding and setting them.

In addition, they have no powers of discrimination, and cannot, for instance, switch on public lamps in the case of a sudden fog.

Be the reason what it may, the fact remains that for public lamps, for instance, failing a separate system of mains, it is still in most cases necessary to send a man round to switch on each lamp individually, with the result that the first one has to be switched on long before it is really wanted, because of the time—sometimes two hours or more—taken by the lamplighter to get round his district.

The same delay occurs in switching off, with the result that a large number of units, which may amount to 10 per cent of the whole, are used unnecessarily and the life of the lamps is shortened.

The growing use of electricity for street lighting, and for various domestic purposes apart from lighting, has made it more than ever desirable to find some inexpensive method of operating switches or relays to control at will any particular arc or incandescent lamp, meter, motor, or the like, without in any way affecting the remaining apparatus connected to the mains.

It occurred to the authors that it would be possible, either on an alternating-current or direct-current system, to effect this by altering the constitution or wave-form of the main supply current when desired, leaving the mains and the system of distribution untouched.

In the case of either a direct- or alternating-current supply, one can superpose on the circuit a small high-frequency alternating current without affecting the lamps, motors, or other apparatus connected to the circuit, provided that the voltage and frequency of the superposed current be properly selected.

The underlying principle is to control relays, which may be inserted anywhere on the general system of mains, by means of a superposed current impressed on the main current flowing in the system. If the main current be continuous, then the control current may be alternating of any desired frequency. If the main current be alternating then the impressed current must be of a different frequency, the relay in either case being so designed that while it instantly responds to the impressed current it is unaffected by any alteration of the main current or pressure.

In the case of an alternating-current system one may superpose a

direct current, that is, move the zero line of the wave-form a little away from the symmetrical position. We do not propose to consider this alternative in the present communication owing to certain difficulties in its practical application.

It is obvious that if the main generators of a supply system give direct current at a pressure of 200 volts, the addition—in series with them—of an alternating-current generator producing a pressure of 10 volts, will not, provided the periodicity be above, say, 25, appreciably affect the lamps on the mains, the voltage at the lamps simply oscillating a little on either side of 200.

In the same way, if on an alternating-current circuit at 200 volts and 50 cycles there be superposed a voltage of 10 at a periodicity of 200, the lamps are practically unaffected.

As long as the feeder busbar voltage * is kept constant, the amount of power supplied to consumers is practically unaffected by the ripple. The output of the main generators is simply reduced by an amount equivalent to the power produced by the "ripple generator."

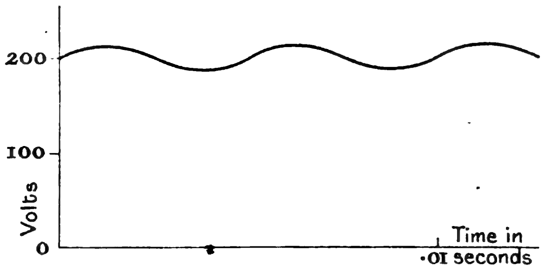

FIG. 1.

Fig. 1 shows the effect of superposing an alternating current of 10 volts at 200 frequency on a 200-volt direct supply, and Fig. 2 the wave-form obtained when the same alternating current is superposed on a 200-volt 50-frequency supply.

The problem was to design a satisfactory relay, cheap to manufacture and economical in operation, which would be unaffected by changes in the main current but would respond instantly to the superposed current.

In direct-current systems the solution is simple. In Fig. 3 let L represent the relay connected as a shunt across the mains and in parallel with the lamps and with the meter to be controlled. If a condenser, K, be placed in series with the relay, it is obvious that as long as there is only a continuous voltage on the mains A, B, no current passes through the relay. As soon, however, as an alternating voltage is impressed on the mains a current passes through the relay and

* It is assumed that the voltage is measured by a voltmeter of the dynamometer or similar type whose indication depends on the mean squared value of the voltage, and that the ripple voltage is small compared with the main voltage.

causes it to actuate the particular mechanism required, such as the switch S.

When the main supply is alternating, the problem is not so simple, inasmuch as the condenser will allow a current to pass through the

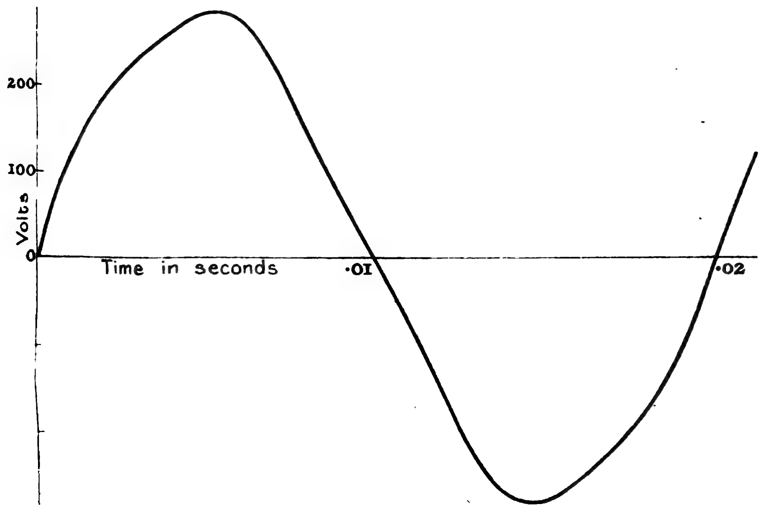

FIG. 2.

relay from the main 200-volt 50-cycle supply as well as from the small superposed ripple.

The relay must in this case be made "selective," so that it will

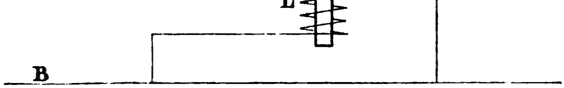

FIG 3.

operate only when a current of the selected frequency is impressed on the line. Such a selective relay is also to be preferred for a direct-current supply, as it enables several relays adjusted for different frequencies to be employed, any one of which can be brought into action as required.

In order to achieve this result, we take advantage of the principle of resonance, and so choose the self-induction of the relay L, and the capacity of the condenser K, that at the frequency F′, at which the relay is intended to operate, they are in resonance, whilst at the normal frequency F they are not. That is to say, neglecting resistance, the following relation is established :—

$$L \times K \times (2\pi F')^2 = 1.$$

When this relation holds, the potential differences between the terminals of the relay and of the condenser are much greater than the applied potential difference, and consequently the current passing through both the relay and the condenser is much greater than that due to the applied voltage. The two potential differences are out of phase with each other, and their vector sum is equal to the applied voltage. In practice we find the potential difference between the terminals of the relay coil at the resonance frequency to be some ten times the applied potential difference.

The selection of the correct frequency for which the relays are to be constructed is a somewhat difficult matter. A high frequency would be convenient from the point of view of keeping the capacity of the condenser K small, and hence reducing its cost, but capacity and self-induction of the mains, feeders, etc., in a distribution system preclude the use of very high frequencies. The lower frequencies, on the other hand, are liable to come too close to the working frequency of the supply and lead to further difficulties. In considering the choice of the frequency it is well also to avoid odd multiples of the supply frequency, as there is a chance of these frequencies being present in the wave-form of the main generators as harmonics.

These considerations have so far led us to adopt, on alternating circuits, frequencies comprised between the third and fifth harmonic of the supply.

It is possible to make the resonance very sharp by keeping down the losses in the condenser and in the relay to very small values. Small losses in the condenser present very little difficulty at the present day, as good paper condensers are quite cheap. The losses in the relay L are more difficult to keep small without spending an undue amount of money on the copper of the coil. There is, further, another reason which tends to discourage the use of very sharp resonance, namely, that if the resonance be very sharp then a very small change in the frequency of the superposed alternating current will cause the relay to fail.

We have already mentioned that the superposed alternating current or ripple is small compared with the supply voltage. From many points of view it is advisable to keep this ripple small. Experiments have led us to conclude that a satisfactory value for the R.M.S. value of the superposed voltage is 5 per cent of the supply voltage whether direct or alternating. That is to say, on a 100-volt circuit, direct or alternating, the R.M.S. voltage of the ripple is to be 5. The simple

resonance circuit described above works perfectly in the case of a direct-current supply, and has many advantages over the non-resonant relay; but in the case of an alternating-current supply we find that unless the resonance be made unduly sharp, or there be a large difference between the frequencies, the condenser K and relay L will let through sufficient current at the lower or supply frequency to attract the armature of the relay. For instance, with a supply circuit of 100 volts 50 frequency it seems impracticable to build commercially a plain resonance relay to work with 5 volts superposed.

To get over this difficulty we add an additional or compensating circuit to the relay, as illustrated in Fig. 4. L, as before, is the relay coil, the armature and core being omitted for the sake of clearness. On top of this coil is wound a second coil, L_1, which may have, roughly, the same number of turns; and this coil is connected in series with a choking coil, L_2, having a high self-induction. The current through the condenser K and relay coil due to the supply frequency, say 50, leads

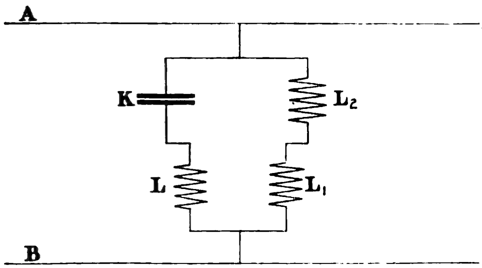

FIG. 4.

almost 90° on the applied potential difference. The current through the choking coil L_2 and compensating windings L_1 is made to lag about 90°. If these two currents be adjusted to approximate equality their action on the core of the relay can be made very small. It cannot be completely eliminated, because the two currents are not exactly at 180° to one another; but it is sufficient for practical purposes. By making the choking coil L_2 with an adjustable air-gap in its magnetic circuit it is very easy to adjust the compensation.

A compensated resonance relay of this sort will work with certainty at 5 volts at 200 frequency and will take no notice of 100 volts at 50 frequency.

In order to test the completeness of the compensation the supply voltage was gradually increased, keeping the frequency constant at 50, when the armature was found to become unstable at about 240 volts. This relay would therefore operate when it should not if the voltage of supply were allowed to rise from 100 to somewhat over 200 volts. As it is not probable that any supply station would allow its voltage to rise 100 per cent above the normal, there is no fear of failure from this cause.

On the other hand, the force acting on the armature of the relay varies approximately as the square of the superposed voltage, so that if instead of superposing 5 volts a smaller value were superposed, the relay might fail to attract its armature. In the relays so far constructed this failure takes place when the superposed voltage is reduced to about 3·5 volts.

If the superposed voltage be maintained at its normal value of 5 volts a variation of some 5 per cent can be made either way in the frequency before the relay fails to act.

For the purpose of calculation we may consider the two currents—the one produced by the supply voltage and the other by the ripple—as acting independently, and it may be of interest to give the data of the above-mentioned relays for the two frequencies.

The relays are adjusted for use on a 100-volt 50-frequency supply. The capacity of the condenser K, Fig. 4, is 2 mfd. and the self-induction of the relay winding L, 0·3 henry. The compensating winding L_1 is wound closely on top of L, and has approximately the same number of turns. The choke coil L_2 is adjusted until the force acting on the armature produced by the 50-frequency current is a minimum. Under these conditions there flow through K and L, and also through L_2 and L_1, two currents, each approximately 0·06 ampere, and having a phase difference of somewhat less than 180°. The current taken from the mains is the vector sum of these two currents and only amounts to 0·02 ampere. The true power taken at 50 frequency and 100 volts is 0·7 watt.

Consider next the data at 200 frequency and 5 volts. The currents through the two circuits are no longer equal; that through K and L being 0·15 ampere and that through L_2 and L_1 0·01 ampere. The current through the relay winding is nearly in phase with the applied potential difference between the mains, whilst the current through the compensating winding L_1 lags about 90°. Hence the ampere-turns on the two windings of the relay are nowhere nearly equal, and consequently there is a force acting on the armature. The actual power taken at 200 frequency and 5 volts is about 0·7 watt.

The above figures are given as an example. It is of course quite easy to modify them by slight alterations in the windings of the coil.

It must be realized that the two windings on the relay coil are both arranged so that the current would flow round the core in the same direction if a continuous current could be passed through the two circuits as connected up, that is to say, if the condenser were short-circuited. This means that the two windings can really be converted into one, and the arrangement illustrated in Fig. 5 can be used.

The arrangement in Fig. 5, although electrically identical with that in Fig. 4, may be looked upon as functioning in a different way. Due to the pressure of supply a certain current flows at the lower frequency through the condenser K. We may supply the greater part of this current without taking it from the mains by placing a choking coil in parallel, and adjusting the choke coil so that its self-induction

and the capacity of the condenser are practically in resonance for the supply frequency.*

In this case the only current taken from the supply mains is the small current to supply the losses, and this small current is the only current, due to the supply frequency, which flows through the relay coil L. At the high frequency of the superposed current, K and L_2 are no longer in resonance, and consequently a considerable current will flow through K and L, which may be further enhanced by making K and L also in resonance.

In order to superpose the ripple on the supply, whether direct-current or alternating, the simplest way is to put a small high-frequency alternator in series with the main generators. Where high voltages are employed, and also in the case of direct-current supply, the secondary of a transformer can be placed in series with the main

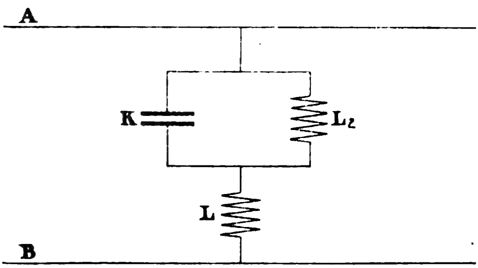

FIG. 5.

generators and the primary be connected to the high-frequency alternator.

Several relays adjusted for different frequencies can be connected to the same supply and be brought into action by varying the speed of the high-frequency alternator.

It is evident that such a relay gives us at once a control over the whole of the distributing system which we have never possessed before; and many ways in which it can be of the greatest assistance will readily occur to station engineers and those responsible for the "lay-out" of electric supply systems.

By inserting a relay in the bases of the lamp-posts, street lighting can be switched on or off from the station either as a whole or in sections as may be desired.

Arc lamps or high-power lamps can be switched off at any desired time and smaller lamps substituted for them.

Transformers in sub-stations can be cut in and out exactly as required. Effective control can be maintained over the main switch

* The two ways of looking at the matter which we have given above are not strictly accurate, as there are slight reactions between the compensating circuit and the other circuit which can be, however, easily allowed for in practice.

on the premises of consumers who are taking current at special rates during restricted hours.

In return for a small payment shopkeepers can have lights burning for window display or for advertising purposes till any agreed hour, when they can be switched off from the station by means of a relay fixed on the premises.

In many residential districts public clocks are either absent or so unreliable that a ready means of setting the domestic clocks would be very desirable; and there are a number of people who would be very willing to pay a small sum per annum to have installed on their premises a relay which would be actuated from the station, and give an audible signal daily at, say, 8 a.m. Greenwich mean time.

The compensated resonance relay should be of considerable assistance in electric track signalling. As is known, the track rails of an electric railway often carry both the return of the main driving current, either alternating or direct, and also the small track signalling current, and there is a danger of the track signals being operated by stray currents from the main operating current. The resonance relay would be absolutely unaffected by any current except one of the periodicity of the signalling current.

The increasing use of electricity for various domestic purposes other than lighting has increased the need for some simple system of charging. The two-rate, or, better still, the varying-rate system of charging has much to recommend it, perhaps the principal advantage being that it is based on a principle that all consumers understand.

Everybody can understand selling current at one price when the station is busy, *i.e.* at peak load, and at a much lower price at slack times, when otherwise some of the plant would be more or less idle. It does not matter in the least to the station for what purpose the current is used. At present, with the majority of stations the peak load corresponds to the time of maximum lighting load, but in the course of time, as electricity is used more and more for domestic and other purposes, this will be less and less the case. This will emphasize the absurdity of selling current at one price for lighting and at another price for heating or power. The consumer has in most cases already formed this opinion, owing to the complication inseparable from having two different meters and two different systems of wiring for lighting and power purposes.

In some alternating-current stations in residential districts the two-rate system has been introduced as an alternative to a flat rate, and by far the greater number of consumers have elected to take the two-rate system despite the increased meter rental entailed by the necessity of having two separate meters and a clock switch.

In such districts where the plant is always running, one can obviously afford to sell current at a low rate off the hours of peak load, and it is evident that the ability to use lights during the day and after peak load at night at a low rate per unit is greatly appreciated both by the buyer and seller. The consumers are no longer so sparing of light

in passages and bedrooms, shopkeepers turn it on in the afternoon if it is at all dark, and many keep it on all day where they find it improves the appearance of their goods.

The chief advantage, however, is in regard to other domestic uses of electricity. If consumers are advised to have all plugs wired with not less than 7/20 S.W.G., the system makes it possible to attach to any plug not merely a portable lamp but any other piece of apparatus desired. From time to time the station can offer on loan various useful electric appliances which can immediately be put to work wherever current is available. No fresh wiring has to be put in, no special maximum demands or rates arranged with the station, and a profitable load can be quickly built up with the minimum of inconvenience and trouble to all concerned.

It will be seen that the "two-rate system" presents many advantages, but the necessity for constantly altering the setting of the clocks and the winding and keeping them in order is a serious drawback, and undoubtedly deters many engineers from adopting the system, especially when ordinary meters are in use.

Where two-rate meters are already installed, a simple relay as described above (to which the name of "Handyell" has been given) can take the place of the clock switch, thus effecting a considerable saving in first cost and in cost of upkeep. It has, moreover, the advantage that the time of operation can, if desired, be adjusted each day to suit the variation in the time of peak load.

This dispenses with the clock, but it would be a further great advantage if the special two-rate or double meters could be also eliminated, and a method devised applicable to any existing system of mains and meters.

The essence of the two-rate system of charging is that the rate for current is, say, 7d. or 6d. per unit, but that during certain times of the day one is willing to sell current at a much lower rate. As a matter of fact one might just as well say to a consumer, "Our price per unit irrespective of the time of day is 7d. per unit; but at times other than of peak load we are prepared to let a considerable proportion of the units pass through the meter without being registered."

If half of the units at such times were not registered it would be equivalent to a charge of 3½d. per unit consumed; if three-quarters were not registered, 1¾d., and so on, depending on the proportion which the units registered bore to those not registered.

The above system can easily be put into effect: A relay is inserted alongside an ordinary meter, or in some cases is combined with the meter, which either breaks the shunt circuit, if a watt-hour meter, or short-circuits the meter entirely, if an ampere-hour meter, whenever a superimposed current of a particular frequency is sent over the system.

In this case the high-frequency alternator which produces the ripple is kept running continuously except during the hours of peak load, when all units are charged for, its field circuit being made and broken periodically by means of an automatic mechanism described below.

Such a method of charging is exceedingly simple and has the advantage that it complies in all respects with Section 19 of the Electric Lighting Act of 1882, in that every one gets his supply on exactly the same terms.

It is not, of course, necessary that there should only be two rates of charge—one on peak load and one off. The charge can be graduated if desired in any manner by varying the number of seconds during which the meter is in and out of operation.

It may be objected that the consumer has no record of the exact number of units he has used. The reply to this is that an inspection of the meter will show him at any moment whether it is registering or not, and if that does not satisfy him he can for a small payment (not as much as is charged now for a two-rate meter) have a second meter on his premises which is not affected by the relay and will thus register the total number of units consumed.

After all, what the average consumer wants to know is what is the total of his bill. If this is satisfactory he does not much mind how it is made up.

It may be of interest to describe a station equipment for an alternating-current generating station having a maximum winter peak load of 500 kw., and 2,000 volts primary, 100 volts secondary pressure; and we shall assume that such a station, if fully equipped with the resonance relay system, would have about 1,000 relays installed. The actual power required to operate the relays would therefore be approximately $1000 \times 0{\cdot}7$ watt $= 0{\cdot}7$ kw. It must be remembered that the consumers' lamps, motors, etc., are also in parallel with the relays, and are therefore taking power out of the superposed ripple; but as the output of the main generators is reduced by a corresponding amount this power is not a loss.

Where the system of periodically breaking the shunt circuit is adopted, the relays will not require to be actuated during the peak load, as this is the time when electrical energy will be charged for at the top rate, and we think it a fair assumption that electricity will not be sold at a low rate when the load on the station exceeds, say, 30 or 40 per cent of the winter peak. In order to allow ample margin we will take the figure at 50 per cent, which means that the relays will be operating when a load of 250 kw. is connected to the above station.

If this load were entirely non-inductive it would correspond to a current of 2,500 amperes at 100 volts, so that the equivalent resistance of the load might be looked upon as being $\frac{100}{2500} = 0{\cdot}04$ ohm. Five volts are required to work the relays. To allow a margin for drop in the mains, etc., we should allow 7 volts (which corresponds to 140 on the primary at the generating station). Seven volts across a resistance of $0{\cdot}04$ ohm would take 174 amperes. This corresponds to $1{\cdot}23$ kw., which, with the $0{\cdot}7$ kw. taken by the relays, makes the total power of the superposed ripple just under 2 kw. This, however, is not the size of the generating plant which must be installed. The alternator

to superpose the ripple can either be connected directly in series in one of the mains on the earthed side, or it can be connected to one of the mains through a transformer. Taking the first case, the full current of the generating station, *i.e.* 125 amperes at 2,000 volts, passes through the small alternator, that is to say, in our present example the alternator must be able to carry 125 amperes and produce an alternating voltage of 140, hence its kilovolt-ampere rating must be 17·5 k.v.a.; but of course the power required to drive it will only be 2 kw., plus the losses.

The arrangement we have so far adopted is to drive the alternator by means of a motor. In the case of the alternating-current supply we find it convenient to use a synchronous motor, as in this case as long as

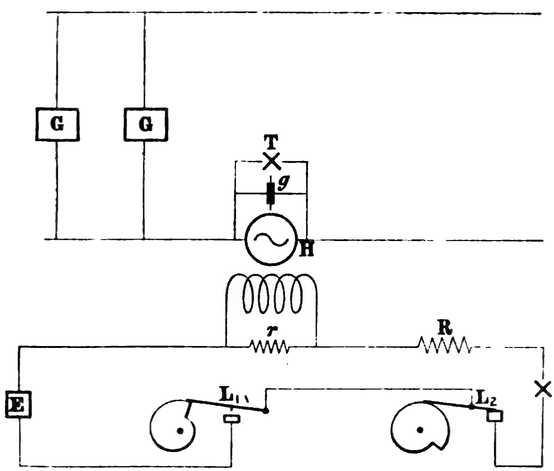

FIG. 6.

the frequency of the main generators is kept adjusted the frequency of the superposed ripple is also correct.

Let us assume that it is required that the maximum price shall be 7d. a unit during the peak-load hours, and that during the rest of the day the price is to be 1d. per unit. This means that the meters must be in operation for one minute during each seven; in other words, the relay must be energized during six minutes and the ripple removed for one minute out of each seven. To do this we attach, to the shaft of the alternator, cams driven through reduction gearing, so that the cams make one revolution in seven minutes. The cams are adjustable, and are so arranged that by means of levers they switch on the field circuit during six minutes and interrupt it for one minute, thus giving the desired effect on the meters.

The complete connections are shown in Fig. 6. In this diagram G, G, are the main generators and H the high-frequency alternator to supply the ripple, the whole of the main current passing through H when in use.

During the peak load the high-frequency alternator can be shut down and short-circuited by the switch T. In a high-pressure supply a very short spark-gap, *g*, is placed in parallel with the small alternator to prevent an interruption of the main current in case of an accident to the small alternator. The field of H is shunted by a resistance, *r*, to prevent any risk of its failure due to the constant switching on and off. In the diagram E represents the station exciter and R the regulating resistance.

The two cams which actuate the levers are mounted on the same shaft and rotate together. When the lever L_1 falls down the step in the cam it closes the field circuit. The field circuit remains closed until the lever L_2 falls on the second cam. While the circuit is open the two levers travel together up the slopes of the cams, and the operation repeats itself. This method of using one cam to make the circuit, and the second to break it, enables a very definite make and break to be obtained, and also the length of the make to be adjusted. In the actual machine the two cams are combined into one, the principle remaining the same.

It may be thought that the relays which are always connected across the mains would, in an alternating-current supply system, consume a considerable amount of power. Taking the above case we have seen (page 246) that the consumption is 0·7 watt per relay, so that the 1,000 relays connected will take approximately 700 watts at 50 frequency. There has to be set against this, however, the fact that when in use the meter shunts are disconnected for a certain number of hours a day. For instance, in the system described above they are disconnected six-sevenths of the time the relays are in action. If we assume that current is supplied cheaply for 20 hours out of the 24, this means that for $\frac{6}{7} \times \frac{20}{24} = \frac{5}{7}$ths of the day there are no meter shunt losses. If the shunt losses of the meter are $1\frac{1}{2}$ watts, which is a fairly average value, then the 1,000 meters operated by the relays would ordinarily take 1,500 watts, and this loss is saved for five-sevenths of the day. Considering a complete day of 24 hours, the meter shunt losses would be 36 kw.-hours. With the relays installed, the meter losses will be reduced to 10·3 kw.-hours, but there must be added to this the 17 kw.-hours taken by the relays, making a total of 27·3 kw.-hours, as against 36 kw.-hours *per diem* for the meters alone, a slight economy in favour of the relay system.

In direct-current systems the comparison is more favourable to the relays, as they consume practically no energy when a steady difference of potential is applied to them. In the above comparisons the power taken at the 200 frequency by the relays is not included; it amounts, as shown on page 246, to about 0·7 watt per relay whilst the ripple is on.

When the relays are only required to operate at infrequent intervals, such as for switching on and off public lamps, the power consumed by them can obviously be considerably increased without increasing the size of the high-frequency generator, which has then only to work for a few seconds at a time.

For ordinary incandescent street lighting a relay is fixed in the base

of the lamp-post, operating a switch by a step-by-step movement. On switching on the high-frequency current the armature of the relay is attracted and then released, causing the switch to be released; when next the current is switched on, the armature is again attracted and released, causing the switch to open.

In the case of the larger switches for motors, transformers, etc., the relay closes or opens the circuit of a local solenoid operated direct off the mains.

An attempt has been made to indicate some of the ways in which a distance-controlled relay can be constructed and be usefully employed, and the authors venture to hope that its advent will open up a new field and give the supply engineer one more tool with which to attack the many problems that lie before him in the future.

NOTES ON THE TESTING OF EBONITE FOR ELECTRICAL PURPOSES.*

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(From the National Physical Laboratory.)

(*Paper received 3rd September*, 1912.)

The investigation described here has been made with a view to defining physical tests which will differentiate highest grade ebonite from those kinds which are made from the poorer qualities of rubber, or which are adulterated with the various "pigments" commonly used in practice. It is known to be well-nigh impossible to predict with certainty from a chemical analysis what quality of rubber has been used in the manufacture of this material. Nevertheless, when ebonite is required to withstand a high electric stress it is of real advantage to have material which is manufactured from pure Para rubber and sulphur only; for an examination of the results given here will show that no other ebonite equals this in its electric strength.

One of the first difficulties met with is the exceedingly high electric strength of the material. Most ebonite sold in sheet form has a strength of approximately 80,000 volts per millimetre. The ordinary purchaser of ebonite is not in a position to carry out tests of this nature, and although breakdown is the only entirely satisfactory one, the authors find that other tests of a simple nature can be applied, which will at once show up materials containing certain classes of adulterants.

The following are perhaps the principal properties sought for in ebonite used for electrical purposes:—

1. Immunity from the action of light in deteriorating the surface of the material, resulting in bad surface insulation.
2. High electric strength.
3. Mechanical toughness and the absence of any tendency to yield under pressure or at temperatures above that of the air.

1. *Action of Light.*—Unfortunately, as regards the first of these, the authors have not found, among the samples tested, any ebonite which

* Tests made at the request of the Admiralty for the purpose of drawing up a specification for ebonite used in the Navy.

can be said to be superior to the others from the point of view of surface deterioration, and they have no suggestion to make on the point as to how this trouble can be obviated in the manufacture of ebonite.

Specimens of ebonite of all the kinds, pure and adulterated, dealt with in this paper have been under observation for upwards of three years, but no one type can be said to be appreciably better than another in this respect. For the purposes of these observations, three discs, 4 in. diameter, were taken from each type of ebonite. One was left with the surface in the condition in which it left the factory, a second had the surface removed with a sharp tool and was then left untouched, and the third had the surface removed and was repolished. All these samples were laid out in a case with a glass window, and were exposed to the ordinary north light of the Electrotechnics building of the National Physical Laboratory.* They have had no direct sunlight on them. After a few months the characteristic formation appeared on them all in the form of small globules of acid. These were most noticeable when the humidity condition of the air approached saturation. Considerable discoloration also took place, which was greatly intensified in some of the samples after washing them with soap and water. The surfaces were thoroughly cleaned from time to time with soap and water in the hope that eventually all the sulphur in the surface layers of the material to which this effect is ascribed would be used up; and that, although perhaps discoloured, the ebonite would become aged and be free from the defect which spoils its surface insulation. Up to the present, however, there are no signs of betterment, and where the illumination is strong the only plan seems to be to protect the material altogether from light, or at least from the more actinic rays.

The tests specified as the result of the investigation are :—

(i) Electric strength.
(ii) Yield.
(iii) Specific gravity.

It must be pointed out that the test for specific gravity is chiefly of importance in helping to prove the quality of the material, as it serves to eliminate many amorphous ingredients which the yield test will not detect.

When high electric strength is not a matter of vital importance (ii) and (iii) will give a good deal of information on the question of quality, although test (i) is the only final one to prove if the material is of the highest grade. The application of test No. (i) is of vital importance, however, when the highest electric strength is required; and when Para rubber has been specified in manufacture, it is the only test to prove conclusively that it has been used.

A further factor should be noted at the outset. From the point of view of electric strength ebonite must not be regarded in all cases as a homogeneous substance. Sheet ebonite, for instance, is built up of a number of thin sheets calendered together when in the condition of dough. Although the process of curing follows this and the junction

of these sheets cannot be detected in any way, the fusion being apparently complete, nevertheless the electric strength in the direction of the planes of the sheets may be less than that through the sheet from surface to surface. The same phenomenon is observable with the larger diameter rod ebonite. This is made by rolling sheets of ebonite dough into cylinders, compressing and curing afterwards. No sign of formation can be detected when the material is examined in section, but the electric strength of rod made in this way is relatively low when tested along the axis. On the other hand, if an attempt is made to squirt ebonite rod, say of 2 in. diameter, the centre of the rod

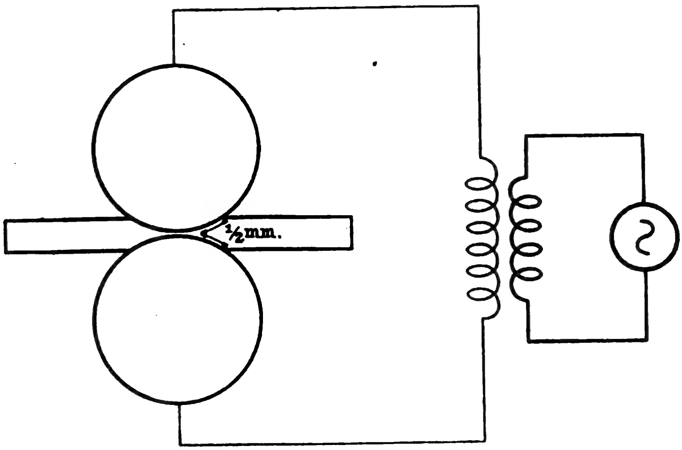

FIG. 1.—Arrangement of Specimen for Breakdown Test.

is generally to some extent spongy, and has in consequence a low electric strength.

PRELIMINARY EXPERIMENTS.

Preliminary tests were first made on one or two samples in order to establish a basis on which the more elaborate investigation should be conducted. It is desirable, whenever possible, to obtain a true measure of the electric strength of an insulating substance, rather than to introduce arbitrary factors which make the value of its electric strength depend on the shape of the electrodes or other features in the test conditions. Experiments were therefore made with a view to obtaining an even voltage gradient in the portion of the material which is tested for breakdown. Flat electrodes, even with well-rounded edges, placed on opposite sides of a sheet will not answer the purpose, since the material in the neighbourhood of the edges must always be subject to a greater stress than that which is near the centre. To meet the latter difficulty means were devised for machining the ebonite so as partially to embed in it metal spheres of 2 in. diameter.

The spherical recesses were made on opposite sides of the sheet to be tested, so that the spheres, when in position, were separated at one point by about 0·5 mm. (see Fig. 1).

When high voltage is put across spheres of this size, separated by a relatively small distance, the voltage gradient in the medium between them is sufficiently even * for the electric strength to be calculated as the voltage per millimetre of distance between the spheres. A method of machining the samples was eventually found which subjected the material to a minimum of mechanical stress, and left a good surface on the ebonite. All breakdown tests were therefore made under these conditions, which ensures strict comparability of the results.

The difficulty of testing the electric strength of ebonite is enhanced by the very high pressures which are necessary to puncture small thicknesses—the electric strength of the better qualities being over 100,000 volts per millimetre. Pressures of this order will spark over very large surfaces, and it would be necessary if the tests be made in air to use a sheet of several square feet area for a single puncture.

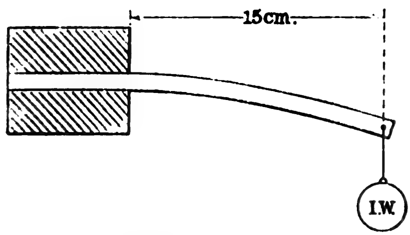

FIG. 2.—Arrangement of Specimen for Yield Test.

On the score of expense this would be out of the question, and some other medium had to be tried in which to make the breakdown tests.

It was found that specimens as small as 4 in. diameter could be tested for breakdown by immersing them completely in oil. Tests were made to determine whether the thin film of oil between the electrodes and the ebonite had any effect on the value of the breakdown voltage found in this way. Punctures were made on large sheets of ebonite in air and on small specimens cut from the same sheets under oil. No difference could be detected in the figures obtained in the two media—a result which would be expected from a comparison of the relative electric strength of oil and ebonite.

As regards the yield test, a convenient arrangement was secured by using a strip of the material, 25 mm. wide and about 10 mm. deep, as a cantilever with a 1-lb. weight fixed at 15 cm. from the support. The specimen was subjected to this bending moment at different temperatures and the permanent deflection of the end of the cantilever after two hours was taken as a measure of the yield of the material (see Fig. 2).

* A. Russell, "The Dielectric Strength of Insulating Materials and the Grading of Cables," *Journal of the Institution of Electrical Engineers*, vol. 40, p. 6, 1907.

EBONITE USED IN THE INVESTIGATION.

Most of the ebonite tested was made up specially, and was given for the purpose by the India Rubber, Gutta Percha, and Telegraph

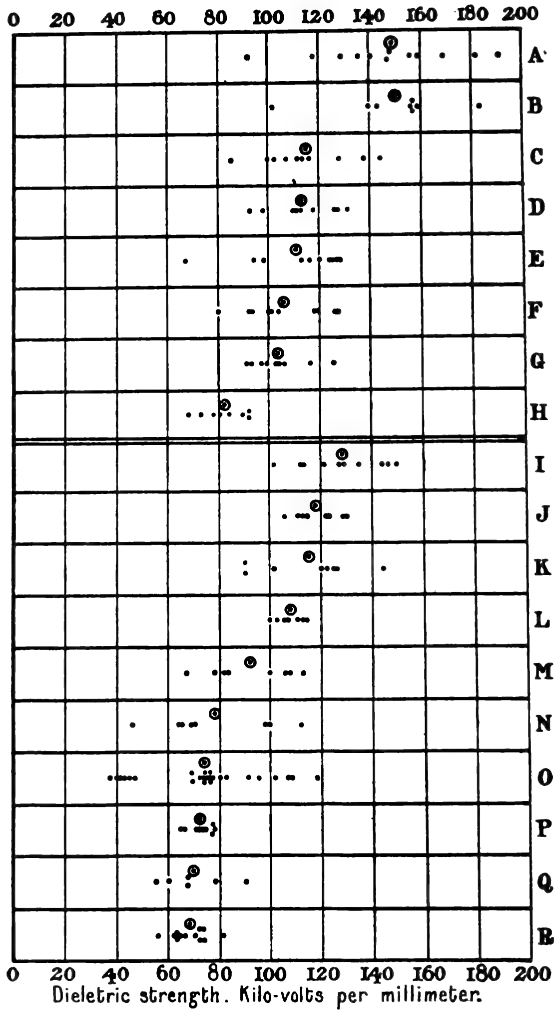

FIG. 3.—Results of Breakdown Tests on the Various Samples of Ebonite.

Works Co., Silvertown. It was decided to make up a series of ebonites containing representative adulterants, and another series containing representative rubbers. The number of possible adul-

terants is almost limitless; but seven representative ones were selected in order, as far as possible, to limit the amount of material to be experimented upon. Except for quality **A**, it must be understood that these mixtures do not represent the stock grades manufactured by this firm, but the mixtures were designed to show up the effect of the different fillings on the electric strength and mechanical stability of ebonite.

The various mixtures tested are scheduled in the table. It will be seen from this that some of the adulterants are of a viscous description such as wax and pitch, whilst others are of a powdery nature, such as zinc oxide and French chalk. As a general rule 15 per cent of the adulterant was added. The constitution of two of the mixtures is unknown, viz., O, which is known as iron ebonite and is manufactured abroad, and R, which was material obtained on the market from an unknown origin. One sample of unadulterated ebonite, B, was made up, using a different procedure in curing, but its quality did not appear to be affected. All the mixtures were made up with 35 per cent of sulphur and were cured in a manner which, in the opinion of the Silvertown Company, would produce material of the highest grade.

RESULTS OF THE TESTS.

Breakdown.—The results of the breakdown tests are shown graphically in Fig. 3, and are given in column 4 of the table.

Results of Tests on Various Samples of Ebonite.

	Mixture.		Electric Strength, Volts per Millimetre (R.M.S.).	Yield Temperature.	Specific Gravity.
	Rubber.	Adulterant.			
1.	2.	3.	4.	5.	6.
A	Para (Mix. 1)	None	148,000	72	1·201
B	,, (Mix. 2)	,,	150,000	76	—
C	Plantation	,,	115,000	77	1·198
D	Borneo (*a*)	,,	113,000	71	1·199
E	Manaos	,,	111,000	81	1·206
F	Sierra Leone	,,	106,000	61	1·213
G	Assare	,,	104,000	72	1·253
H	Borneo (*b*)	,,	82,000	76	1·212
I	Para	French chalk	128,000	77	1·298
J	,,	Soft palm pitch	118,000	61	1·185
K	,,	Waste soft grade	115,000	72	1·192
L	,,	Hard palm pitch	108,000	75	1·224
M	,,	Waste hard grade	92,000	75	1·199
N	,,	Caramba wax	78,000	63	1·171
O	Unknown	Unknown	74,000	51	1·184
P	Para	Vulcanized oil	72,000	55	1·187
Q	,,	Zinc oxide	69,000	81	1·335
R	Unknown	Unknown	68,000	56	1·469

About ten specimens from each sample of ebonite were tested for electric strength in the manner illustrated in Fig. 1. Each test is represented on the diagram by a black dot, and the mean of the ten punctures on any one type of ebonite is indicated by a larger circle. The scale of electric strength is from left to right, and the tests on any one type of ebonite are plotted between the thick horizontal lines. The breakdown tests were made by running up the voltage as evenly as possible, and at such a rate that the pressure at which the specimen would be expected to break down would be reached after an interval of one minute from the first application of the voltage.

An inspection of the results shown in Fig. 3 indicates that the electric strength of any given type of ebonite is a fairly definite quantity. The tests on the different samples of a type certainly do not vary amongst themselves more than is usual in the testing of dielectrics. In a few instances a breakdown point will fall rather wide of the mean, but an examination of most of the cases will show that if two specimens from a type are prepared and tested it is probable that the mean electric strength of these two will not differ from the true mean value by more than 10 per cent. In the actual specification which has been drawn up on the result of these tests, a minimum limit of electric strength has been fixed; and of any two specimens selected for test, at least one of them must hold up to the prescribed figure.

The following points may be noticed in connection with these diagrams:—

1. Samples A and B, which were made from the best Para rubber with 35 per cent of sulphur, have an electric strength of over 140,000 volts per millimetre. This is a considerably higher electric strength than that possessed by any of the other samples, whether adulterated or not.

2. The samples made from other rubbers than Para all had an electric strength in the neighbourhood of 110,000 volts per millimetre. It is noteworthy that the plantation Para used in these particular tests was not so good as the Brazilian Para, but the authors are not able to say if this result would be confirmed in the case of all plantation rubbers.*

3. The adulterated samples were all made up with the best Para, but in all cases the effect of the adulterant has been to lower the electric strength and in some cases to a very marked extent. One of the commonest adulterants is vulcanized oil, and it will be seen that the electric strength of this sample is one of the lowest. The only adulterant which did not greatly diminish the electric strength was French chalk, said to be powdered steatite. This material, however, gives the ebonite a spotty appearance and prevents it taking the characteristic high black polish of pure ebonite. It rather improved the material from the point of view of yield, and for electrical purposes would be the least objectionable of the adulterants tried.

In view of the foregoing considerations, it is suggested that ebonite

* This plantation rubber was obtained in 1909.

purporting to be of the highest grade shall have an electric strength not less than 125,000 *volts per millimetre.*

YIELD.

The method of applying the yield test shown in Fig. 2 has already been described. A yield curve has been drawn in Fig. 4 for each mixture of ebonite.

About ten specimens were used for obtaining the figures from which any one curve is plotted. Specimens of each type were heated in an oven at different temperatures and the initial rate of yielding determined. This is expressed as the deflection in two hours at that

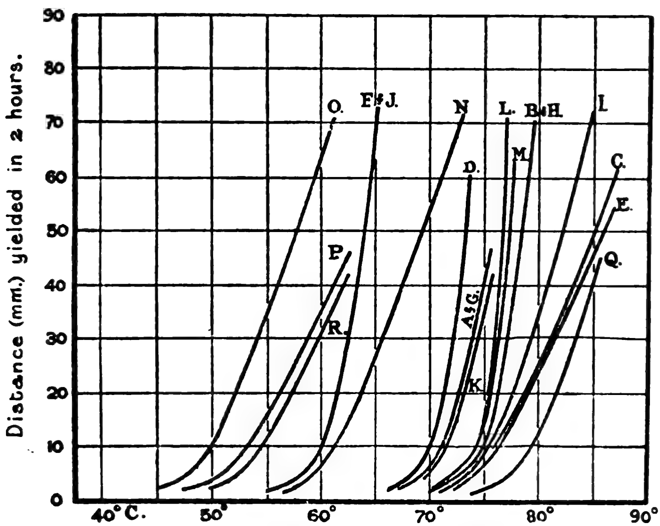

FIG. 4.—Results of Yield Tests on Different Samples of Ebonite.

temperature. Thus a curve such as is illustrated in Fig. 4 can be plotted for each type of ebonite connecting the rate of yielding with temperature.

The point which is brought out most clearly by these curves is that as any given type of ebonite is heated up under a small bending moment, a temperature is reached at which the material begins to yield rapidly under the stress. Such yielding is in the nature of a permanent set, and there is no recovery on cooling or on removal of the stress. The fact that the point of yield is such a sharp one makes the test within reasonable limits almost independent of the precise dimensions of the specimen and of the weight applied. The only really accurate measurement must be that of the temperature of the specimen in the oven, since one or two degrees may make all the difference in the neighbourhood of the yield point.

It must be emphasized that the exact shape of these curves must not be considered as being too rigid. Samples cannot always be relied upon to behave quite consistently, and although the yield curves in Fig. 4 have been drawn to an open temperature scale, to avoid crowding of the curves, the small differences indicated between some of them do not necessarily represent differences which will be always found between the types. Some margin must therefore be allowed in specifying the conditions for a yield test with which ebonite must comply, otherwise it would be necessary to test a good number of samples of a type, in order to obtain a mean which would really represent the average behaviour of the material in question.

It will be seen that a temperature of 70° C. divides off broadly what may be considered the more stable specimens from those which yield more rapidly. Amongst the latter are the two ebonites with unknown constituents and those adulterated respectively with caramba wax, soft palm pitch, and vulcanized oil. Sample F had no adulterant, but was made with a rubber from Sierra Leone. The ebonites which yield at points above the 70° line are those without adulterants, and also those whose adulterants are of a granular description, such as French chalk and zinc oxide. It is quite clear that the yield test will not differentiate between pure ebonite and ebonite adulterated with these granular constituents, and some additional tests must be imposed for this purpose. As far, however, as the yield is concerned it is proposed that ebonite purporting to be high-grade material *shall not yield more than 20 mm. in two hours in an oven maintained at 70° C. when tested under the conditions outlined above.*

Specific Gravity.

The specific gravity test is a useful one taken in conjunction with yield, since by means of it material with an excess of sulphur is eliminated, and whatever adulterant it is proposed to add must have a specific gravity less than 1·2.

The specific gravities of the various samples dealt with in this investigation are scheduled in the table, and it will be seen that the unadulterated Para ebonite has a specific gravity of less than 1·21. It is suggested, therefore, that this shall be imposed as the upper limit for specific gravity.

Discussion on the Test Results.

It now remains to examine these results in order to see what test conditions should be imposed in order to eliminate the undesirable grades of ebonite, keeping in mind that the fewer and simpler the tests in a specification the better and more useful it will be.

An examination of the table will show that if ebonite is specified to pass the specific gravity test of 1·21, and also to comply with a yield test at 70° C., all the worst types of those tested are at once eliminated. The only ones left in are :—

		Adulterant.
A and B	Para rubber and sulphur... ...	None
C	Plantation rubber and sulphur ...	"
D	Borneo rubber and sulphur ...	"
E	Manaos rubber and sulphur ...	"
K	Para rubber and sulphur... ...	Waste soft grade

It is doubtful whether the various samples of Borneo and Manaos rubbers could be relied on to be sufficiently uniform in quality always to pass these tests satisfactorily. On the other hand, some samples made of the other rubbers might, sometimes, be found to hold up to the yield and specific gravity tests, but there must always be an element of uncertainty unless Para rubber is used. The plantation rubber used in the tests is the same as Para except that at present the trees have not all grown to maturity. As time goes on, plantation rubber may become indistinguishable from first-grade Para.

It will thus be seen that these two simple tests for specific gravity and yield serve to eliminate nearly all but the best grade of ebonite, which is made from Para rubber. Such tests are easily applied, provided proper precautions are taken to keep the temperature of the specimen constant in the yield test. In order to make an adulterated ebonite to pass this test, it would be necessary to find a suitable loading material whose specific gravity is less than 1·2, and which would not affect the yielding properties of the ebonite.

As already mentioned, the test for electric strength is the only really final criterion of quality, but it can only be applied where extra high-tension testing facilities are available, and where accurate machining of the specimens can be carried out. This test is always applied at the National Physical Laboratory; but it is thought that buyers of ebonite who merely wish to obtain a general opinion of its quality without the trouble of a dielectric test will find the first two tests of real value. In cases, however, in which the material is to be used for high-tension insulation, the test for electric strength is the only one which should be relied on. Nevertheless, it often happens that the form in which ebonite is bought renders an electric strength test difficult or impossible to carry out. If the material, for instance, is in sheet form, and is less than $\frac{1}{4}$ in. or $\frac{3}{16}$ in. in thickness, or if it is in the form of rod less than 2 in. diameter, the test for electric strength is hardly possible. In such cases one or both of the other tests can be made with useful results. The authors have not obtained actual data for yield tests on very thin sheet ebonite, but by using a small specimen and reduced load there appears to be no reason why this method of test should not apply equally well.

SPECIFICATION.

The following specification is therefore suggested for tests on sheet ebonite purporting to be made from best Para rubber without adulteration :—

Specific Gravity.—To be not more than 1·21.

Yield Test.—A cantilever of the ebonite 25 mm. (1 in.) wide and

10 mm. ($\frac{3}{8}$ in.) thick, supporting a 1-lb. weight 152 mm. (6 in.) from the support shall be placed in an oven maintained at a temperature of 70° C. The ebonite shall not yield so that the point of support of the weight drops through a greater distance than 15 mm. (approximately $\frac{5}{8}$ in.) during two hours at this temperature. The distance dropped shall be measured before and after the test with the weight removed.

Electric Strength.—The sheet ebonite shall be tested for electric strength by embedding metal spheres, 51 mm. (2 in.) diameter, into opposite sides of the material so that the thickness of ebonite between them is about 0·5 mm. An alternating potential difference with an approximately sine wave distribution shall then be put across the spheres, and the voltage gradually raised over a period of about 1 minute until breakdown occurs. The test specimens and spheres may be placed under oil during this test, in which case a specimen 100 mm. (4 in.) diameter will suffice to prevent sparking round the edges.

The spherical recesses in the test specimens must be machined out with a keen-cutting tool, which must be re-ground before the last cut is taken, and the spheres must be a good fit in the recesses. Before the rejection of any material takes place on the ground of insufficient electric strength, at least two specimens shall have been tested for breakdown, and if either of them shows the requisite strength, rejection shall not take place on the ground of this test.

Under these conditions of test the ebonite shall show an electric strength of not less than 125,000 volts per millimetre (R.M.S.).

JOURNAL

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Proceedings of the Five Hundred and Forty-fifth Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 12th December, 1912—Mr. W. DUDDELL, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 28th November, 1912, were taken as read, and confirmed.

The list of candidates for election and transfer approved by the Council for ballot was taken as read, and it was ordered that it should be suspended in the Hall.

Messrs. W. Green and W. R. Hackworth were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the following were declared to have been duly elected :—

ELECTIONS.

As Members.

Sydney Trewhella Allen.
Charles Smith Davidson.

As Associate Members.

William Young Anderson.
Henry Newton Baker.
Richard Baxendale.
Leonard Holdsworth Crowther.
William Joseph Davis.
Thomas Edwin Fletcher.
Alfred George Fox.
George Gordon Fraser.
Thomas Reginald Graty.
Walter Edwin Haley.
Alexander Rodger Harvey.
John William Jackson.
Eric Reginald Jameson.
John Griffith Jones.
Harold F. A. Kinder.
James Mirrey.
Trubie Moore.
William Henry Palmer.
William Redclift Rogers.
Reginald Vivian Weare.
George Norris Wright.
Edgar Robert Wynne.

As Graduates.

Arthur Peachey Aldridge.
Thomas B. Carrigg.
Edgar J. Davidson.
Horace Girdlestone.
Cecil Allan Grut.
George Pell Howkins.
Andrew Armstrong Hurry.
Harman Willoughby Lance.
Alfred Lord Lintott.
Norman Victor Lloyd.
Robert Sell.
Arthur Stubbs.
Albert Victor Summers.
Cyril Leslie William Tucker.
Hubert William Walker.
Frederick William J. West.
Reginald Wood Whittle.
Edward Moses Wolf.

As Students.

George Alexander Allan.
Harold Armstrong.
Alfred Bailey.
William Charles Barnes.
Leslie Wilfred E. Baxendell.
Clarence William G. Booth.
Harold Brooke.
Charles Lionel Bunt.
Fred Colburn.
Arthur Geoffrey Crosby.
Robert Dowson.
Edward Harland Duckworth.
Paul Frederick Elling.
Alan Stewart FitzGerald.
Hugh Leslie Hudson.
Bernard Joseph Kennedy.
Walter Arthur Reeves.
William Kenneth Smith.
Stanley Jackson Snowden.
Desmond Gardiner Trouton.
David Wilkin.
Eric Western Wilson.

TRANSFERS.

From the class of Associate Members to that of Members :—

Andrew William Blake.
Joseph Harding Bolam.
John Keats Catterson-Smith.
Manley Farrer.
Edward Fortescue Long.
Herbert William Miller.
Leonard Murphy.
Edwyn Lonsdale Pope.
James Scott Pringle.
Cyril Hunter Robert Thorn.

From the class of Associates to that of Members :—

Frederick Arthur Knight.

From the class of Associates to that of Associate Members :—

Joseph James Charles Bacon.
Frederick Berry.
John Percival Forster.
Charles Henry Haddrell.
Arnold Strangman Hughes.
Louis F. R. Lewin.
David Stuart Mitchell.
William Mosse Robinson.
Lewis Hey Sharp.
William Walker.

From the class of Students to that of Associate Members :—

Kenneth Byres Findlay.
Arthur Carr Hall.
Percy H. I. Humphreys.
Philip Sydney Edward Jackson.
Robert Gordon Jakeman.
Robert Eric Keelan.
Ernest William Moss.
Bertram Edward Stott.
Frank Stroude.
Noel Bannister Tomlinson.
Guilherme Dumont Villares,

From the class of Students to that of Graduates :—

Walter Jervis Batchelor.	John Herbert Jones.
Harold Cooper.	William Reginald Lewis.
Arthur Douglas.	Thomas Carr Richardson.
William Donald Douglas.	Tom Taylor.
Walter George Ford.	Godfrey P. Willoughby.

The PRESIDENT : The Council has decided to encourage research ; and for this purpose it has appointed a Research Committee. The object of the Research Committee is twofold. It is to co-ordinate research in the electrical industry, and it is to originate research. I want members to realize that we shall be very pleased to receive any suggestions as to researches of general importance to the electrical industry that want encouraging. If the Committee has this information before it, the Committee can act as a general clearance house for research topics, and say what researches want carrying out, and where there are men in universities and in institutions willing to carry out researches.

Then, another matter the Research Committee has in hand is the collection of data relating to the facilities that exist in different parts for carrying out special research. Many of you know that certain universities possess special facilities for carrying out research work. Some of them have specialized in testing iron ; some of them have specialized in testing rubber, and others in different subjects. Arising out of the researches a large amount of literature is sure to have to be looked up. The Committee will take in hand the compilation of bibliographies of any literature relating to the investigations which will be published for the use of members.

That is one side of the Committee's work. The other side is to take in hand origination of research. In the first place it is thought that researches might be originated into the properties of materials used by electrical engineers. There are a great many materials used by electrical engineers of which we do not fully know the properties, such as magnet steel, copper, carbon, paper, rubber, mica, porcelain, and varnishes ; and there are many different data required relating to materials which will be useful to electrical engineers. The Council has decided to-day to appoint this Committee, and I trust that the members of the Institution will help the Committee by sending any suggestions as to the work that they think might be carried out.

A paper by Mr. J. S. Nicholson, B.Sc., Associate Member, and Mr. B. Parker Haigh, B.Sc., entitled "A Single-phase Motor with Pole-changing Windings" (see page 268), was read and discussed, and the meeting adjourned at 9.22 p.m.

A SINGLE-PHASE MOTOR WITH POLE-CHANGING WINDINGS.

By J. S. NICHOLSON, B.Sc., Associate Member, and B. PARKER HAIGH, B.Sc.

(*Paper first received 2nd May, and in final form 20th November*, 1912; *read before the* INSTITUTION *12th December*, 1912, *and before the* SCOTTISH LOCAL SECTION *14th January*, 1913.)

A new type of single-phase commutator motor, recently installed in the James Watt Engineering Laboratories of the University of Glasgow, was brought to the notice of the Institution on the occasion of their recent visit to Scotland, and it is hoped that a description of the machine may be of interest. This motor, which is the invention of one of the authors, may be termed a railway motor, although, as will be understood, the machine tested is of small size and open construction, suitable for experimental work. As the main difference between this type of machine and other commutator motors lies in the use of pole-changing windings, the first part of the paper will deal with the principles involved in the use of such windings in commutator motors.

All single-phase commutator motors have this in common, that the reason for introducing the complications of the commutator, with its attendant brush-gear, is that it is not otherwise practicable to obtain a strong flux "in phase" with the armature current unless the speed of the motor is nearly that corresponding to synchronism.

$$\text{r.p.m.}_{\text{syn.}} = \frac{2 \times 60 \times (f)}{2p} = \frac{120 \times \text{cycles per second}}{\text{number of poles}}.$$

As the mean torque of any single-phase motor is proportional to the cosine of the time-angle between the waves of armature current and flux, it is maximum when these are in phase, and when this is the case the peripheral tractive effort of the motor armature is given, in lb., by the equation—

$$\text{P.T.E.} = \frac{2 \cdot 21}{9 \cdot 81} \times \text{poles} \times \left\{ \begin{matrix} \text{amperes per cm. of} \\ \text{armature periphery} \end{matrix} \right\} \times \frac{\text{flux per pole}}{\sqrt{2} \times 10^6}.$$

When the motor has to work over a wide range of speed, and especially when it has to develop a heavy torque when starting, a commutator is necessary to ensure that the bands of armature currents are kept in suitable positions relatively to the flux, so that the useful flux per pole

shall be as nearly as possible identical with the total flux produced by the field magnets.

This essential principle being common ground for all commutator motors, the new motor shares in many of their characteristic features and limitations. In order to ascertain the proper aim of a pole-changing winding applied to a commutator motor it may therefore be well to make a brief comparison between two ordinary motors of equal size, developing equal torques, but differing in their design in the choice of the number of poles.

Compensated-series Motor.

Two such motors, with compensated-series connections, are shown in Figs. 1 and 2, with 8 and 4 poles respectively. In each of these diagrams FW indicates the field winding producing the flux ϕ_f, which is cut by the band of armature conductors A, and held in place laterally by the band of conductors in the compensating winding CW. The three windings FW, A, and CW, are, of course, connected in series. The two motors may be regarded as similar in respect that, having the same dimensions, the same flux density in the air-gap (B_g), and the same values for the amperes per centimetre of armature periphery (A.S.), they develop equal peripheral tractive efforts. In each case the E.M.F. generated in the armature by its rotation in the flux ϕ_f will be given by the equation—

$$E_g = 2\sqrt{2}\, f_r S_a \phi_f\, 10^{-8} \text{ (effective volts)},$$

where f_r is the "frequency of rotation" (= poles × r.p.m. ÷ 120). As the change in the number of poles will affect the two terms f_r and ϕ_f inversely, it is clear that the number of armature turns in series (S_a) will have to be the same in each of the two motors if the voltages and speeds are to have the same values in each.

The armature E.M.F. (E_g) is, of course, only part of the terminal voltage E_t, which includes in addition the resistance and reactance voltages (= E_s) and also the field voltage E_f, induced in the field windings (F.W.) by the pulsation of the flux ϕ_f.

$$E_f = 4{\cdot}44\, f\, S_f\, \phi_f\, 10^{-8} \text{ (effective volts)}.$$

This E.M.F., being 90° out of phase with the current, must be minimized in order to obtain a high power factor, and it is important to note that its value will not be materially changed by altering the number of poles. The two motors, having the same flux density (B_g), will require the same number of turns per pole, so that the total number of turns in the field-winding will be changed in inverse ratio to the flux per pole.

In order to keep the ratio $E_f : E_g$ as low as possible, it is clearly advantageous in either case to work with a low ratio $S_f : S_a$, *i.e.* with a small air-gap (necessitating the use of a compensating winding), and with a low frequency of supply (f), whose value may be only one half or one-third of that of the frequency of rotation (f_r).

The two compensated-series motors may be compared with one another as regards their commutation, by estimating the voltages induced in the armature turns short-circuited by the brushes—firstly, when starting against a given torque, and secondly when running on full load at equal speeds. When stationary the only E.M.F. tending to cause sparking is that induced in the short-circuited turns, $S_{s.c.}$, by the pulsation of the main flux ϕ_f; and measuring this E.M.F. between the tips of the brushes we obtain the value—

$$\Delta\, e_t = 4{\cdot}44\, f\, S_{s.c.}\, \phi_f\, 10^{-8} \text{ (effective volts).}$$

If the brushes are of the same breadth in the two motors the values of this E.M.F. will be equal when the motors develop the same torque, for the number of turns short-circuited will be twice as great in the 8-pole as in the 4-pole motor, while the value of the flux is reduced in the same ratio. When the motor is running on load the E.M.F. induced in the turns short-circuited by the brush is increased by a second E.M.F. (corresponding to that which appears in direct-current machines), due to reversal of the direction of current in the coils as these pass under the brush. The value of this second E.M.F. is given by the equation—

$$\Delta\, e_c = 8\, S_{s.c.}\, l\, \tau\, f_r\, (\text{A.S.})\, \lambda\, 10^{-8} \text{ volts,}$$

in which τ and l are the pole-pitch and core-length respectively, and λ a constant identical with that used by Arnold and La Cour (λ is equal to the number of interlinkages of stray flux per conductor, per centimetre length of core, per ampere per slot). It is clear that this voltage $\Delta\, e_c$ is twice as great in the 8-pole as in the 4-pole motor, as the number of short-circuited turns, $S_{s.c.}$, is doubled; but for the same reason the density of the commutating flux produced by the compensating winding (C.W.) need only have the same strength in each motor, given by the equation—

$$B_c = 2\,(\text{A. S.}) \times \lambda.$$

Further, if a shunt commutating field is provided, according to the Milch-Richter system, to neutralize $\Delta\, e_t$ at any particular speed, the density of this shunt commutating flux need be only half as great in the 8-pole as in the 4-pole motor.

As regards commutator dimensions, the total brush contact area must be the same in each motor as the total currents are the same. But the 8-pole motor having twice as many brush spindles need have only half as many brushes per spindle, and can therefore be built with a much shorter commutator than the 4-pole motor, provided that the output is in reality limited by the heating of the brushes rather than by that of the commutator surface as a whole. The end connectors of the windings are also shorter in the motor with the greater number of poles, which can therefore be constructed with smaller overall dimensions in the axial direction than the other. The body of iron necessary is also less on account of the reduced flux. The number of

poles is therefore chosen high in series motors, the only limits being general convenience, an increase in the leakage fluxes, and in some cases heating of the commutator surface.

Repulsion Motors.

The same choice would be made in repulsion motors, were it not that other influences come into play. In the repulsion motor the electrical input is supplied to the compensating winding, which acts as the primary of a transformer of which the secondary is the armature winding, short-circuited through its brushes. The magnetic connection between this primary and secondary is the so-called "cross-flux" (ϕ_c), located at right angles to the main flux (ϕ_f), and in quadrature with it in time. The value of the cross flux being given by the equation, $\phi_c = \phi_f(f_r \div f)$, the two fields are equal, and combine to form a uniform rotating field, when the motor runs at a speed approximating to synchronism. The rotary motion of the flux is, of course, a considerable advantage in favour of the repulsion motor, minimizing the iron losses in the armature, and assisting commutation, as the rotating flux induces no E.M.F.'s in the armature coils. In other words, and referring particularly to the armature turns short-circuited by the brushes, the E.M.F. Δe_t induced by the pulsation of the main flux ϕ_f is equal to, and is neutralized by, the E.M.F. generated in these same turns by their motion through the cross field ϕ_c. Over a limited range of speed, a strong cross field may be maintained in quadrature with the main field by impressing an E.M.F. E_B in the circuit of the brushes, such that—

$$E_B = 4{\cdot}44 f S_a q \phi_c \left(1 - \frac{f_r}{f}\right) 10^{-8} \text{ volts.}$$

In this way fair commutation may be obtained at speeds differing widely from the synchronous value, but in general it is desirable to choose the number of poles such that the synchronous speed is close to the normal running speed of the motor.

In making the following comparison between two similar repulsion motors developing equal torques, we shall therefore assume that the 8-pole motor is intended to run at half the normal speed of the 4-pole motor supplied at the same terminal voltage and frequency. As the armature voltage may be chosen independently of the primary voltage of supply, we may assume that the space allocated to the commutator is the same in each machine. Fig. 2, which has already been used to represent a 4-pole compensated-series motor, may now be regarded as the 4-pole repulsion motor, while Fig. 3 represents the 8-pole repulsion motor. As the commutators are of equal length, the normal current per brush spindle may be the same in each case if brushes of equal breadth short-circuiting equal numbers of armature turns are used, and the total current will be doubled in the 8-pole armature. As regards the primary windings, which carry only half as much current in the 8-pole

as in the 4-pole motor, the compensating windings will require twice as many turns in series to give the same value of A.S., while the field windings will require four times as many turns in series in order to generate the same flux density (B_g) in the air-gap. The stator copper

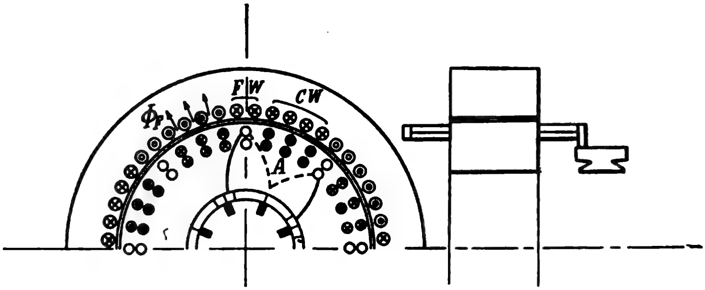

FIG. 1.

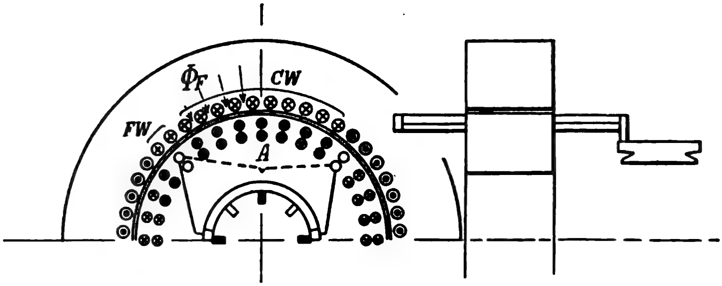

FIG. 2.

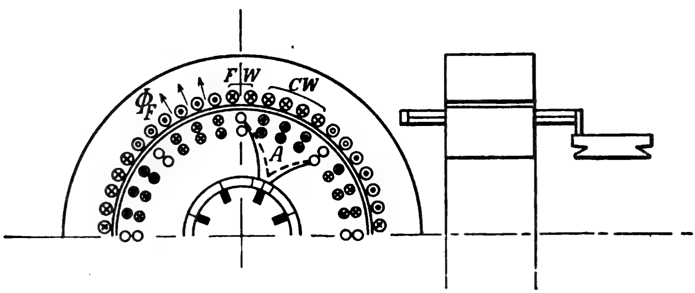

FIG. 3.

losses may be slightly increased, although the end connectors are shorter, and it may be desirable to increase the diameter at the air-gap, still keeping the external diameter of the 8-pole core less than that of the 4-pole machine. As the total losses are approximately equal in the

two motors it is clear that the efficiency of the 8-pole machine, which gives only half the output, will be less than that of the other; this would, of course, be anticipated, for low-speed machinery is in general heavy in proportion to the power developed, and correspondingly inefficient.

To weigh against this disadvantage, the low-speed 8-pole machine offers considerably better conditions as regards commutation at low speeds. When running at normal speed, excellent commutation is obtainable in either machine, but when starting under load the voltage induced in the armature turns short-circuited by the brushes forms a barrier to the increase of the torque beyond a certain limit. As in the case of the series motor, the induced voltage is given by the equation—

$$\Delta e_t = 4{\cdot}44 f \, S_{s.c.} \, \phi_f \, 10^{-8} \text{ volts},$$

and as the number of short-circuited turns is the same in each motor, while the flux is doubled in the 4-pole machine, the value of the induced voltage is twice as great in the 4-pole as in the 8-pole motor when these are developing equal torques. Were it practicable to increase the flux in the 8-pole motor proportionately to the current until the values of Δe_t were the same in each machine, the 8-pole motor would be capable of developing four times as much starting torque as the 4-pole machine, without sparking. If we assume that the current can be temporarily increased 50 per cent without undue overheating, with a corresponding increase of 35 per cent in the flux, then the 8-pole motor will develop more than double the torque of the 4-pole motor with reduced liability to sparking.

As regards the starting current required, it might be anticipated that the torque per kilovolt-ampere would be twice as great in the low-speed as in the high-speed motor, for the motors develop equal torques when running at their respective normal speeds, with currents that do not greatly differ in ratio from 1 to 2. When starting, however, the greater part of the terminal voltage is absorbed in the field windings, which in the low-speed motor have four times as many turns interlinked with half as great a flux as in the other. The voltage required is therefore almost twice as great although the current is only half, so that the two machines make an equal wattless demand on the line when developing equal torques.

For the same reason the power factors are not equal when running at normal speeds; the wattless voltage is twice as great in the low-speed motor as in the other, and the power factor is correspondingly reduced.

So long as the motor is stationary or running at only low speeds no means exist for reducing the wattless voltage, other than the use of a small air-gap. But when the speed rises towards synchronism Winter-Eichberg or La Tour connections may be employed to utilize the opposite wattless E.M.F. generated in armature by its rotation in the cross flux (ϕ_c). This may be done by passing the magnetizing

current through the armature by means of extra brushes on an axis at right angles to that of the "working" brushes (Winter-Eichberg system) or by dividing the working brushes into pairs, short-circuiting part of the armature in the "working" circuit, while the remainder is used for magnetizing (La Tour system). No stationary field winding is required in either case, and when the machine runs at a speed corresponding to the frequency f_r the wattless magnetizing voltage required on the brushes is reduced to the value given by the equation—

$$E_B = 4\cdot 44 f S_a\, q\, \phi_f \left(1 - \frac{f_r^2}{f^2}\right) 10^{-8} \text{ volts},$$

where S_a is the number of armature turns used in series, and q their "breadth coefficient." When running at low speeds the wattless volt-amperes required for magnetizing by means of the armature are as great as when a stationary field winding is used, but at higher speeds, approaching synchronism, the net wattless voltage is quickly reduced and a high power factor may be obtained, even when the iron of the magnetic circuit of the motor is worked at high flux densities.

Comparing the two repulsion motors shown in Figs. 2 and 3, it is clear that the 4-pole machine offers better conditions for introducing the extra brushes required in magnetizing by the armature, as the commutator of the 8-pole machine may be already somewhat crowded. This feature, which renders a high power factor attainable, therefore forms an additional incentive towards the use of the lower number of poles, thus, as we have already seen, giving a high efficiency with good commutation at convenient speeds, higher than those suitable for the motor with the greater number of poles. As the latter motor, nevertheless, gives a greater torque before reaching the limit imposed by commutation at low speeds, it appears that a certain field of utility exists for pole-changing windings capable of combining the characteristics of machines with two numbers of poles in a single repulsion motor. It remains to be seen to what extent a practical pole-changing winding can be arranged to combine these characteristics without undue sacrifice in output or efficiency.

Pole-changing Stator Winding.

In comparison with the three-phase pole-changing windings that are already widely used in induction motors, single-phase pole-changing windings are notably simple, and economical in their use of active material. Coil windings especially (which are not generally effective for three-phase pole-changing) give good results in single-phase stators, while "shortened-step" windings are suitable for armatures.

A typical stator winding for 8 and 4 poles is shown in Figs. 4 and 5, and consists essentially of two sets of coils, I, I and II, II. In Fig. 4 the directions of current in these are such that the winding has 8 poles, the conductors being arranged in eight groups, alternately + and —, occupying in this instance four of the six slots of each

pole-pitch. On reversing the direction of the current in coils II, II, as shown in Fig. 5, the winding gives 4 poles, each having eight active slots arranged in two groups of four, separated by groups of two empty slots. As the presence of these latter inactive slots would be undesirable in a stator winding intended for compensating the ampere-turns of the armature, an extra set of coils, III, III, may be added, so as to complete four uniform groups of conductors while still leaving four groups of slots in the neutral zones unwound. The three sets of coils are preferably connected so that coils I and II are in series when employed for 8 poles, and in parallel with one another (but in series with the extra coils, III) when used for 4 poles. The values of A.S. are then equal for the two numbers of poles when the currents are in the normal ratio of 1 to 2.

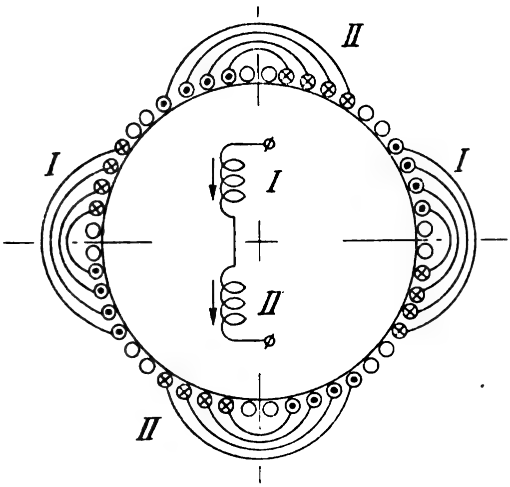

FIG. 4.

Such a winding as the above, although well adapted to act as the "main" stator winding of a repulsion motor with either 8 or 4 poles, leaves no space available for an 8-pole field winding (although a 4-pole field winding might, if so desired, be wound in slots in the neutral zones shown vacant in Fig. 5). By shifting the brushes, when this is permissible, a suitable field may always be obtained in a repulsion motor without a separate field winding; but as a fixed brush position is in many circumstances imperative it is desirable to consider other means of magnetizing. In order to obtain a high power factor when the motor is developing its maximum power, it is preferable to obtain the necessary field ampere-turns for the lower number of poles by passing a magnetizing current through the armature; and as we shall see immediately, this is convenient in practice on account of the loca-

tion of the brushes. When running with the greater number of poles, on the other hand, power factor is of less importance as the power developed at the reduced speed is less; and as it would be inconvenient to add extra brushes for passing a magnetizing current through the armature (on account of the short pole-pitch), it is important to provide an effective 8-pole field winding on the stator.

This is a matter of some difficulty, as such a winding requires the use of all the slots shown vacant in Fig. 4, some of which are already utilized by the extra 4-pole main winding (III, Fig. 5). Fig. 6 shows a winding which meets both requirements, serving not

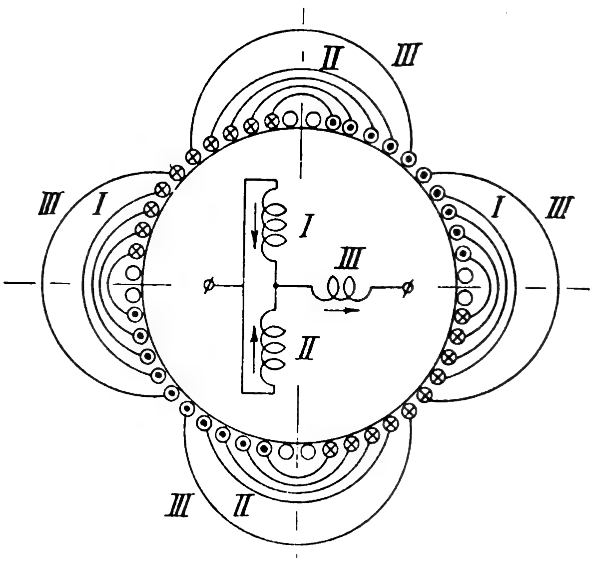

FIG. 5.

only as an 8-pole field winding, but also as an additional 4-pole main winding according to the directions of current in the component coils 1, 2, 3, and 4. When these are coupled in the two pairs, 1, 2, and 3, 4, which may be connected in series, as in Fig. 6, *a*, or singly, or in parallel, an 8-pole field is produced, covering nearly five-sixths of the total pole area at uniform density. By reversing the currents in coils 3 and 4, while those in coils 1 and 2 remain unchanged, as shown in Fig. 6, *b*, the directions of the currents in four of the eight groups of slots are such as to fulfil the purpose of the extra main winding (III, Fig. 5), while the directions of current in the remaining four groups are such as to neutralize one another, leaving these slots inactive so far as external magnetomotive forces are concerned, although subject to copper

losses and magnetic leakage. The use of the extra main winding, or of the reconnected field winding, not only improves the overload capacity of the motor by reducing the stray fluxes, but also increases its output in proportion to the voltage induced in the active conductors of the additional winding. This voltage is 37 per cent of that induced in coils I and II of the main winding when all slots contain the same numbers of conductors and the flux density is assumed to be (as is approximately the case in practice) distributed in a sine wave.

Pole-changing Armature Winding.

Armature windings differ from the so-called "coil windings" that are used for stators in respect that, as the winding is connected to a commutator, it must necessarily be coupled in a closed circuit consist-

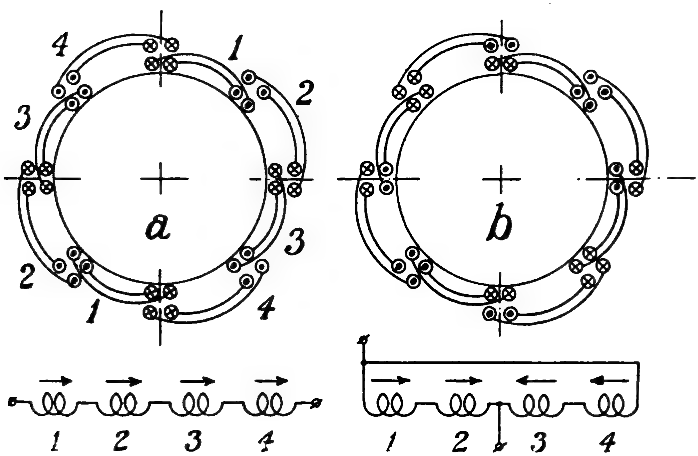

Fig. 6.

ing of a large number of elements. Each element is made up of at least two active conductors embedded in slots which, in a normal armature intended for use with only one number of poles, are separated by a span approximately equal to, or somewhat less than, the pole-pitch. In a pole-changing armature this span is chosen with a value intermediate between the pitches of the two numbers of poles; and, as a rule, the best results are obtained when the span is chosen as the geometrical mean between the two pitches. The number of poles produced at any moment depends upon the connections of the brushes which control the directions of current in the different parts of the armature winding in much the same manner as is effected by the series-parallel connections in the stator winding.

This is shown in the case of an 8- and 4-pole armature in Figs. 7, 8, and 9, in which part of the winding is outlined with lap connections,

having a span of 60°, *i.e.* two-thirds of the longer pole-pitch, or four-thirds of the shorter. When the winding is used for 8 poles, alternate brushes are of opposite polarity; and on passing currents through the winding from each brush to its neighbours, as shown in Fig. 7, eight bands of conductors are formed. Each of these active bands covers two-thirds of the pole-pitch, and is separated from other bands of opposite polarity by shorter inactive zones in which the conductors in each slot carry opposite currents that neutralize one another. It is clear that the number of inactive conductors depends upon the choice of "slot-

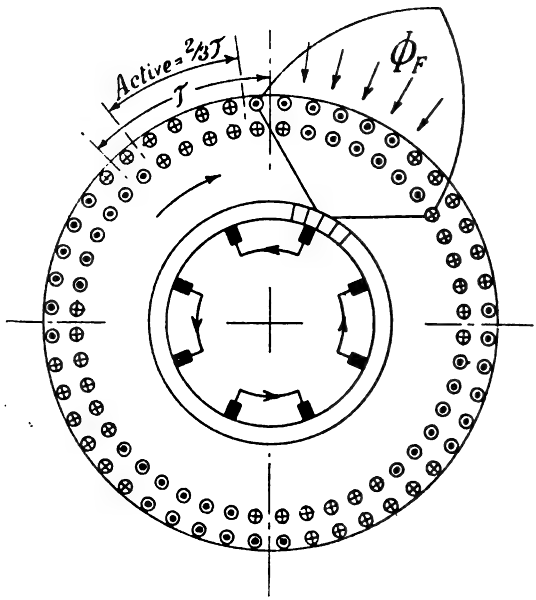

FIG. 7.

step," and would be reduced by using a shorter step; but as the winding is used in conjunction with the 8-pole stator winding of Fig. 4, in which only two-thirds of the total number of slots are allocated to the main winding, little advantage would be gained by further shortening the step, as the reactance of the machine would be increased by the less effective compensation of the two windings.

On changing over from the greater to the lesser number of poles, two methods of connecting up the brushes are available. In the first of these methods, shown in Fig. 8, only half the total number of brushes (viz. those shown black) are used to carry the "working" currents (compensating the ampere-turns of the main stator winding and producing the torque), while the remaining brushes are used to carry magnetizing currents not shown in the diagram. As the step of

the coils is two-thirds of the pole-pitch, the currents are now distributed in four active bands each covering two-thirds of the pole-pitch, and, as before, these bands are separated from one another by shorter inactive bands located in the neutral zones. Comparing this winding with the 4-pole stator winding shown in Fig. 5, it is clear that the two compensate one another fairly well as the active zones cover respectively 67 per cent and 83 per cent of the pole-pitch. As the active zones overlap one another symmetrically when the brushes are placed in the positions shown, but not when the brushes are left in one and the same position for both numbers of poles, this method of pole-changing is less suitable for motors of moderate size, such as are used on multiple unit

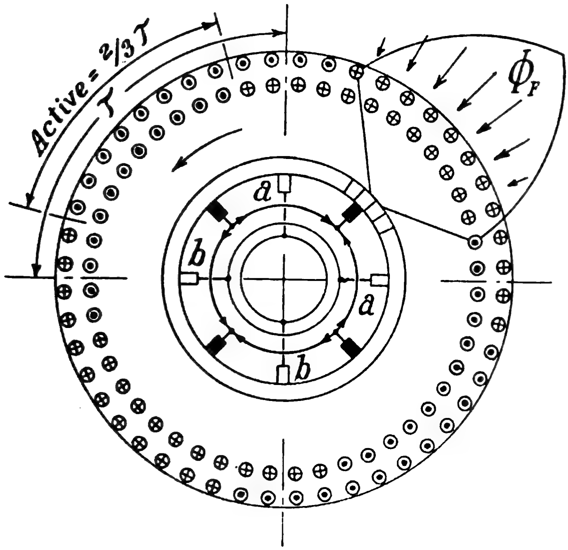

FIG. 8.

vehicles, than for motors of larger size for locomotives, in which service brush-shifting is by no means inconvenient. The connections of the magnetizing brushes and of the change-over switches will be dealt with after the second method of connecting-up the "working" brushes has been described.

In this second method, shown in Fig. 9, the brushes are short-circuited in pairs, and all are used to carry the "working" currents which pass through only half the total number of armature conductors at any one moment. As the distribution of the bands of current is not so uniform as in the previous case, as is clearly seen by comparing Figs. 5 and 9, the reactance of the machine is somewhat increased. The same two diagrams show that the brush positions do not require to be altered when changing the number of poles, for the bands of

current in the armature and stator overlap one another symmetrically when the brushes are in one and the same position for each number of poles.

Each of the arrangements of brushes shown in Figs. 8 and 9 is suitable for leading magnetizing currents into the armature to produce a field with the lower number of poles. Thus in Fig. 8 the brushes *a*, *b*, not used for the working currents, may be supplied from an exciter transformer with series or shunt connections as in a compensated-repulsion motor running without pole-changing connections. Similarly in Fig. 9 the magnetizing current may be supplied to two of the four

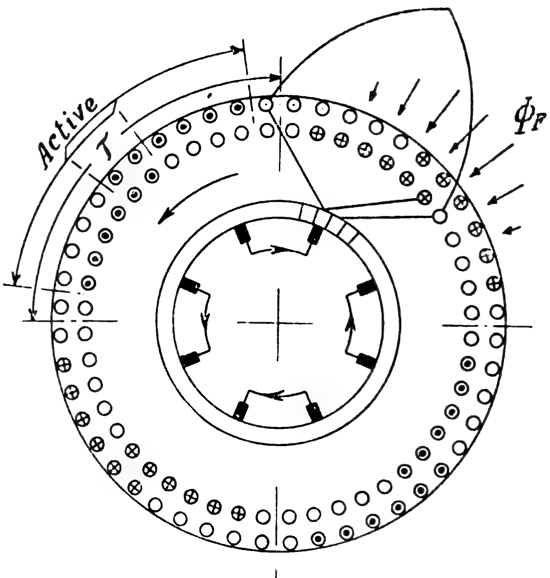

Fig. 9.

pairs of brushes and led away from the other two pairs. The distribution of the field wave differs slightly in the two cases, the magnetizing ampere-turns being distributed in the same manner as the "working" ampere-turns shown in the diagrams, but in either case the distribution is very approximately a sine wave.

It is of interest to note that shortened-step windings may also be used on the stator, but as they are neither so efficient nor so convenient for this purpose as the coil winding that has been described they will not be dealt with here.

Commutation.

In the armature with eight working brushes commutation occurs in two stages, the + current changing to zero under one brush and thence

to – value under the next. This naturally assists commutation, but as only one half of the total number of armature conductors carry load currents at any one moment, a greater current is required to gain the same output, or alternatively, the number of conductors per slot may be increased 40 per cent while the current is kept at the same value. This increase can be allowed without extra losses, as each conductor carries current during only half the revolution; but as the increase in the number of conductors doubles the self-induction, commutation is not eventually different for the armatures with eight and four working brushes respectively, when designed for the same total current.

As the groups of conductors in any one slot do not undergo commutation at the same moment, that part of the total sparking E.M.F. which is ordinarily induced in the coils by mutual induction is absent, and in comparison with a normal armature having a slot-step equal to

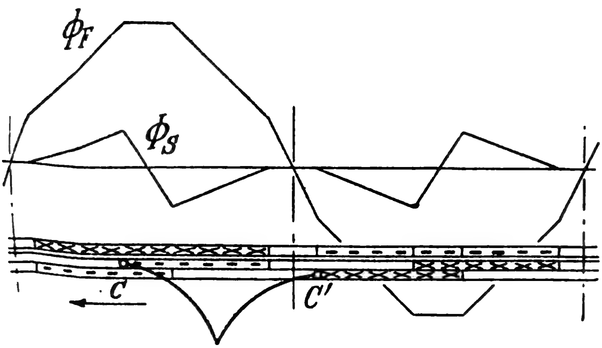

FIG. 10.

the full pole-pitch, commutation on load is correspondingly improved. Since this allows the number of turns per commutator bar to be increased 40 per cent as compared with a normal design, the armature current may be correspondingly reduced and a smaller commutator used. To a great extent, however, commutation on load depends on the stray fluxes produced by local inequalities in the M.M.F.'s of the load currents in the armature and main stator windings. Fig. 10 shows the state of affairs in the case of an 8-brush armature working with a stator having uniform A.S. across five-sixths of the pole-pitch. The flux distribution produced by the magnetizing currents in the armature is shown by the curve ϕ_f, while the density of the stray fluxes is represented by the curve ϕ_s, whose ordinates are reversed so that the area between the curves represents the total flux. The position of one of the coils undergoing commutation is shown by C C′, and it is seen that the conductor C is cutting the stray flux at one of its peaks, and that this peak is in the reverse direction to that which would oppose the E.M.F. induced in the coil by the commutation of its current. The

diagram is drawn for an armature with slot-step equal to two-thirds of the pole-pitch, in which case the effect is considerable when a small air-gap is used. It can be minimized by using a slightly shorter slot-

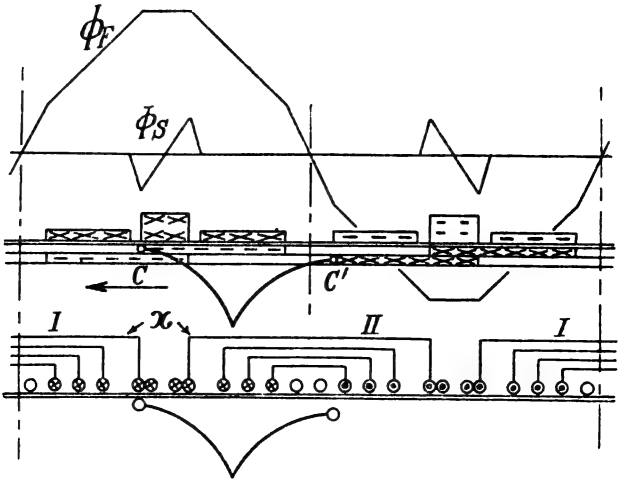

FIG. 11.

step so as to improve the compensation, or alternatively by increasing the value of A.S. for the stator coils in the mid portion of pole-face (belonging to the reconnected field winding, Fig. 6). Fig. 11 indicates

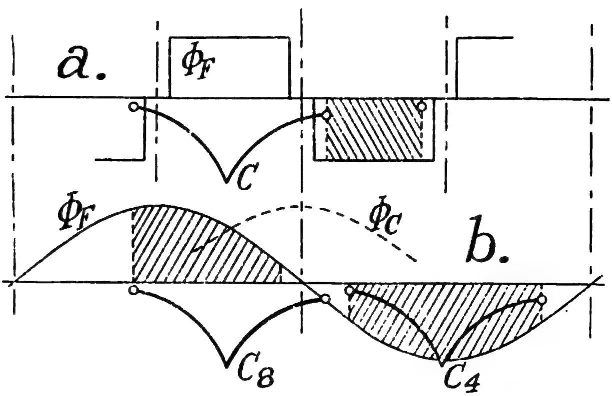

FIG. 12.

how this may be carried out by slightly lengthening the coils X belonging to groups I and II of the main winding, so that they enter wider slots which also contain the field windings. This gives a stray flux of

small magnitude, having peaks in the right direction for assisting commutation, and located so that the coil-step of the armature may be chosen as two-thirds of the pole-pitch, or slightly more or less if so desired. The ordinates are here again reversed so that the resultant field is represented by the ordinates between the curves.

Although the E.M.F.'s induced in the short-circuited conductors by their rotation in fluxes ϕ_f and ϕ_c respectively neutralize the E.M.F.'s induced by the pulsation of these fluxes (in the reverse order) when the motor runs at approximately synchronous speed, nevertheless these E.M.F.'s are considerable at reduced speeds, and differ in the case of the 4- and 8-brush armatures as the fluxes interlinked with the coils at the moment of commutation are different. This is shown in Fig. 12, in which diagram "*a*" represents 8-pole conditions, while diagram "*b*" shows the field wave ϕ_f, and the positions C_8 and C_4 in which the armature turns are short-circuited in the 8- and 4-brush methods respectively. Choosing (for the sake of comparison) the maximum flux density for 8 poles as 83 per cent. of the 4-pole maximum so that the torque with 8 poles shall be equal to that developed by the 4-pole armature with four working brushes when the values of A.S. are the same, we see that the net flux interlinked with the short-circuited coils is twice as great in the latter case. The advantage gained by using this pole-changing armature is therefore the same as in the ideal case in which we saw that the flux interlinked with the armature-turns was 100 per cent greater when the motor was wound for 4 poles than when wound for 8 poles.

Even when using an armature with 8 working brushes carrying the same total current with a shorter commutator the net flux interlinked with each turn is 40 per cent less with the greater number of poles, and such a machine is therefore capable of developing almost twice as great a torque when starting with the greater number of poles as when using the smaller number.

Output.

As regards I^2R losses in the armature copper, the motor with 8 working brushes is quite as efficient as that with only 4 working brushes, in spite of the reduced number of armature conductors which are active at any one moment. This is due to the somewhat higher "breadth coefficient" of the active windings, which are located mainly in the strongest zone of the field. A comparison between the two types is made in the following table, in which a third column is added for the normal repulsion motor (with coil-step equal to the pole-pitch). To enable this machine to run at the same maximum speed it is designed with the same number of poles (4) as the pole-changing motors employ when running at full speed with the lesser number of poles. The numbers of armature conductors are chosen so as to give equal liability to sparking when running at full speed, and in each case it is assumed that the field distribution along the periphery of the armature follows a sine-wave.

COMPARISON OF 4-POLE ARMATURE OUTPUTS.

Type.	Shortened Step.	Shortened Step.	Full Step.
No. of working brushes ...	4	8	4
Diagram	Fig. 8	Fig. 9	(None)
Step of coils...	60°	60°	90°
No. of armature conductors	C	1·41 C	0·71 C
Section of ditto	A	0·71 A	1·41 A
Mean length of conductor	0·87 L	0·87 L	L
Resistance of armature ...	R	$\frac{1}{4} \times 2$R	$1{\cdot}15 \times \frac{1}{2}$ R
Current for equal losses ...	I	I	1·32 I
Number of turns in series...	T	0·71 T	0·71 T
Breadth coefficient ...	$\frac{\sqrt{3}}{\pi} = 0{\cdot}552$	$\frac{2\sqrt{2}}{\pi} \times \cos 30° = 0{\cdot}780$	$\frac{2}{\pi} = 0{\cdot}637$
Induced E.M.F.	1·23 E	1·23 E	E
Output (volt-amperes) ...	0·93 W	0·93 W	W

Although the figures given in the above table may be modified in particular cases owing to small variations in the relative lengths and sectional areas of the armature conductors, yet it is clear that the output of the armature is not greatly reduced by using the shortened-step connections suitable for pole changing. The pole-changing motor is therefore not only capable of developing a greater starting torque, but is equally efficient, and on account of the shorter armature end connectors and commutator is more compact and simpler in construction than the normal design of compensated-repulsion motor.

While the above comparison is applicable in the case of motors of moderate size, suitable for multiple unit vehicles, it does not apply directly to large-sized locomotive motors. The compensated-series motor, working with a rectangular field-wave and designed with a large number of poles, appears to give better results than the repulsion motor with sinusoidal field, in machines of large size. The pole-changing motor is similarly handicapped by its sinusoidal field, and on account of the distribution of its windings the output is not greatly increased when a rectangular field-wave is used. The output is therefore materially less than that of a series motor with the greater number of poles. Nevertheless the pole-changing motor offers some advantages over such a series motor. It is unnecessary to employ shunt commutating poles whose strength must be regulated to suit the speed, or resistance connectors; and as the stator may in some cases be wound for the full line voltage the use of a transformer may be avoided. With suitable connections, the speed may be varied by brush shifting without the use of a variable voltage of supply.

CONTROL CONNECTIONS.

In a motor intended for railway service the details of the control diagrams are clearly of great import, as the expense of upkeep of contactors dealing with heavy currents is considerable. In the space available it is not possible to deal fully with all the points that present themselves in connecting up a pole-changing motor, and it must suffice to indicate the main features of each type. Part of the problem lies in making provision for reversing when running with each number

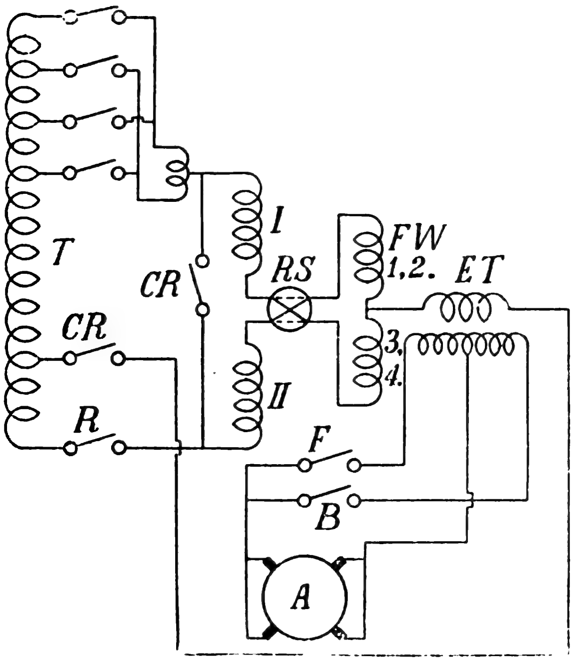

FIG. 13.

of poles; and in viewing the four diagrams that now follow it should be borne in mind that in each case the motor is reversible and controllable over the full range of speed in both directions.

In each of the diagrams the motor is started as a repulsion motor with the greater number of poles, and after running up to half speed is changed over to the lower number of poles, running as a compensated-repulsion motor excited by a magnetising current in the armature.

Fig. 13 represents connections in which no brush-shifting is used, and which are therefore suitable for multiple unit vehicles. Current is supplied at suitable voltage from tappings on the transformer T

to the main stator windings I and II, connected in series with the field-windings (FW) by means of the reversing switch (RS), and is returned to the transformer by the switch R, closed only when the motor has to run as a repulsion motor with the greater number of poles. To change over, the switch R is opened and switches C R closed, so that current now passes through windings I and II in parallel, thence through the reconnected field windings and back through the exciter transformer (ET). The motor is reversed on the high-speed connections by closing switches F or B alternatively. It may be noted that the working currents in the armature circuit do not pass through any switches unless it is desired to impress a voltage on this circuit so that the motor can run on a still wider range of speed.

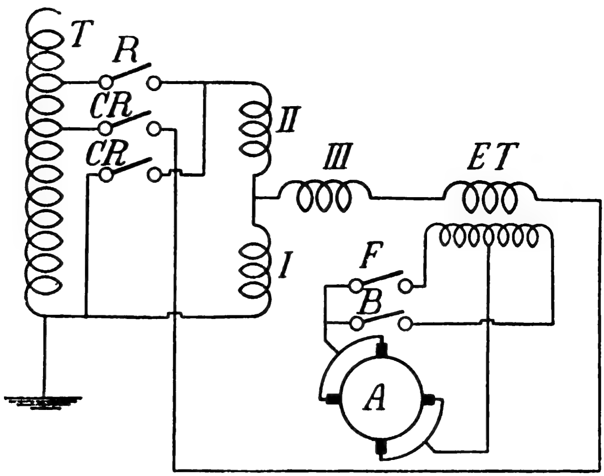

FIG. 14.

In the diagram shown, six different speed ranges are available, as 7 main contactors are provided.

When it is permissible to regulate the speed by brush shifting, the reversing switch (RS) may be omitted and the number of contactors very much reduced, as in Fig. 14. On closing the switch R, the brushes having been previously shifted forward in the direction of motion required, the motor may be gradually started by moving the brushes back towards the normal position, thus regulating the speed without voltage control. On changing over to the lower number of poles the field is excited partly by passing a magnetizing current through the armature and partly by the load currents, and the speed may be gradually increased by shifting the brushes into the normal position so that the load currents have no magnetizing action.

Fig. 15 shows a modification of the preceding diagram, the three main contactors being replaced by three high-tension switches in the

leads to the two main transformers T_1 and T_2. As such switches or contactors are necessarily interlocked with the brush-shifting gear, it

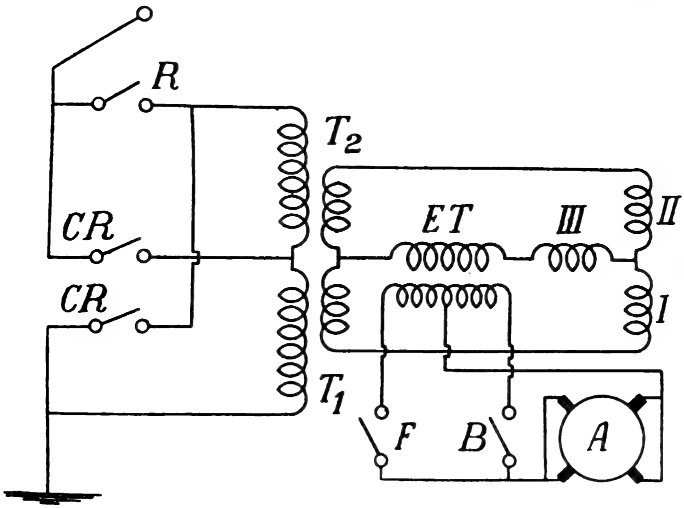

FIG. 15.

may be doubted whether it is desirable to introduce high-tension control even for the heaviest powers.

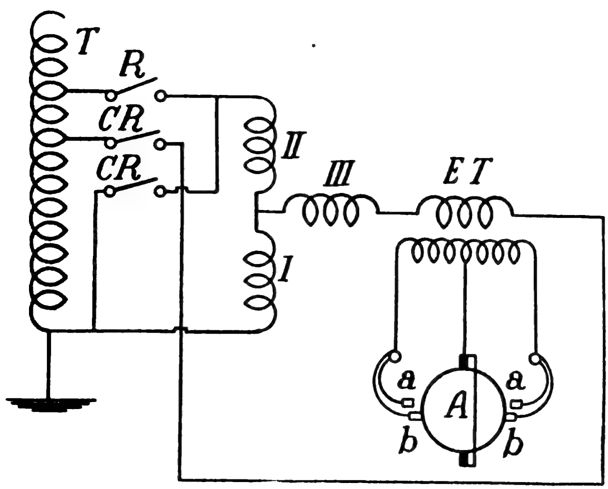

FIG. 16.

Fig. 16 indicates connections suitable for a pole-changing motor in which only half the total number of brushes are included in the work-

ing circuit when running with the lesser number of poles. The motor is started, after setting the brushes in the neutral position, by closing the switch R and moving the brushes in the direction of motion required. To change over to the lesser number of poles the brushes are first shifted back to the neutral position, thus taking off the load momentarily, and then the contactors C R are closed in place of R, the direction of rotation being determined by lifting off one of the two sets of magnetizing brushes, either *a* or *b*, all of which had been in use while the motor was running with 8 poles. This latter method of changing over appears to offer advantages for motors of large size, but even in such cases the machine with the full number of working brushes is more convenient, and would, no doubt, give the better results in practice.

Experimental Pole-changing Motor.

Some particulars of the motor installed in the James Watt Engineering Laboratories may be of interest, not only because the motor embodies the principles that have been dealt with above, but also because the method of testing the machine on load and in the separation of losses is not widely used, although convenient and capable of giving very reliable results. As shown in the illustration, Fig. 17, the single-phase motor is direct coupled to a small generator—a two-phase inductor-type alternator—which acts as a load. The output of the motor is controlled by regulating the excitation and load resistances of the generator, and is measured by the torque transmitted from the magnet-wheel to the stator of this machine. The stator, which is mounted on ball bearings, carries a graduated arm with a movable weight, and a counterpoise is fitted so that the complete alternator is balanced on its ball bearings when the moving weight is almost at its inmost position. The torque developed is therefore proportional to the distance which the weight has to be moved outwards in order to effect a balance. The moving weight is fitted with a vernier, and as the alternator is balanced with a slow period of oscillation and has little friction in its ball bearings, it is possible to measure the torque with remarkable exactitude, the error being within one-thousandth part of full load. The speed of the motor was measured by means of a vibrating reed frequency meter supplied from the generator circuit, and the same instrument on being connected to the single-phase mains gave the frequency of supply. The input to the motor was measured by a Kelvin and White astatic wattmeter, the voltage by a moving-coil instrument, and the current by a hot-wire ammeter, all of which were standardized before and after the tests by means of a potentiometer with continuous current.

The authors wish to express their indebtedness to the Carnegie Trust for the Universities of Scotland for the use of several of these instruments.

The motor tested is one rated at 10 h.p. at 150 volts, 30 cycles, this size being chosen to suit the supply available in the Laboratories. The

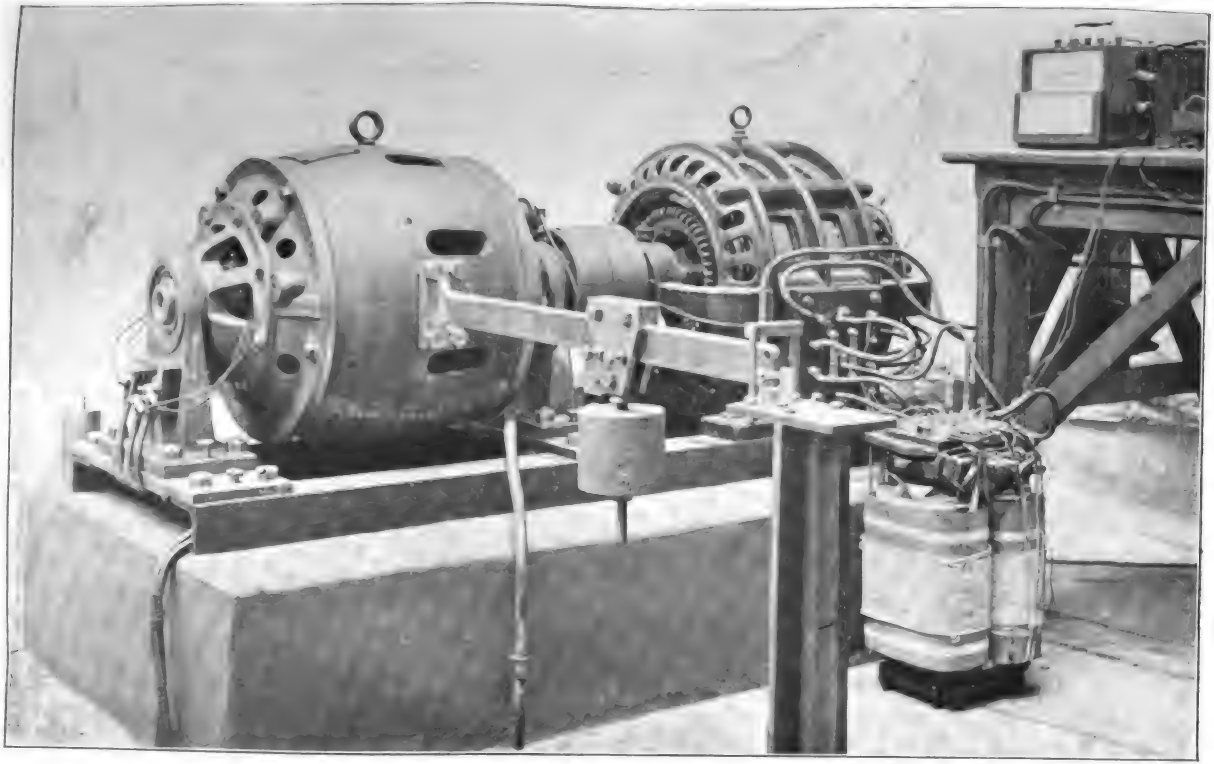

FIG. 17.

stator-winding is similar to that shown in Figs. 4 and 6, and is arranged for 8 and 4 poles, so that the speeds corresponding to synchronous running are respectively 450 and 900 revs. per minute. The motor was built by Messrs. Mavor and Coulson, Ltd., Glasgow, to the authors' design, with leading dimensions as follows:—

External diameter of stator core	457 mm.
Internal " " "	306 "
Length of stator core	160 "
Air-gap	0·75 "

The stator has 48 semi-closed slots, each of those allocated to the main-winding containing 9 conductors, while those of the field-winding contain 10 conductors. The armature has 49 slots, each containing 12 conductors connected in a two-circuit wave-winding without resistance connectors. The commutator has a diameter of 216 mm. with 147 segments, and is fitted with eight sets of brushes, each having two carbons $1\frac{1}{4}$ in. $\times$ $\frac{5}{8}$ in.

Magnetizing, short-circuit, and load tests were carried out with each of the two numbers of poles, and also with the brushes arranged so as to include alternately four and eight sets in the working circuit. For the sake of comparison the machine has also been tested by senior students as a compensated-series motor, supplied with direct and alternating currents, but only those tests which relate to the use of the machine as a pole-changing motor are given in the section that follows.

Magnetizing Tests.

In the first series of magnetizing tests, the armature of the motor was allowed to remain stationary, while single-phase 30-cycle current was supplied alternately to the field and compensating windings of the stator, these being connected for 8 poles. The brushes were lifted off the commutator in order to avoid the demagnetizing action of currents in short-circuited armature coils. The tests were therefore similar to open-circuit tests on a transformer, the voltage induced in the armature windings corresponding to that induced in the secondary of the transformer.

In Fig. 18, which gives the results of these tests, curves E_{FW} and E_A^f show the voltages in the field and armature windings respectively, when the field-winding was used as the primary. Curves E_A^c and E_{CW} show the voltages in the armature and compensating winding respectively, the latter being used as primary with two circuits in parallel.

In carrying out magnetizing tests with the windings connected for 4 poles, the armature was used as the primary and was kept running at the synchronous speed (900 revs. per minute) by employing the load alternator as a driving motor. Comparative tests were made with the two diagrams of connections shown in Figs. 8 and 9, and the winding-factors applicable when using the whole of the armature windings, or

alternatively half, for magnetizing were thus verified. In each case the stator windings were arranged so that the reconnected field-winding, Fig. 6, *b*, was coupled in series with coils I and II of the main winding, Fig 5, and the voltages induced in each portion were measured. The voltages induced in the reconnected field-winding, expressed as percentages of the total voltage, are given in the following table, together

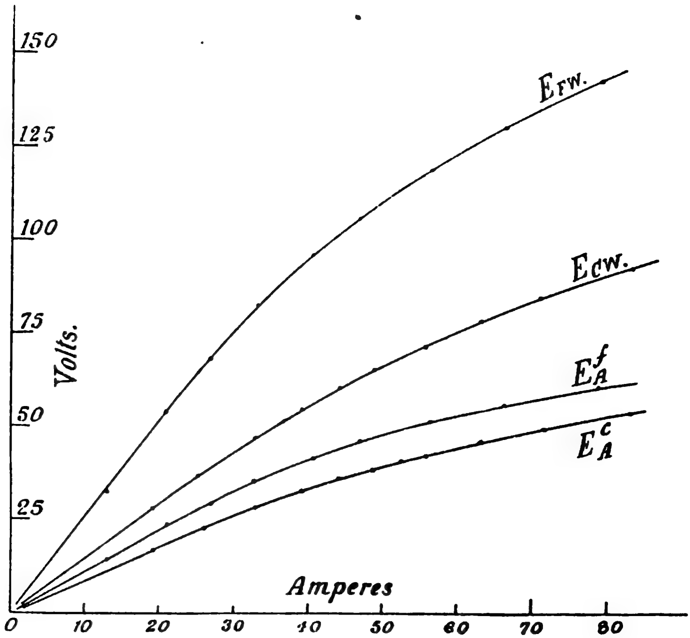

FIG. 18.

with the value calculated on the assumption that the field-wave follows a sine curve :—

Measured, with 4 brush connections	...	28·6 per cent.
Measured, with 8 brush connections	...	29·7 „
Calculated, on sine-wave assumption	...	29·1 „

From these figures it appears that the field-wave approximates very closely to a sine-wave. The two field-waves are shown in Fig. 19, *a*, while the magnetizing curves, giving the total induced stator voltage in terms of the magnetizing current in the armature, are shown in Fig. 19, *b*. The ratio between the tangents of the angles marked $a8$ and $a4$ is measured as 1·40, which agrees very closely with the value of 1·42 calculated on the assumption of a sine-wave field. For all practical purposes the field may be assumed to be a sine-wave, but naturally this

is only the case when care is taken in choosing the slot-step of the winding such that the field is not unduly distorted by the load-currents.

The accuracy with which the torque required to drive the armature could be measured (by the balancing apparatus already described) has led the authors to make a number of measurements of the iron losses in the core, when running at different speeds in constant and alternating fields of varying strengths. The amount of data obtained is considerable, and it is hoped to publish these results on another occasion.

Short-circuit Tests.

While short-circuit tests upon commutator motors are not generally useful, they are nevertheless of some importance; and particularly so in a pole-changing motor, where questions arise as to the effectiveness

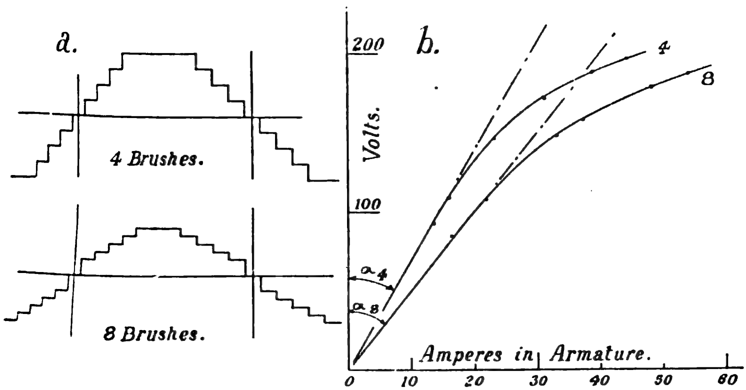

Fig. 19.

of the compensation between the primary and secondary windings. Four sets of short-circuit tests were made, viz. two sets with the armature connected for 4 working brushes and two sets with 8 working brushes. With each method of armature connection, tests were made with and without the reconnected field-winding (Fig. 6) included in the stator-circuit. The stator windings were used as the primary and were parallel connected, carrying a current of 70 amperes supplied at 30 cycles in each test. The values of the reactances measured were as follow :—

With 8 working brushes	with	F.W. (Fig. 6)	...	0·435	ohm.
„ 8 „	without	„ „	...	0·415	„
„ 4 „	with	„ „	...	0·470	„
„ 4 „	without	„ „	...	0·420	„

It appears that the use of the reconnected field-winding is very advantageous, especially when the armature is connected with 8 working

brushes; for while the reactance is only very slightly increased by its introduction, the output of the machine is increased by 30 per cent. The small increase in the reactance is remarkable when one reflects that the total number of turns in series is increased from 72 to 112, and serves to emphasize the importance of choosing the slot-step of the windings so as to avoid the presence of stray fields.

Load Tests.

In the load tests which are now described, the connections used were similar to those shown in Fig. 13. The motor was run with 8 poles as a plain repulsion motor and with 4 poles as a compensated-repul-

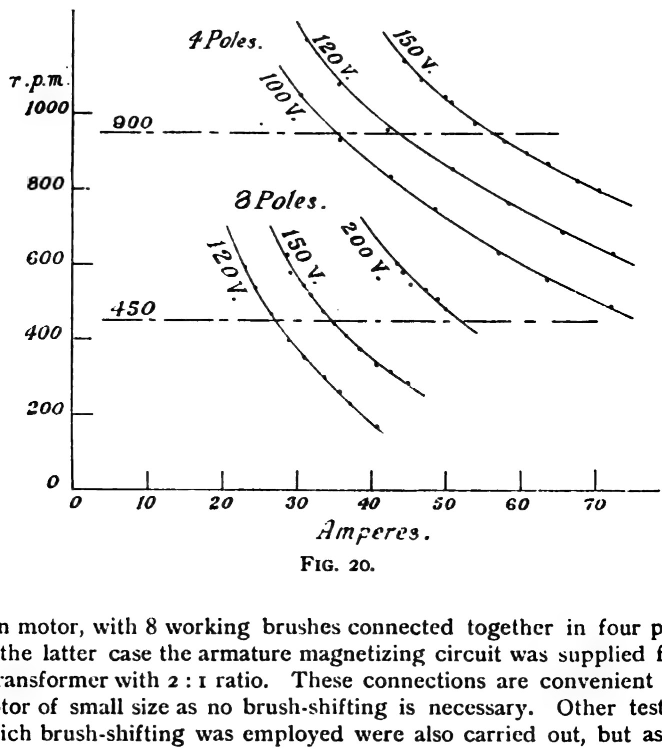

FIG. 20.

sion motor, with 8 working brushes connected together in four pairs. In the latter case the armature magnetizing circuit was supplied from a transformer with 2 : 1 ratio. These connections are convenient in a motor of small size as no brush-shifting is necessary. Other tests in which brush-shifting was employed were also carried out, but as the results are similar they need not be dealt with in detail.

Speed and Torque Characteristics.

Curves giving the relation between the speed and line current of the machine, supplied at various voltages, are shown in Fig. 20. The two sets of curves correspond to 8 and 4 poles respectively, and it will

be noted that the working speed ranges are in the neighbourhood of the synchronous speeds of 450 and 900 revs. per minute. In Fig. 21 the torque (measured in "equivalent horse-power at 1,000 revs. per minute") is plotted as a function of the speed. It will be noted that the curve for 200 volts with 8 poles approximately overlaps the curve for 100 volts with 4 poles, and it follows that if the connections are changed over from one of these ranges to the other, the motor continues to run against any given torque at approximately the same

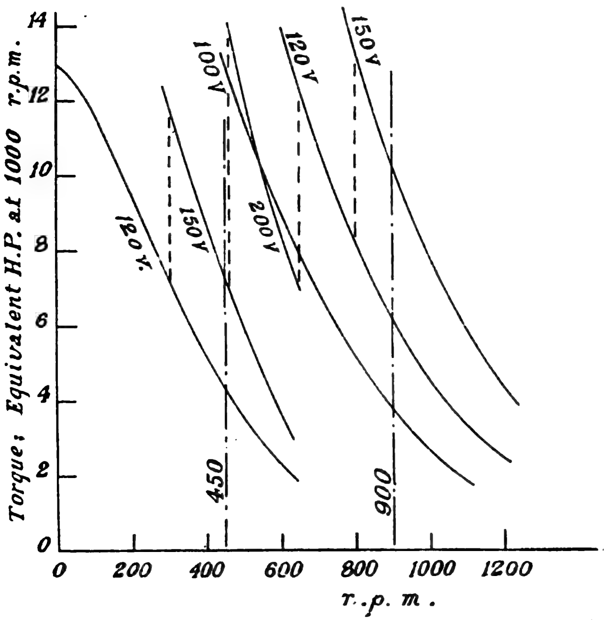

FIG. 21.

speed, and no mechanical shock is experienced. For lower speeds the 8-pole winding is supplied from transformer tappings giving 120, 150, or 180 volts; while for higher speeds the 4-pole connections are used with 120 or 150 volts. The dotted lines indicate the speeds at which the contactors are operated in order to maintain a high and fairly uniform torque during the starting period. These speeds are 300, 460, 650, and 800 revs. per minute. During this process the torque varies between 13 and 8 "h.p. at 1,000 revs. per minute." A higher average torque may naturally be maintained by changing over at somewhat lower speeds, and in this case the motor is liable to spark slightly when subject to the extra load. This is especially noticeable when changing over to 4 poles at the lower speeds, and on this account it is preferable to use the 200-volt 8-pole tapping in place of the corresponding 100-volt 4-pole connection.

Efficiency Characteristics.

In Fig. 22 two sets of curves are given showing the efficiency of the motor when supplied at different voltages and worked alternately with 8 and 4 poles. It will be noted that at all speeds under 460 revs. per minute the efficiency given by the 8-pole connections is greater than that obtainable with the 4-pole connections, while the reverse is the case at higher speeds. It is clearly disadvantageous to use voltage control as a means of speed regulation over too wide a range of speed, and preferable, in the case of a repulsion motor, to change the

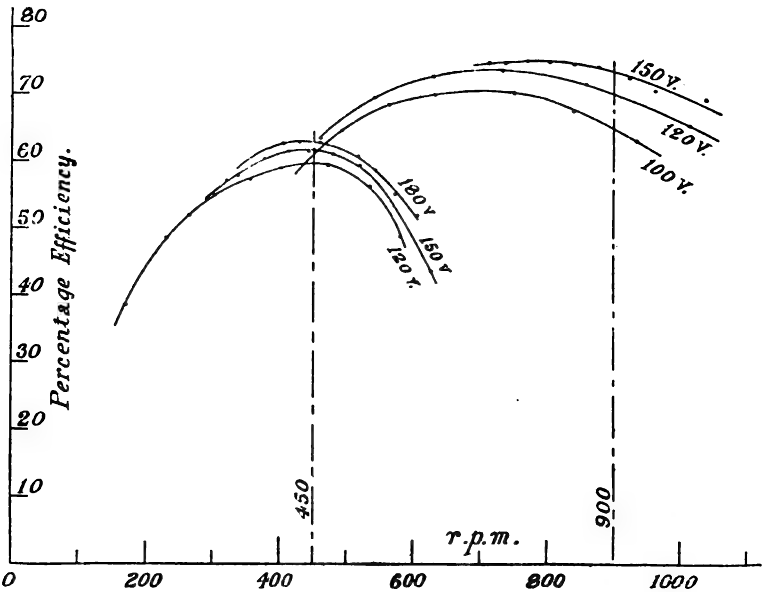

Fig. 22.

number of poles. The use of the greater number of poles is especially advantageous when the motor is required to develop its maximum torque at low speeds. The copper and constant losses are then more evenly balanced, and a higher efficiency is obtained.

In conclusion, the writers venture to express the hope that the foregoing investigation of the use of pole-changing windings in repulsion motors may be of interest to the Institution. It is not intended to suggest that the pole-changing motor is in general preferable to the compensated-series machine as a traction motor. Where low-frequency current is employed, it appears that the latter machine is simpler and equally satisfactory, as good commutation is obtainable, together with a sufficiently high power factor to meet practical requirements. When

a higher frequency is employed, the repulsion motor in which armature excitation is employed gives a materially higher power-factor than the series machine, and it may be that the pole-changing motor marks a sufficiently great advantage over the normal repulsion motor, as regards commutation, to warrant the use of the higher frequency. This question, however, is intimately connected with that of the economical generation and distribution of power, consideration of which would carry us beyond the scope of this paper.

Discussion.

Dr. S. P. Smith: The authors have certainly dealt with the subject very thoroughly indeed; but whilst I fully appreciate the work they have done, I dislike the results they have obtained. The motor they have developed does not appeal to me; I feel that it is fundamentally wrong, and the results, to my mind, prove that it will not be able to establish itself for the purpose they propose. I take it that their main object in developing this motor is to bring forward a motor that might be, under certain circumstances, used for single-phase electric traction. It is quite true that the repulsion motor has many other applications and is largely used in practice, chiefly, however, under stationary conditions for comparatively small outputs, where a constant speed is required. This speed is generally chosen somewhere about, or a little below, synchronism, owing to certain conditions which make the commutation conditions somewhat better at that speed. To me, therefore, the only justification for introducing the pole-changing winding seems to be to try and develop a motor that would be suitable for powerful locomotives for main lines, because for suburban traffic or short distances we seem to be more or less standardizing the continuous-current system. Dr. Smith.

Considering the proposed motor from this point of view, I think it would be impossible to build a motor of the type that the authors suggest, for it is very difficult to build even a plain repulsion motor for large outputs such as are demanded on locomotives. The present demand is for locomotives between 1,000 and 2,000 h.p., to judge by the single-phase locomotives now being built on the Continent. It is customary to use one or two motors, so that in any case at least 500 h.p. would be required, and it might be necessary to go up to 1,000 or 1,250 h.p. as in the new motors which will be ready for use when the Lötschberg tunnel is opened next year. It seems impossible, if one sketches out the design, to make a satisfactory repulsion motor for 500 h.p. or over at the low frequencies now adopted.

Repulsion motors are quite satisfactory for small outputs. For anything up to 50 or 100 h.p. they are splendid machines, and probably have a great future in front of them; but for very large outputs they are unsuitable. Then, in addition to that, there is this further complication of pole changing. I listened very carefully to the remarks the authors made with regard to power factor, and it seems impossible, to

Dr. Smith. my mind, to make that satisfactory. I cannot agree with Mr. Haigh's remarks about the shortened pitch. From my own experience, the only object in shortening the pitch, apart from saving a little copper and reducing the overhang, is to make the rotor winding identical with the stator winding, and thereby get a transformer with the minimum leakage and consequently the maximum power factor. Beyond that I cannot see how they are going to reduce the current by shortening the pitch. In the table that Mr. Haigh referred to on page 284, if a larger current is obtained in the ordinary motor that is easily explained, because they use fewer conductors. It is quite a simple matter to use more conductors and get a higher pressure. The final criterion of all such motors is the pressure between the brush tips, which at the present day must not exceed about 7 volts. Assuming a motor were designed to give that, I think it would be found that the plain repulsion motor would show up better than either the 4- or the 8-pole in the case given.

The main reason why I think the motor is fundamentally wrong is because it is—as pointed out by the authors—limited to synchronism, and even with a ratio of 1 to 2 in the number of poles only two speeds will be obtained, namely, synchronism and double synchronism. It seems to me that, for railway work, one utterly defeats the whole object of introducing a commutator when one gets a motor which is still essentially limited to synchronous speed, despite its series characteristic. All synchronous or induction machines suffer from the great disadvantage that they depend on the number of cycles for the speed, but the single-phase motors have also no inherent starting torque. To overcome that one brings in that big mechanical disadvantage, the commutator, thereby hoping to get a machine with all the good starting and regulating qualities of the continuous-current machine. For the sake of that a good deal is sacrificed, because all the bad qualities of the alternating current are introduced. If we are thus going to make a big sacrifice by using a commutator, it is most important that we get every advantage from the commutator, and not find as a secondary effect that the motor is still dependent upon the frequency. The repulsion motor is ultimately dependent upon the frequency, for the only proper speed for sparkless running is in the neighbourhood of synchronism or, in the authors' motor, of synchronism and double synchronism.

On the other hand, in the alternating-current rival, the compensated series motor, which is just an ordinary series machine compensated to do away with the reactance, and if necessary with a local field to support commutation, we have a motor independent of synchronous speed. It is certainly a worse motor than the continuous-current motor for ordinary use, but for traction work and for obtaining a good series characteristic the repulsion motor cannot be compared with it. With the additional complications that the authors are introducing, I think it will be impossible to build such a motor for a large output. The only promising repulsion motor for traction seems to be the Dèri motor, where the stator can be wound for high tension, say, 10 or 15 thousand

volts, and starting is accomplished without any special apparatus other than that needed to rock the brushes out of the no-load position into the working position. But the trouble is that the machine remains a repulsion motor all the time; and I am afraid the present motor will be subject to the same disadvantage. Therefore, for that reason, I cannot think that the proposed motor will ever bring us any further forward in our search for a good single-phase commutator motor for traction work. Dr. Smith.

Mr. ROGER T. SMITH: Most railway engineers will warmly welcome the attempt by the authors to improve the single-phase motor for traction purposes. It is one of the tools used by railway engineers that is very much in need of improvement. The authors have been so modest in the claim put forward as to the advantages of their special design as almost to disarm criticism. I wish to speak, however, from the point of view of one who does not criticize design *per se* at all, but only asks for a definite result, expecting others to make a design which will give that result. On page 274 the authors have, I think, very fairly stated their reasons for making a pole-changing design of single-phase repulsion motor. The table on page 284 seems to me to show that the resulting pole-changing motor is shorter than the corresponding repulsion motor of equal diameter, but has a less output. I should like to know if this is so. The advantage for traction purposes in shortening the motor is inconsiderable compared with a decrease in diameter. One of the great difficulties with railway motors applied directly through gearing to the axle and not raised up as in a locomotive, is that a reasonably sized motor in every case that I know of encroaches seriously on the load-gauge allowed by the Clearing House Rules. The underside of the motor is always outside the standard load gauge. It is against the rules, but it is accepted. Consequently any decrease in the size of a motor should, if possible, be in its diameter rather than in the length. The authors claim that the pole-changing motor is most suitable for small sizes, by which I presume they mean motors up to about 200 h.p., which is about the maximum size used for multiple-unit trains. But it is just this type of motor, used for a suburban service with short distances between stops, in which weight is of the utmost importance, and I would ask the authors if the weight of their pole-changing motor is less or greater than that of the corresponding repulsion motor having the same output. If the weight is not less I hardly think that the advantages of the design would lead to its adoption for multiple-unit equipment motors, though of course in smaller sizes it might be useful for large fans and also for electric lift and crane work. On page 284 the authors also say that for large motors, to which the last speaker referred, *i.e.* motors over 500 h.p. and up to, say, 1,000 h.p. in output, this type of motor would give less output than the corresponding series commutator motor. I should like to ask the authors if that comparison is on the basis of size or of weight, that is, whether the corresponding series commutator motor is smaller in extreme Mr. Smith.

Mr. Smith.

overall dimensions or less in weight. In Fig. 16 on page 287 I agree with what the authors say, that the arrangement for halving the number of brushes by momentarily moving them back to the neutral position and so absolutely stopping the output is not a thing that can be considered from the point of view of a traction motor. In Fig. 21, on page 293, the diagrams show that approximately there is the same torque at the same speed for the highest voltage with 8 poles and for the lowest voltage with 4 poles, which makes the change-over from 4 to 8 poles quite possible and satisfactory for traction purposes. But I would ask the authors if it is always possible in a motor of this sort to arrange the tappings of the step-down transformer so as to obtain these conditions; because if it is not always possible there are going to be quite serious difficulties in the change-over from 4 to 8 poles. As far as I can gather from Fig. 22 on page 294, the authors have hardly made out a case for the advantages of the pole-changing repulsion motor except for high periodicities. They say that the advantages are much greater for high periodicities than for low. I should like to ask them what exactly is meant by high periodicity. I do not think that anybody would, for traction purposes, ever use more than 25 periods; and I assume that the advantages of this arrangement, as far as periodicity is concerned, only come in when the number of periods is considerably above 25. In connection with the measurement of the torque of the alternator driven by the motor under test, the stator is carried on ball bearings so that the torque can be actually weighed. In all other cases which I have seen of that form of torque measurement the stator has been supported on knife-edges and not on ball bearings. The authors must have had a great deal of experience with this machine. If the ball-bearing suspension is as satisfactory from the point of view of negligible friction as the knife-edge it must be a much more satisfactory mechanical form of torque measurement than the knife-edge arrangement. I would ask if this is their experience.

Mr. Mordey.

Mr. W. M. Mordey: I wish to refer only to a side-issue in the paper. The authors describe, and illustrate by Fig. 17, an interesting machine on which measurements of considerable accuracy can clearly be made. They state they have made a number of such measurements of the iron losses in the core of the rotor when running at various speeds, both with direct and alternate currents. They hope to publish the results on another occasion. Allow me to encourage them in that intention, and to make a suggestion. One of the outstanding questions connected with the magnetization of iron in dynamos—a question which might perhaps usefully be taken up by the Research Committee which the Council has just formed—is the difference between the losses with a rotating flux and with an alternating flux of the same frequency and density. I do not think there is much reliable information published on that question; it is a practical question on which dynamo designers would probably be very glad of accurate information. Increased core losses with a rotating flux—as was found with an alternating flux—are often apparently considerable, partly no doubt because they

are made to include pole-piece or other losses which are not properly iron-core losses at all. My suggestion is that for the purpose of such losses the authors might use their machine in a particular way, keeping both rotor and stator fixed—except, of course, so far as the stator may move about its zero position—I may illustrate what I mean by a diagram (Fig. A). The two-phase stator S carries a graduated arm, A, on which is a weight, W, adjustable in amount or position. R is the rotor, which I propose should also remain stationary. Now if S is magnetized as a two-phase stator, the equivalent of a rotating direct-current field will pass through the fixed rotor R, the induction in which may be measured on certain assumptions by a suitable fixed winding. The stator S will tend to follow the rotating magnetism and will be brought back to zero by adjustment of W; this will give the energy, the speed of the rotating magnetism being known from the frequency. If there is anything in this suggestion it should be possible to get the effect of rotation without having to revolve either the field magnet or the armature, and without having to make any correction for friction. Mr. Mordey.

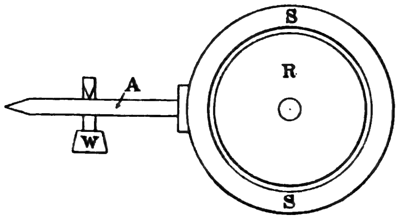

FIG. A.

Mr. W. E. BURNAND: These commutator motors are chiefly used for starting up heavy loads; and unfortunately it is at starting that the awkward features of the machine are most in evidence. There is its reactance, mainly of the field winding; the short-circuit current in the armature coil under the brushes; and then, when it begins to move, the usual sparking due to the self-induction of the coil undergoing commutation. As is pointed out in the paper, the field reactance is minimized by using a very small air-gap; but it is unfortunate that that small air-gap increases the self-induction of the armature coils, so that it makes the sparking very much worse when the machine is running. I think that is an argument for the use of a non-uniform air-gap. This is applicable to the plain series machine, but in a repulsion motor it is a disadvantage, as it would upset to some extent the rotary field; it would make it non-uniform, and it would increase the loss. Probably the worst feature is the very heavy short-circuit current at the moment when the motor starts up. That is evidently a very serious defect, because if one looks at any alternating-current driven train one finds the initial acceleration is very slight. That is due to the large amount of energy that has to be got rid of in a very limited space in the Mr. Burnand.

Mr. Burnand.

resistances between the armature winding and commutator bars, to keep down the short-circuit current. Another point is that this heavy current through the short-circuited coil is acting almost directly against the main flux, thus making it doubly serious. The only way I know of to get over that, is to utilize an open-circuit type of winding. This brings the short-circuited coil into a neutral part of the field with little or no tendency for a heavy current to circulate. I believe that method has been proposed by Steinmetz in America. It removes the difficulties connected with starting and the heavy short-circuit current through the brushes, but it introduces other difficulties as soon as the motor starts. The commutation conditions of an open-circuit winding are very difficult, but I have no doubt that they can be overcome.

I think that the future machine for starting up with a heavy load will resemble very closely the continuous-current type of machine; that is, it will be a plain series salient-pole machine, but with an open-circuit winding and compensated windings for the armature. It is rather curious that my first attempt to make a motor, some twenty years ago, combined all these features. Since then I have built a number of alternating-current commutator motors, and although I favour the straight series machine, when these have been tried I have nearly always found them give better performances as repulsion machines. I do not think, however, that is going to be the final type. I notice the very low average efficiency shown in Fig. 22, and the rapidity with which it decreases as the speed is reduced. I think it is quite evident from these curves in Fig. 22 that for stationary work, where two or three phases are available, the ordinary slip-ring motor would give a better performance; that is, wherever the load near synchronous speed is on for any length of time, say even two or three times the starting period, it would give a much better result than a commutator motor as now made.

Mr. Haigh.

Mr. B. Parker Haigh (*in reply*): Dr. Smith has pointed out the difficulties that exist in regard to the voltage induced between the brush tips in all repulsion motors. The difficulties are exactly the same when starting any compensated series motor. It does not matter of which type the motor is, the difficulty is the same when there is an equal number of poles in each. The pole-changing motor was devised with the idea of limiting this difficulty. The number of turns given in the table on page 284 is not chosen haphazard, but so that under working conditions the liability to sparking is the same in the three motors. Therefore it is not a fair argument to use against the pole-changing motor, that the greater number of turns with which it is credited here is simply a handicap. That is not the case. The difficulty with regard to the sparking at starting is the same in each of these three motors, and therefore I claim that the comparison is fair.

The maximum voltage that exists between the brush tips in the case of an ordinary repulsion motor has got to be measured when the motor is standing still. In the case of the pole-changing motor, on the other

hand, it is only necessary to measure that voltage when the motor is running at half-speed, because from no-speed up to half-speed the 4-pole connections are not intended to be used; only the 8-pole connections would be used, and these give little or no trouble as regards sparking. That point, however, is dealt with in the portion of the paper that precedes the table, so that I need not now go into the matter more fully. Mr. Haigh.

I think that the synchronous character of the repulsion machine was perhaps overrated. It is a series machine in its characteristic, and except in its advantages does not differ from the compensated series machine as regards synchronous speed. The one respect in which the repulsion machine is handicapped by considerations of synchronism is that it is not always possible to use as many poles as may be desired—and it is precisely here that the feature of pole-changing offers advantages.

I only suggest that the pole-changing motor is suitable for machines of moderate size. In very large motors the present practice seems to be to use a low frequency and a compensated series machine. In reply to Mr. Roger Smith, a number of calculations that I have made show that in the case of large motors of 500 to 1,500 h.p. the series motor is considerably lighter, although of about the same overall dimensions. When we speak of comparatively high frequencies, we mean only moderately high frequencies. I would call 25, therefore, a high frequency for work of this class. There are undoubtedly great advantages to be gained if we can use such a moderately high frequency instead of being forced to use such extremely low frequencies as have become common for single-phase work. Those who are likely to adopt single-phase motors would much more readily do so, I think, if they could adopt such a frequency as would render their generators available for other purposes as well as railway traction. The reduction in the dimensions that can be effected through using the pole-changing motor is, as Mr. Roger Smith points out, mainly on the length, and that is not the direction in which a reduction would be most valuable; but to a certain extent a compromise can be effected between the diameter and length. The design of most single-phase motors has been so very cramped that I think mechanical engineers would very willingly accept even a reduction in the length as tending towards reliability in service, which is no small matter.

The tappings from the transformer can be quite easily arranged so as to give a smooth change-over: Thus in Fig. 13 it will be seen that the tapping which we may term the "negative" from the transformer is made at a lower position for the 8-pole repulsion motor connections than that which is used for the 4-pole compensated-repulsion machine. That means that the voltages which are used in that particular case for repulsion work with 8 poles are, on the whole, higher than those used with the 4-pole compensated connections. There is no difficulty in arranging such tappings. It will be noticed that the pole-changing connections are greatly simplified by the fact that two speeds are

Mr. Haigh. available for each transformer voltage, instead of only one as in ordinary motors.

Mr. Nicholson. Mr. J. S. Nicholson (*in reply*): I should like to make a few remarks on the comments of Mr. Roger Smith and Mr. Mordey with reference to the measurement of the torque of the single-phase motor. Mr. Roger Smith mentioned the probable advantage of ball bearings over knife-edge supports for the stator of the alternator which served as a brake. We have had no experience of knife-edge supports in this connection, but so far as this machine is concerned we had no difficulty in measuring accurately the small torque required to rotate the armature of the single-phase motor in its magnetic field (the alternator serving as a synchronous motor to rotate the armature of the single-phase motor). On load tests the torque was probably measured with a greater degree of accuracy than was the electrical input to the motor. The slight amount of vibration served to make the ball bearings practically frictionless. This was proved by the fact that when a balance had been obtained and the brake lever was raised or lowered a short distance, it returned at once to its former position of equilibrium.

The diameter of the ball race should be kept as small as possible. In the present instance a couple of cone-shaped cast-iron pieces were bolted to the end-shields of the alternator. These carried the ball races, as shown in Fig. 17, the whole machine being supported by two cast-iron columns bored to fit the ball races. The ball bearings are certainly a more satisfactory mechanical arrangement than knife edges. With this arrangement of brake the only possible error would lie in the recording of the windage torque of the brake rotor. The alternator, however, is practically completely enclosed so that the windage torque is included in the brake readings. In any case, supposing there was a big windage torque, we could uncouple the brake alternator and run the alternator as a motor. The torque recorded by the lever would then be the windage torque.

Mr. Mordey's suggestion for the measurement of the losses in the core of the rotor due to alternating or rotating fields is, in my opinion, excellent; I hope we shall be able to adopt it and thus confirm the results already obtained. We have carried out a considerable number of tests on iron losses by measuring the torque required to rotate the rotor at various speeds in the magnetic fields, and to avoid complications we have also got an additional rotor core from Messrs. Mavor and Coulson without slots; we are starting experiments with this.

Proceedings of the Five Hundred and Forty-sixth Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 19th December, 1912—Mr. W. Duddell, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 12th December, 1912, were taken as read, and confirmed.

Messrs. I. H. Jenkins and R. W. Hughman were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

James Conner.
Graham Morton Stevenson.
James Wagner.
John Crum Whitmoyer.

As Associate Members.

Lawrence G. Bennet.
Augustus Oscar Buckingham.
Arthur Edward Clarke.
Thomas Ingram Craig.
Charles George Cutbush.
Thomas Emile Dransfield.
James Frederick Driver.
Samuel Ferguson.
Walter William E. French.
David Fulton.
George Lawrence Gifford.
Herbert Henry Gresswell.
Ehret Ernest Grover.
John Hirst.
Sidney John Hough.
Reginald Lewis Jenkins.
James Joseph Kavanagh.
Alfred George Knapp.
Robert Hermann Kulske.
Alban Lewis Lovill.
Andrew G. L. McNaughton.
Beaufoi John Moore.
Richard D. Thomas-Jones.
Frederick Thompson.
John Thomson.
Robert Townend.
Joseph Tronche.
Alec Edward Guy Wood.
William Woodward.

As Associates.

Thomas Willis Cole.
George Franklin.
William Henry Wilson.

As Graduates.

D. Visvanath Ayya.
Willie Barraclough.
Harold Bishop.
P. C. Chakravarti, B.A.
William Randolph Churchill.
John Rhys Danson.
Leslie James Doudney.
William Henry Duncan.
Edgar William Fleming.
Walter Trevethan Hilder.
Donald Courtney Jewell.
Thomas Sidney L. Mann.
Gordon Juro Orchiston.
Ernest Richard Phillips.
Harry Hollins Powell.
Ernest Bloxidge Preston.
Laurie Stuart Richardson.
John Wilfred Rodger.
Robert Kidd Simpson.
John Edward Walker.

As Students.

Charles James R. Alsford.
T. K. Ramasami Ayengar.
Edward Gentleman Bannister.
Ernest Blakemore.
Guy Desmond Canton.
Edward Lawrence Chadwick.
Claude Stuart Coombs.
Wilfred Windham Cooper.
Edward James Edgar.
Maurice Ord F. England.
George Ferneaux.
Lewis Aylmer Fookes.
Cyril Herbert Ford.
Gordon Minter Frieake.
Harrold Cracroft Gibson.
Leslie John Hancock.
William Gordon C. Jackson.
Hugh Lionel de K. Millington.
Samuel Emil Newman.
Walaiti Ram.
James Theodore Rodwell.
John Gale Wellings.

TRANSFERS.

From the class of Associate Members to that of Members :—

Arthur James Abraham.
William Thomson Anderson.
Arthur William Ashton.
Wellesley Curran Clinton.
Claud Crompton.
Arthur Dimmack.
Harry Percy Girling.
Sydney Elliot Glendenning.
Lawford Stanley F. Grant.
Arthur Stephens Herbert.
William Thomas Hodgson.
Edward Massey Hollingsworth.
Percival André Lundberg.
Joseph Lustgarten, M.Sc.
Norman Wm. Prangnell.
Frederick Walter Purse.
Herbert Walter Ridley.
William M. Selvey.

From the class of Associates to that of Members :—

Henry Melville Ackery.

From the class of Associates to that of Associate Members :—

Richard Gilbert Allen.
Ernest Vincent Graham.
Arthur P. Hutchinson.
Robert Alex. Downes Macalister.
John Francis McMahon.
Reginald Savory.

From the class of Associates to that of Graduates :—

Charles Edward Clayton.

From the class of Students to that of Associate Members :—

Frank Embleton Allen.
William John Adcock Anderson.
Arthur Chantrey Baker.
Christopher Charles Casperd.
Bernard Alfred M. Cooper.
Harry Norman Cunliffe.
Ronald Stuart Dolleymore.
Clarence Harold Lauth.
Frederic H. R. Lavender.
Francis A. Lawson.
Lionel Douglas Leonard.
Alexander Rodger McCallum.
Alexander Simpson MacWhirter.
Kenneth Graeme Maxwell.
Thomas D'Arcy Nassau.
Henry Hartley Pearson.
Edward Stanton Ritter.
Harry Gilkes Sharp.
Maurice E. F. Shuttleworth.
Frederick Herbert Williams.

From the class of Students to that of Graduates :—

Francis Edgar Burnett.
Amulya Charan Mukherjee.

The PRESIDENT : I have to announce that Mr. Hirst has presented to the Institution a complete set of the *Electrician* portraits of the early electrical engineers.

A vote of thanks to Mr. Hirst was carried by acclamation.

A paper by Professor Silvanus P. Thompson, Past President, entitled "The Aims and Work of the International Electrotechnical Commission" (see page 306), was read and discussed, and the meeting adjourned at 10.6 p.m.

Before reading his paper, Professor Thompson announced that Mr. Le Maistre, the Secretary of the International Electrotechnical Commission, had received the following telegram from Mr. Arthur Balfour : "Should greatly like to be present Thursday, but unfortunately have engagement in the country. Please to convey my great regret."

THE AIMS AND WORK OF THE INTERNATIONAL ELECTROTECHNICAL COMMISSION.

By PROFESSOR SILVANUS P. THOMPSON, D.Sc., F.R.S., Past President.

(*Paper read before* THE INSTITUTION 19*th December*, 1912.)

This address is prepared in consequence of a request made in January, 1912, by the Papers Committee of the Institution of Electrical Engineers, that a report should be presented upon the objects of the organization known as the International Electrotechnical Commission, and upon the work which it has already accomplished. To fulfil the aim of giving an account of that organization it is expedient to prefix some general observations upon the prior development of electrical affairs, and upon the antecedent events which led to the formation of the Commission.

It is self-evident that as science grows, and as its industrial applications develop, there will follow a growth and development of scientific ideas, entailing a corresponding growth in the technical terms in which those ideas are expressed. Now in every civilized nation there are independent advances made both in scientific discovery and in technical invention. And since the work of the independent pioneers who from time to time arise is the product both of the individual genius and of his environment, it must needs be expressed in terms that are tinged with the idiosyncrasies of the language of the nation in which the discovery or invention appears. Hence it necessarily follows that in different nations differences of usage arise in the employment and connotation of scientific terms, and in the methods adopted for the description and specification of the apparatus and machines which the advance of science has brought into being.

On the other hand, in the progress of civilization in general, and in the development of the national resources of the various peoples, the most potent factor has ever been, and will ever be, the interchange of natural products and manufactured commodities. In a word, commerce, the exchange, direct or indirect, of merchantable produce between the nations, has been the keynote of their material development. The great industries of the world, whether in textiles or in machinery, whether in steel or in chemicals, whether in the preparation of food-stuffs or in means of transportation, have expanded amazingly

in modern times; and that amazing expansion, essentially based upon the commercial intercourse between the populations of different parts of the habitable globe, is both the result of scientific progress in the past and the incentive to scientific advance in the future. But the unification of the scientific bases of commerce is far from complete. Differences of race and language are far too deep-seated for any present possibility of a universal accord devoid of differences of usage. It is true that all the nations have now accepted a gold standard as the basis of exchange, and that a world's postal system is nearly complete. But in other matters the nations are far from being at one. The dream of one universal language is as vain as the dream of an impossible and barbarous Utopia of equal wealth. Even in matters that are within the grasp of immediate possibility the most advanced nations are the most conservative. While all other civilized nations of both hemispheres have adopted a decimalized coinage, Great Britain retains a mixed decimal and duodecimal system to her own great disadvantage. In so simple a matter as the measurement of temperature, the inhabitants of Great Britain, with the exception of half her scientific men, still keep to the irrational and arbitrary scale invented by one Fahrenheit of Dantzig; while most of the European nations have adopted the more rational (though also arbitrary) scale invented by Celsius of Upsala. None has yet accepted the rational thermodynamic or absolute scale suggested sixty-four years ago by Lord Kelvin. It is the meteorological observers who block the way of international agreement in this matter. Nevertheless, it is essentially true that human progress and the development of commercial relations depend on unification of practice. Where should we be if every town in the empire had its own coinage, every county its own different system of weights and measures, every railway its own different gauge? At the beginnings of every new departure, differences of practice, of standard, inevitably arise. Then commerce steps in, involving broader issues. With the widening and development of the incipient industry unification becomes inevitable. Let us take a simple instance in a matter familiar to every member of this Institution. In the early days of telegraph engineering, the engineer found it necessary to be able to express numerically the values of the resistances offered to the flow of the current by the instruments and the batteries used in working the telegraph lines. Having need of some standard in which to express such values, what standard would he more naturally adopt than a fixed length, whether a mile or a foot, of the particular kind of wire used in his work? And so it came about that in the fifties and sixties the English telegraph engineer used to describe the resistances of his coils and his batteries in terms of a mile of a No. 16 gauge copper wire, and the French telegraph engineer in terms of 1 km. of iron wire 4 mm. in diameter. Then Jacobi, in Russia, proposed * as a standard a copper wire 1 m. long and 1 mm. in diameter. Werner von Siemens, in Germany, proposed † for his unit a column of mercury

* *Comptes Rendus*, vol. 33, p. 227, 1851.

† *Poggendorff's Annalen*, vol. 110, p. 1, 1860.

1 m. long and 1 sq. mm. in cross-section. All these arbitrary units came into more or less extensive use, with a resulting confusion, since their values differed widely from one another.

Take another instance where the co-ordination is still very imperfect. James Watt had the genius to perceive that the duty of an engine must necessarily be expressed in terms of work done in a given time; and, having need of a unit, he suggested the horse-power, which he defined in terms of British gravitational units as 33,000 ft.-lb. per minute (at London), which is $7{\cdot}46071 \times 10^9$ ergs per second,* or 746·071 watts. But when Continental engineers sought an equivalent measure in terms of which to express the duty of their engines, they must needs express that equivalent in terms of the metre and of the kilogram, and, to avoid awkward fractions, settled upon 75 kg.-m. per second (at Paris) as the nearest figure, in round numbers, to 550 ft.-lb. per second, and they denominated it as the *Cheval-vapeur*, being equal to $7{\cdot}3588 \times 10^9$ ergs per second,* or 735·88 watts. It is not even equivalent, being 1·386 per cent lower. Nowadays we are trying to get rid of both these arbitrary units, in order that we may adopt the international kilowatt as the unit of power.

The impulse towards unification of electrical measures came first through the practical submarine telegraph pioneers, whose methods of electrical measurement were even in the fifties far more scientific than those of the professed men of science. All honour to Gauss and Weber for their suggestions of an absolute system of magnetic and electrical measurement; but the inception of our present system of international units dates from a paper read at the British Association meeting of 1861 by two distinguished telegraph engineers—Sir Charles Bright and Mr. Latimer Clark, both of them honoured Presidents of this Institution in its early days. That paper led to the formation of the historic Committee on Electrical Standards of the British Association, which in the course of its six years' labour formulated definitions of the ***ohm***, the *volt*, the *coulomb*, the *farad*, and the *weber* (now called the ***ampere***), and gave the world the first approximate determination of the value of the standard of resistance, the *ohm*. We honour the names of Kelvin, Clerk-Maxwell, Fleeming Jenkin, Grylls Adams, and Carey Foster for their labours in this co-operative effort; nor do we forget that they enjoyed the counsels of Werner von Siemens in fixing the magnitude of the standard, and of Matthiessen in selecting the material—platinum-silver alloy—for its construction. But with the course of years the need was felt for increased precision and of wider agreement. To the earlier industry of telegraphy there had now been added that of telephony;

* These are the values given in Latimer Clark's *Dictionary of Measures* (1891), where the pound is given as 453·5926 grammes, the foot as 30·47945 centimetres, and the value of the gravitation constant is given as 981·17 at London and as 980·94 at Paris. In Carl Hering's Conversion Tables (1904) the horse-power is given as $7{\cdot}45650 \times 10^9$ ergs per second, and the *Cheval-vapeur* as $7{\cdot}35448 \times 10^9$; the pound being taken as 453·5924277 grammes, the foot as 30·4800 centimetres, and the gravitation constant as of the ***mean*** value 980·5966. As the constant of gravity at Berlin is 981·27, the German *Pferd-kraft* of 75 kilogram-metres per second is equivalent to 736·115 watts.

and electric lighting had sprung into existence as a new commercial field. A Select Committee of the House of Commons inquired, in 1879, into the legal questions involved in the public supply of electric energy. Great developments, involving vast commercial interests, were in advent. Legislation was impending. The times were ripe. And so it came about that the years 1881–1882 witnessed three important events: two of them immensely helpful, the third disastrous. The first was the ever-memorable Electrical Exhibition at Paris; the second the International Congress of Electricity at Paris; the third the passing of Mr. Chamberlain's Electric Lighting Act, which set back the industry for years, and had to be repealed ten years later.

The Paris Congress of 1881, the first of its kind, was truly International: that is to say, it was the meeting of a body of delegates sent officially either by Government departments or by scientific institutions of recognized authority in the various nations; and its deliberations were presided over by the French Minister of Posts and Telegraphs. The foreign Vice-Presidents were Lord Kelvin, Signor Govi, and Professor Hermann von Helmholtz; while Werner von Siemens, Du Bois Reymond, Mascart, Clausius, Wiedemann, Lord Moulton, Rowland, and Eric Gerard were amongst the delegates present. At this Congress there was a strong feeling in favour of abandoning the British Association's unit of resistance in favour of Siemens's unit, the column of mercury one metre long. It had become known from measurements made by Joule, Rowland, Mascart, H. F. Weber, Roiti, and Lord Rayleigh that the numerical value of the B.A. unit was in error by a small percentage as compared with the theoretical "ohm." There was a warm debate: but disruption was averted by a timely adjournment, during which, by the efforts of M. Mascart and Lord Moulton, a compromise was arranged to accept the B.A. ohm, but to represent it concretely by a column of mercury of appropriate length, the precise specification of which was to be fixed after further researches at an adjourned conference in the next year. The names of the electrical units—*ohm*, *volt*, *coulomb*, and *farad*—were agreed to, but the name of the unit of current was, on the motion of von Helmholtz, changed from *weber* to *ampere*, in honour of the great French savant. Thus was international agreement on units secured by mutual concessions. The commission charged with determining the ohm met in 1882, adjourned to 1884, and then adopted for acceptance during the next ten years, as representing the ohm, a mercurial column 106 cm. long, though it was known that the true value was nearer 106·3 cm. The second Paris Congress of 1889 adopted the *watt* and the *joule*, also the unit of self-induction of 10^9 cm., or one *quadrant* of the earth. In December, 1890, the Board of Trade appointed a Committee consisting of two members each from the Board itself, the General Post Office, the Royal Society, the British Association, and the Institution of Electrical Engineers, to advise it as to adopting with legal force the denominations of the various electrical standards as settled in 1889. On that Committee, which met from

January to July, 1891, we find the names of Lord Kelvin, Lord Rayleigh, Dr. Glazebrook, Professor Carey Foster, and Professor Ayrton. The result was the Order in Council of August, 1894, which gave legal force in Great Britain to the standards in the custody of the Board of Trade for the *ohm*, *volt*, and *ampere*.

All that has occurred since 1889 in the matter of units has been in the direction of more precise specification of their values. At the British Association meeting of 1892 at Edinburgh, the length of the mercury column was reaffirmed at 106·3 cm. at 0° C.; but instead of prescribing its cross-section, its mass was declared to be 14·4521 grm. At the International Electrical Congress of Chicago, in 1893, the five fundamental units were reaffirmed, with the adjective "international" prefixed to each of them. The name *henry* was assigned to the unit of self-induction, and the *joule* and the *watt* were defined as units of work and of power, respectively, in the centimetre-gramme-second system, irrespective of any electrical quantities whatever. The third Paris Congress, of 1900, added the names of *maxwell* and *gauss* to denote the units of magnetic flux and of intensity of magnetic field, respectively.

The different nations, however, had followed no uniform plan in giving legislative effect to the various resolutions of the Congresses. Some had already ratified the earlier suggestions, others had waited for riper results; and though the discordance thereby stereotyped was not great, yet it was great enough to arouse fears of future trouble. But meantime a new and powerful influence was making itself felt. The establishment of the Reichsanstalt at Berlin, with its standardizing laboratories and highly trained staff, was followed by similar creations in other countries. France, in its Laboratoire Centrale d'Electricité; England, in its National Physical Laboratory at Bushy House; the United States, in its Bureau of Standards, have now first-rate institutions for the scientific determination of the electrical standards and their comparison with one another and with the units which they represent. Informal conferences between the representatives of the great laboratories are continually taking place. One was held at Charlottenburg in October, 1905, with the late M. Mascart in the chair. Sometimes a representative of one laboratory will reside for a month or two at another laboratory while joint tests or joint comparisons of standards are made. When three or four great laboratories, after such comparisons, concur in judgment, there is little room for doubt or delay. Hence the long-vexed question of the units has been practically removed from the debating ground of congresses, and from subjection to the opinions of university professors. The last International Conference on Electrical Units and Standards, which met in London in 1908, and consisted of Government delegates only, did little more than register the definitions of the International units; and though in the exercise of its collective wisdom it decided by a majority vote to affix two zeros to the figure of 106·3, which represents in mercury units the value of the ohm—a figure known to be almost certainly inaccurate by about one part in five thousand—it left a loop-hole for escape by creating a per-

manent committee to deal with the matter when the great laboratories have made their final comparisons.

No more need be said about this long labour of years.* The purely scientific units may be regarded as for all practical purposes fixed by the concordance and co-operation of the great laboratories.

But if the question of the fixing of the units has thus been receding into the background, as a thing judged, other questions of a more commercial import have gradually been coming forward; and as these involve both national and international issues they have needed a new organization to deal with them. Reference was made at the outset to differences of language and usage in the description and specification of apparatus and machines. With the growth of the heavy electrical industries these differences have become more serious. Electrical machines and appliances have become so essential a feature in every engineering project that international agreement as to the exact meanings of technical terms, the rating of machinery, and the general methods of testing and specifying electric machinery, is becoming vitally important. The scientific foundation of the electrical industry, as Mr. Arthur Balfour once said, is common to the whole civilized world; yet the actual terminology employed has often very different meanings in different language. Let a simple example suffice.

The word *dynamo*, spelled in identically the same way, is in current use in English, French, German, and several other languages, but does not mean the same thing in them all. In England, though usage varies somewhat, the term usually means a continuous-current generator, and does not include any alternator or any motor. In France it is used as the abbreviation of the full term *machine dynamoélectrique*, and includes any revolving electrical generator, alternator, motor, or converter. In Germany, where the word first came into any extensive employment, it means any generator, whether of continuous or alternating currents, but does not include motors or converters. In France a *convertisseur* means what we call a *motor-generator* (group machine), while what we in England call a *converter* is a *commutatrice*; and the French term *moteur-générateur* has been generally used for what we Britons call a continuous-current transformer.

It ought not to be beyond the bounds of reasonable possibility for some mutual understanding to be reached on matters of definition of this sort. Personally I would be quite willing to alter my usage of the word *dynamo* and accept either the German or the French usage, provided that either our French friends would adopt the German usage or our German friends the French usage. As regards the series of terms for those machines which convert currents or voltages in various ways, I prefer logically to keep the group-word *motor-generator* for the group-machine, the hyphen between the constituent words corresponding to the coupling between the constituent machines; and I am not without hopes that our friends across the Channel, who in all matters of lan-

* The admirable work by the Comte de Baillehache, *Unités Electriques*, published in 1909, gives a very complete account of the whole question of units.

guage are usually so much more logical than we, will recognize the preference. A unification in nomenclature is the more essential because the mischief that arises in disputes about specifications and contracts is almost invariably due to looseness of language and want of precision in definitions. In no branch is this of more importance than in the specification of machinery.

It would undoubtedly be of great advantage to an engineer if he were able to draft his specifications in terms which were practically identical with those in use, not only in his own country, but also in other countries in which similar apparatus is employed. What an amount of misunderstanding would be avoided if this were made possible!

At the present time a motor correctly described as a 10-kw. motor in one country is not necessarily a 10-kw. motor in another country, because the usages and regulations as to rating and the requirements as to the physical tests which are made to determine the normal output of the motor differ in different countries. Surely the same tests should be required and the same temperature-rises should be named as permissible in the determination of the normal rating, in whatever country the machine is constructed; otherwise the intending purchaser who is offered, by rivals in trade, machines of nominally the same output has no guarantee that the competition is a fair one.

Thirdly, there is at present a great divergence of practice in the use of symbols employed to represent the various electrical quantities, leading to a most annoying waste of time and energy. Not only does the usage of symbols vary between one country and another, but within one country—and this is unfortunately more flagrant in Great Britain than in any other country—it varies between individual writers. There are scarcely two textbooks which employ the same symbols, and even in the pages of the *Journal* of our Institution there is no concordance between the symbols used by the writer of one paper and the writer of another. The Papers Committee, it is true, attempts from time to time to suggest to the authors of papers that their nomenclature is irregular, that their symbols are unconventional. But it has no power to compel uniformity, and it is doubtful whether such power of compulsion would be entirely beneficial.

Now it is well known that these various problems have received much study and attention. The Americans, however, were the first seriously to consider the question of the classification of electrical machinery, and the American Institute of Electrical Engineers adopted a report of a Committee under the chairmanship of Dr. Francis B. Crocker in 1899. Gradually several other countries followed suit and drew up reports which have been of undoubted assistance to the industry adopting such reports. Again, the German organization of engineers and professional men of the German-speaking nations—known as the AEF.*—has for the last dozen or more years been grappling with the whole subject of symbols. It is no small advantage to the I.E.C. that it has now obtained the co-operation of this powerful

* Ausschuss für Einheiten und Formelgrössen.

body, which is represented with us by Geheimrat Oberpostrat Professor Dr. K. Strecker.

In 1901, under the auspices of the Institution of Civil Engineers, the British Engineering Standards Committee was formed, Sir William Preece, K.C.B., and Colonel R. E. Crompton, C.B., being nominated as the representatives of the British Institution of Electrical Engineers on that body. The excellent work of that Committee is well known and need not here be referred to in detail.

But the time was ripe for some much more determined effort, and one which should not depend on sporadic efforts at Congresses, but which should possess a fully organized existence. And that more complete organization has come about in the following manner.

At the St. Louis International Congress of September, 1904, the delegates of the British Government to the Chamber of Delegates, the official inner circle of the Congress, were Colonel Crompton, Professor Perry, and Dr. Glazebrook. Mr. R. Kaye Gray and Mr. W. Duddell, though not Government delegates, were present representing this Institution, as were others also.

Mr. Duddell's report on the work of the St. Louis Congress is to be found in our *Journal*.* At St. Louis a paper was read by Colonel Crompton on the Standardization of Dynamo-electric Machinery and Apparatus, a topic on which none could speak with greater competence than he. It gave rise to much discussion. Many delegates felt that the time had arrived for considering these various problems internationally, and that if international co-operation on a proper and permanent basis could be secured, success would be bound to follow. It was quite recognized that the various Congresses held from time to time were of too short duration to allow of the different problems submitted to them being studied to any depth, and consequently the Chamber of Delegates passed a unanimous resolution proposing the formation of an International Commission with an organization capable of giving the continuous effort so absolutely necessary for the solution of these and kindred problems.

The actual resolution is as follows :—

> "That steps should be taken to secure the co-operation of the technical Societies of the world by the appointment of a representative Commission to consider the question of the Standardization of the Nomenclature and Ratings of Electrical Apparatus and Machinery."

Accordingly Colonel Crompton, after his return from St. Louis, put himself in communication with the Institution of Civil Engineers, and after various tentative moves it was decided to entrust the work of preliminary organization to the Institution of Electrical Engineers, who called the delegates of fourteen countries together in June, 1906, under the presidency of Mr. Alexander Siemens. Lord Kelvin was elected first President, and Colonel Crompton Honorary Secretary. An account of

* *Journal of the Institution of Electrical Engineers*, vol. 34, p. 171, 1905.

this preliminary work of organization is to be found in the Presidential Address of Dr. Glazebrook.*

The idea underlying the organization of the International Electrotechnical Commission is that in each country there shall be formed, either by the recognized Electrotechnical Society of that country, or by Government, a local Electrotechnical Committee. Each country possessing an Electrotechnical Committee has equal rights with every other: each pays the same annual contribution to the expenses of the central body; each has equal voting power at those gatherings where the Commission meets as a whole. The statutes drawn up at the preliminary meeting in 1906 in London, and ratified at the first meeting of the Commission itself at London in 1908, provide for the election and rotation of representatives, for the location of the Central Bureau in London, and for the election of a President and Vice-Presidents.

The reference drawn up at St. Louis, U.S.A., in 1904, was necessarily made very broad, so as to permit the Commission, if and when formed, to develop freely. Moreover, at that date it was extremely difficult to put forward any definite outline of the route to be travelled.

In 1906 the idea of equal vote and equal taxation was vigorously supported by the Americans and British at the preliminary meeting, though some delegates leant towards a *pro rata* taxation. Rotation of office is provided for by annual election; though, of course, if for any reason the regular election does not take place, then the officers hold office until their successors have been appointed. Perfect liberty is allowed to each Electrotechnical Committee in the management of its own affairs, the only restriction being that its rules must not be incompatible with those of the Commission. As a general rule the decisions of any International Commission should practically be unanimous. It is a good thing not to attempt, in the charged atmosphere of an international meeting, to coerce a minority; for if they are allowed a certain time for calm consideration they may fall in with the desires of the majority, which otherwise they will not do; and at a future meeting the same proposal coming forward may pass without any opposition. But to avoid the possibility of deliberate obstruction the decisions of the I.E.C. to be officially adopted must receive a majority of at least four-fifths of the countries voting, each country having only one vote, whatever number of delegates it may appoint. The official languages are English and French, in which tongues all debates are held and all resolutions officially expressed.

To prevent the work falling into the hands of any clique, the statutes provide means for calling a meeting by the President or, in the case of his inability to act, by two Vice-Presidents (*i.e.*, the Presidents of any two National Committees). If the Vice-Presidents do not or will not act, any three National Committees may request the Central Office to call such a meeting, and the date must be fixed by the Central Office within three months of receiving the request.

* *Journal of the Institution of Electrical Engineers*, vol. 38, p 4, 1907.

The Council which manages the affairs of the I.E.C. consists of the President, the Presidents of the National Committees, one delegate from each National Committee, and the Honorary Secretary.

The drawing up of these statutes, submitted in draft by the Institution of Electrical Engineers, occupied the whole of the time of the delegates at the preliminary meeting, at which our old friends Mr. C. O. Mailloux and Dr. F. B. Crocker took a leading part. At the final meeting they were adopted subject to subsequent ratification by the National Committees, and this was forthcoming in 1908.

The real business of the International Electrotechnical Commission began when a meeting of the Council of the Commission took place on the very site of the present hall, in 1908. The British Electrotechnical Committee that year had Sir John Gavey as its President, and as Vice-Presidents Sir William Preece and Mr. Alex. Siemens; the delegates whom it sent to the Council were Sir John Gavey and Dr. Glazebrook.* The foreign delegates, representing fifteen countries, were welcomed by the Rt. Hon. Mr. Arthur J. Balfour.

Lord Kelvin had died in December, 1907, and M. Mascart, who, if he had survived, would have been called in succession to the presidential chair, had passed away in August, 1908. As a new President the Council chose Professor Elihu Thomson, from whom, in September, 1911, the presidency passed to its present occupant, Professor Dr. Emil Budde. By choosing its presidents from the various nations the Commission has found great advantage to accrue, as stimulating the sense of national co-operation in a work of international importance. Most of the time of this Council meeting was taken up with consideration of the constitution of the organization and its procedure; but a certain amount of technical discussion took place on units of candle-power, on nomenclature, and on symbols. Here the first suggestion was made for a compromise with respect to the symbols used to represent Ohm's law. At this Council meeting, as at several other meetings of the I.E.C., the unusual linguistic powers of Mr. Mailloux, who acted as voluntary interpreter, proved of the utmost service.

The next event was an unofficial conference held in Brussels in August, 1910, when the delegates were received by the President of the Belgian Committee, Professor Eric Gerard, one of the Secretaries of the Paris Congress of 1881. Here the question of symbols was raised by Dr. Kennelly and debated by M. Boucherot and others. M. Boucherot hinted at the probability of the French accepting C for current instead of I, if the Germans would be willing to abandon W in favour of R for resistance. The French delegates, following on the lines of the late M. Hospitalier, put forward certain basic rules for the use of letters as symbols, which were, after ardent debate, referred for consideration to the various national committees. It was universally agreed that it would be impossible to put forward any

* For whom Professor Silvanus P. Thompson acted as substitute during most of the proceedings, owing to Dr. Glazebrook's engagements at the Conference on Electrical Units.

long list of symbols which would radically subvert the usage of many years, and that the only hope for international agreement on this matter would be to propose the adoption of a few general rules, and of a very restricted list of the symbols for the principal electrical quantities. In the matter of nomenclature Dr. Budde made two excellent suggestions: one was to narrow the range of discussion from electrical terms in general to a short selected list of the terms relating to electrical machinery only, which he submitted; the other was that the British, French, and German Committees, each should choose one delegate to meet and compare lists of the English, French, and German equivalents for these terms. Here also the first attempt was made to secure a discussion on the question of international rating of machinery. Here, too, the question was raised by Dr. Kennelly, and a resolution was adopted that: "Should it be desirable at any time to introduce a new name for an electrical unit, the name of Lord Kelvin should be brought forward." On this it may be remarked that so far back as 1892 the Board of Trade officially proposed the name "Kelvin" for the kilo-watt-hour. The personal objection which the modesty of Lord Kelvin raised at the time is no longer valid. We shall all agree in this: Such a name, if adopted for any unit at all, must be adopted by international and not merely local consent. Incidentally it may be mentioned that our German colleagues desire to put forward the name of *Siemens* for the unit of conductance.

The small committee named to discuss international nomenclature met duly at Cologne in May, 1911. Dr. Budde came from Berlin, M. Brunswick from Paris, and the writer had the honour and responsibility of representing the British Committee; while M. Eric Gerard, who had presided at Brussels, was invited to take the chair. The first-fruits of mutual co-operation were here harvested in the acceptance by all parties of the symbols E, R, I for the three principal quantities of Ohm's law, the Germans abandoning the use of W for resistance, and the English abandoning the use of C for current. This proposition was ratified at Turin some months later by the Commission as a whole. It is understood that the National Electric Light Association of America, which up till now has used C instead of I, does not resent the change. For the rest, the work of the small committee proceeded then, and has continued at Paris, with uninterrupted harmony, though divergencies of nomenclature innate in the structure of these three languages are by no means easy to adjust.

It should here be remarked that a very large amount of work in the preparation of vocabularies has been accomplished in several directions. The British Nomenclature Committee under Mr. A. P. Trotter has been slowly compiling a long list of English terms. The French Committee presented at Turin a very complete and carefully considered Vocabulary, the result of the labours of M. Brunswick, M. Guillaume, M. Boucherot, and others, which has as yet only been partially compared with the lists of corresponding terms of other languages. The Danish Committee, through its representative,

Professor Absalon Larsen, has done excellent work on nomenclature, even though they have been handicapped by the infrequency of the use in that country of French and English terms. In revising our own English vocabulary we have willingly availed ourselves in several instances of improved modes of expression suggested by the Danish Committee. The German list of preliminary terms has already been mentioned, as having been presented by Dr. Budde. The Austrian Electrotechnische Verein has also presented a short list of terms and definitions. Italy, Japan, Mexico, and Switzerland have also contributed vocabularies which have been consulted, and which have proved of great assistance to the workers.

Through the courtesy of the German Committee, the first plenary meeting, originally arranged to be held in Berlin, took place at Turin, Italy, in September, 1911, at the same time as the International Congress of the Applications of Electricity and the Universal Exhibition celebrating the first fifty years of Italian autonomy. This Congress was organized by the Italian Electrical Society with the aid of the Italian Electrotechnical Committee, and was supported by the various local committees of the I.E.C. The Committee of Honour was under the patronage of H.R.H. the Duke of the Abruzzi, and included twenty-eight members, amongst whom were Professor Elihu Thomson and Colonel Crompton. The membership numbered about five hundred, including many Government representatives and delegates from various technical societies.

Fifty-six delegates from nineteen different nations attended the meetings of the I.E.C., and for the first time in the history of the movement the Governments of Great Britain, British India, and the United States of America were officially represented. The suggestions put forward at the Brussels Conference, which had been carefully considered by the various Local Committees, were submitted to the meeting. The official decisions have been published in the *résumé* of the meeting, October, 1911, and also in the full report of the Turin Meeting. They include a decision as to the previously disputed question of the direction to be considered positive in the rotation of vectors.

Although the technical work occupied most of the hours of daylight, yet the Italian Committee spared no pains, and indeed succeeded admirably in making the visit one of social pleasure as well as of technical accomplishment. The cordial co-operation which characterized the meeting to a marked degree is certain to be reflected in the work of the special committees appointed on that occasion.

Of special interest was the resolution, unanimously adopted at a plenary meeting of the Congress, requesting the I.E.C. to accept the task of organizing future Electrotechnical Congresses in regard to the date, the place of meeting, and the object.

The Local Committees of the I.E.C. have all signified their adherence to the resolution adopted by the Council in regard to this formal request of the Congress, and the next International Electrotechnical

Congress, to be held in San Francisco in 1915, has been authorized by the I.E.C.

The First Annual Report of the Commission was published in March, 1910, and the Second in April, 1911, and several detailed reports have from time to time been issued in French and English, the official languages of the Commission.

The Third Annual Report, issued in June, 1912, briefly recapitulates the different events which have led up to the present situation, and chronicles much practical progress.

Since the issue of that Report three of the small committees appointed at Turin have met in Paris. The Symbols Committee, of which a special report has been issued, was presided over by Professor Paul Janet, Director of the École Supérieure d'Electricité, and was attended on behalf of the British Committee by Mr. L. A. Legros, whose peculiar and intimate knowledge of printing machinery proved very useful. The Nomenclature Committee, attended by Dr. Budde, M. Brunswick, Professor Larsen, Professor M. Ascoli, and the writer, continued the discussion of the vocabulary. The Committee on Rating, presided over by M. Boucherot, at which Mr. H. W. Miller attended as delegate, spent three days from 9 a.m. to 7 p.m. in a strenuous debate which has laid the foundation of probable international rules for the rating of electrical machinery and apparatus. As far as Great Britain is concerned in this subject, the co-operation of our Engineering Standards Committee is proving most valuable. Further, the co-operation of the newly founded British Electric and Allied Manufacturers' Association has opened a new avenue for winning the adhesion of British manufacturers as a whole.

Through the kindness of the German Committee, the next official meeting is to be held in Berlin in 1913, when the delegates are sure to be present in large numbers to do honour to their new President, to whom the first practical success of the Commission is so largely due. Moreover, the hospitality and courtesy of the Germans is so well known to electricians of all nations that much social pleasure may assuredly be anticipated.

Hitherto the official documents of the Commission issued by the Central Office here in London have been printed only in the two official languages, French and English, but the time would now seem to be ripe for attempting in the different countries to reach the wider field of those electricians who are not linguists, as the delegates necessarily must be. It is my personal hope that the efforts to introduce German and Spanish as translations of the official languages may be acceptable; English and French being still adhered to as the media for verbal discussions.

The internal organization of the Commission, and its *modus operandi*, is well illustrated by the two diagrams, Fig. 1 and Fig. 2, which I owe to the kindness of the General Secretary of the Commission, Mr. le Maistre, whose assistance in the preparation of this narrative I take this opportunity of acknowledging with gratitude.

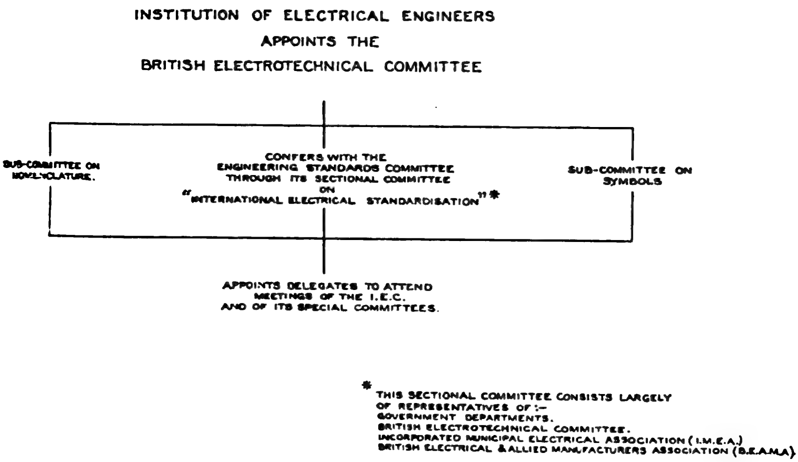
INSTITUTION OF ELECTRICAL ENGINEERS
APPOINTS THE
BRITISH ELECTROTECHNICAL COMMITTEE
SUB-COMMITTEE ON NOMENCLATURE.
CONFERS WITH THE ENGINEERING STANDARDS COMMITTEE THROUGH ITS SECTIONAL COMMITTEE ON "INTERNATIONAL ELECTRICAL STANDARDISATION"*
SUB-COMMITTEE ON SYMBOLS
APPOINTS DELEGATES TO ATTEND MEETINGS OF THE I.E.C. AND OF ITS SPECIAL COMMITTEES.
* THIS SECTIONAL COMMITTEE CONSISTS LARGELY OF REPRESENTATIVES OF:—
GOVERNMENT DEPARTMENTS.
BRITISH ELECTROTECHNICAL COMMITTEE.
INCORPORATED MUNICIPAL ELECTRICAL ASSOCIATION (I.M.E.A.)
BRITISH ELECTRICAL & ALLIED MANUFACTURERS ASSOCIATION (B.E.A.M.A.)

FIG. I.

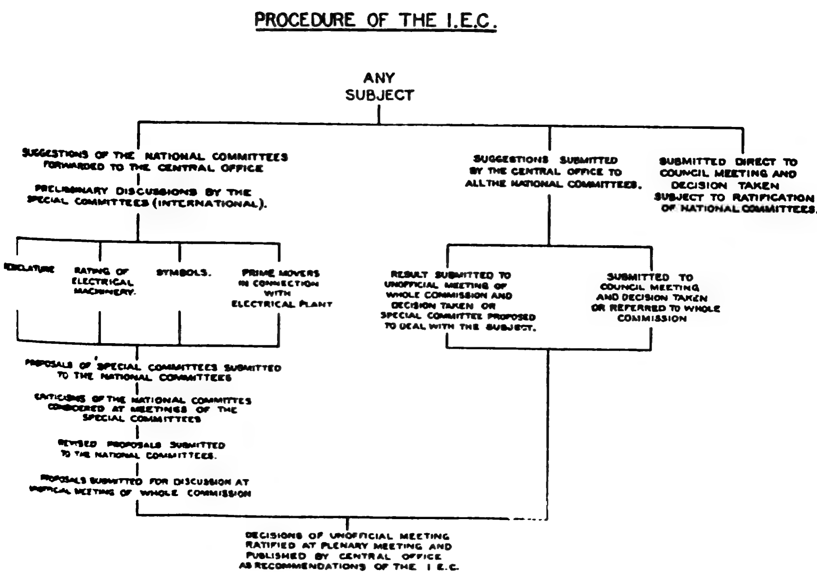
PROCEDURE OF THE I.E.C.
ANY SUBJECT
SUGGESTIONS OF THE NATIONAL COMMITTEES FORWARDED TO THE CENTRAL OFFICE
PRELIMINARY DISCUSSIONS BY THE SPECIAL COMMITTEES (INTERNATIONAL).
RATING OF ELECTRICAL MACHINERY.
SYMBOLS.
PRIME MOVERS IN CONNECTION WITH ELECTRICAL PLANT
PROPOSALS OF SPECIAL COMMITTEES SUBMITTED TO THE NATIONAL COMMITTEES
CRITICISMS OF THE NATIONAL COMMITTES CONSIDERED AT MEETINGS OF THE SPECIAL COMMITTEES
REVISED PROPOSALS SUBMITTED TO THE NATIONAL COMMITTEES.
PROPOSALS SUBMITTED FOR DISCUSSION AT UNOFFICIAL MEETING OF WHOLE COMMISSION
SUGGESTIONS SUBMITTED BY THE CENTRAL OFFICE TO ALL THE NATIONAL COMMITTEES.
SUBMITTED DIRECT TO COUNCIL MEETING AND DECISION TAKEN SUBJECT TO RATIFICATION OF NATIONAL COMMITTEES.
RESULT SUBMITTED TO UNOFFICIAL MEETING OF WHOLE COMMISSION AND DECISION TAKEN OR SPECIAL COMMITTEE PROPOSED TO DEAL WITH THE SUBJECT.
SUBMITTED TO COUNCIL MEETING AND DECISION TAKEN OR REFERRED TO WHOLE COMMISSION
DECISIONS OF UNOFFICIAL MEETING RATIFIED AT PLENARY MEETING AND PUBLISHED BY CENTRAL OFFICE AS RECOMMENDATIONS OF THE I.E.C.

FIG. 2.

The Electrotechnical Committees have a large measure of Governmental support, and the various Government representatives are of great assistance to the work; moreover, the Government support gives a standing, in their own country, to the various Committees.

The British Indian Government is aiding the Commission financially, though without forming a Committee.

Twenty-two countries are now affiliated to the movement. Considerable interest is also being evinced in the work in Argentina, Australia, China, Ecuador, New Zealand, Panama, Peru, Portugal, Roumania, Siam, and South Africa; and it is certain that, at least in some of these countries, an Electrotechnical Committee will be formed at no distant date.

It cannot but be admitted that the generous support which the I.E.C. is receiving in the various countries, and the cordial way in which the delegates are conducting their deliberations, is bound to lead to useful and practical results. This promotion of a better understanding between electricians of various nations by general agreement as to terminology, symbols, and the classification of electrical machinery, is sure to foster the free development of international trade and to be a general benefit both to the purchaser and to the maker. Last, but by no means least, these regular international gatherings, during which national prejudices are laid aside, and at which many lasting friendships are made between electricians of different nationalities, must undoubtedly be a not unimportant factor in furthering the peace of the world.

Discussion.

Colonel Crompton.

Colonel R. E. B. Crompton: I think we Englishmen may pride ourselves that the very substantial realization of a dream of electrical peace throughout the world has so far been carried out from the Central Office in London. There can be no doubt that the countries which have joined the Commission have already gained greatly from getting into touch with one another on electrical matters, for we have begun to have mutual understandings and to be sympathetic. The friendships we have formed cannot fail to lead to the good of the world, and to help forward the cause of science and engineering. The Commission has been very fortunate in its choice of a Secretary, Mr. Le Maistre, who has shown such patience and good temper when dealing with the knotty questions which sometimes seem insoluble, that nation after nation has been brought round to work harmoniously together, although at first some of them were rather intractable. My work has been rendered easy by the work of the Secretary. I think that between us we have developed a real science of making progress at each meeting of international committees. The success has been due mainly to perseverance and to our always supposing that the man who appeared to obstruct was really actuated by the best motives, and that all that was necessary was to make it clear to him

that we appreciate and respect his conscientious objections. Many of these objectors, who at first we thought were objecting for objection's sake and who were delaying our work, have turned out to be our best and most loyal helpers when once they understood the difficulties of the position. I should like to echo what Professor Thompson has said, that the efforts of the International Electrotechnical Commission, by bringing into harmonious working the scientific minds of the world, will be a very potent force for universal peace in other directions. Colonel Crompton.

Professor MILES WALKER: Everyone connected with electrical engineering must recognize the useful work of the International Electrotechnical Commission. In particular their efforts to formulate rules by which the ratings of electrical machinery may be specified in a uniform manner by all manufacturers will result in decisions of the greatest value. The work of the Committees upon Nomenclature must necessarily be very tedious, but I cannot help thinking that it would be very much simplified if they would follow the methods of one of the technical dictionaries in which the words are grouped in a convenient order and illustrated by little sketches. Why not go through such a dictionary word for word, amending and amplifying it where necessary? Then we should hope to have in the course of time a technical dictionary of a much improved kind, in which every word had the authority of the Commission behind it, instead of what is promised now—an incomplete list of words compiled with an enormous amount of independent effort, but without any sketches to show exactly what each word means. Professor Walker.

There is just one matter that I should like to refer to in connection with the symbols which denote mathematical operations. Now that we have an International Commission, it seems a pity that we should not make use of the opportunity of improving our mathematical notation for the benefit of those who come after us. Everyone must admit that each mathematical operation, such as multiplication, extraction of square root, differentiation, or the turning of a vector quantity through a right angle, should have a distinctive symbol and should not be denoted by a letter of the alphabet, because letters of the alphabet denote quantities. It causes confusion in the student's mind to use the same sort of symbol for a mathematical operation as for a quantity. The symbol $\dot{x}$ is very much better than the symbol $\frac{dx}{dt}$. There are not so many different mathematical operations as to make the invention of new symbols, for those that now have no proper symbols, difficult. I do hope that the International Electrotechnical Commission will not fail to make some effort in this direction.

Mr. R. HAMMOND: Prior to this meeting I added up the various sums which the Institution has paid from time to time as its contribution to the expenses of the Electrotechnical Commission and its British Committee; they amount to £1,043. I came here to-night particularly to hear what we had got for that money. I am thoroughly in accord with Colonel Crompton and Professor Thompson as to the immense advan- Mr. Hammond.

Mr. Hammond.

tage of international accord; but I feel that in addition we should have from time to time a report of their stewardship. We should be told that the International Electrotechnical Commission have decided upon certain things, which through the President and through this Institution should become binding upon British electrical engineers. Professor Miles Walker seems to be under the impression that the Nomenclature Committee have only concerned themselves with a few words. This, however, is not the case. As a matter of fact the Committee have settled no words yet, but they have drafted meanings to about thirty pages of words. Before they issue them as their final suggestions the Committee are going to subject them to further careful revision.

To illustrate how important the work is, I will instance the expression "Diversity Factor"—the number obtained by dividing the sum of the maximum loads of the individual consumers supplied from any works during a given period by the maximum load delivered from the works during the same period—a most important word from the point of view of those who are designing works. That is the English definition. In the terminology of the National Electric Light Association of America, however, I find diversity factor described as "The ratio between the simultaneous demand of a number of individual services for a specified period, and the sum of the individual maximum demands of those services for the same period." That is to say, if we find that plant of 54,000 kw. capacity can supply 81,000 kw. of demand on consumers' premises, we should call the diversity factor 81,000 ÷ 54,000, or 1·5, whereas the National Electric Light Association definition gives 54,000 ÷ 81,000, or 0·66. I consider that our definition is very appropriate, because under it the product of the diversity factor and the consumers' load factor gives the actual load factor upon the works. Also when I am talking of improving the diversity factor I want to increase the figure, not reduce it. I have recently read Mr. S. Insull's paper on "The Relation of Central Station Generation to Railway Electrification," which appeared a month or two ago in the *Proceedings of the American Institute of Electrical Engineers*, and I find that he uses the expression "diversity factor" to mean the percentage by which the sum of the individual maximum loads of the various undertakings exceeds the maximum demands for electrical energy on the combined undertaking. Instead, therefore, of talking of a diversity factor of 1·5 he would refer to the diversity factor being 50 per cent. It is the province of the International Electrotechnical Commission to formulate a meaning for diversity factor and to enforce it upon the world.

The work of the International Electrotechnical Commission is not to endeavour to get all electrical engineers throughout the world to use English words, but to get all electrical engineers to describe the same piece of apparatus or the same phenomenon in the same terms. I should like to draw attention to one word which has appeared in the English nomenclature, and to which no previous reference has

been made to-night. The word is "*Kelvin*"; "a term officially proposed and authorized by the Board of Trade in May, 1892, but which has not come into general use, for a kilowatt-hour." I want this Institution and all English-speaking engineers to make that word come into common use. It is for us to adopt the term with the hope that the world may follow. It is important that we should at once come to a decision upon this point, because I notice that in the *Vocabulaire Électrotechnique* issued by the French Electrotechnical Committee, there appears "Kelvin, see Kelvin effect"; and the only word in which Lord Kelvin is brought into the French vocabulary is under "*Effet Kelvin ou pelliculaire*," that is, in a definition of "Skin Effect." Do not let us consign the memory of Lord Kelvin to the "skin effect" of a conductor carrying an alternating current; but just as we have Volta and Ampere commemorated in the description given by Parliament in their Provisional Orders, let us adopt "Kelvin" as a most appropriate word for describing the useful conjunction of the two as energy. Mr. Hammond

The Model Provisional Order of the Board of Trade which I have in my hand says: "The expression 'unit' shall mean the energy contained in a current of 1,000 amperes flowing under an electromotive force of one volt during one hour"—an absolutely incorrect description. I hope that in redrafting this definition the Board of Trade will say that the unit shall mean "the amount of energy that would be consumed which is equivalent to." In any case, let us abolish from our vocabulary such a stupid word as "unit." I heard a man once say: "I fully expect next year that our output will be increased by half a million units, and I shall have to put down another unit, and I am thinking of a unit of 1,000 units capacity." I am sure you will heartily agree with me that, while we have the chance, Lord Kelvin's name should be adopted as descriptive of so important a term in connection with electrical power. Lord Kelvin was approached on the subject by the Board of Trade in 1892, and it was only his natural modesty that induced him to say, "I hardly like the honour." I do not think we can do a greater honour to Lord Kelvin than to adopt the word "Kelvin" as representing the one thing which would carry his name down to posterity in a more emphatic way than linking it with any other unit.

Mr. A. P. TROTTER: The work which is being done by the Nomenclature Sub-committee at first is merely to try to record the accepted usage of words among British engineers. The use of some few words is discouraged—such as those words that have become obsolete. After the terms have been gone through once, the next step will be to revise the work carefully, and I hope that more suggestions will be received than have been sent in already in regard to the published lists of what has been tentatively settled. Then the more difficult task will have to be undertaken of putting them into international shape. It is hardly possible, I think, to secure complete unanimity in the connotation of similar terms, but a list will probably be selected upon which most nations can agree. The difficulties are really surprising. For example, Mr. Trotter.

Mr. Trotter. we find words in one country which have no corresponding term at all in another. It is not easy to define "Rating" for a Frenchman who has no such word in his language, and who has not got even "input" or "output." We, as you know, have no equivalent for *entre-fer*; "air-gap" is a very poor equivalent. So that it will very likely be necessary to borrow. It is surprising what help the different countries can afford to each other. When a short vocabulary in the Danish and English languages was placed in the hands of the Sub-committee they decided without hesitation to delete some of their English definitions and to accept the English translation of the Danish definitions. America is holding her hand for the present. The Sub-committee have been in consultation with Dr. Kennelly, and they think there will be very few, if any, divergencies of any importance, so that there will be no branching off between the two languages. There is just one other matter I should like to refer to, and that is the Kelvin. Twenty years ago Sir Courtenay Boyle, guided by intimate knowledge of the electrical industry and keen interest in all that concerned engineering and science, after carefully weighing the pros and cons of the matter, obtained the consent of the President of the Board of Trade at that time to use the term "Kelvin" in all Provisional Orders instead of "Unit." The President cordially approved, and the decision was communicated to the electrical Press of the day. But a fortnight afterwards Lord Kelvin, on grounds of mere modesty, desired that it should be withdrawn. I have discussed the matter with a great many different people, and I know only two in this country who have any objection to it; their objections were on the ground that it might not be acceptable to our friends on the other side of the Channel. If that difficulty could be removed no one need wait for any authority to use the term. It has already been officially sanctioned, and it can be used if desired.

Mr. Baker. Mr. C. A. Baker (*communicated*): It occurs to me that in adopting foreign words for use in this country, or by asking foreign countries to adopt English words, there are considerable difficulties in selecting words of such simple construction that they may be easily adapted to the composition employed in our own or other countries. For example, German and Russian words are subject to declension; also there are the important considerations of prefixes, suffixes, and hyphened combinations capable of affirming, negativing, or qualifying the original word to an extent that must vary with different languages; the words also must be pliable in regard to variations to meet the singular and plural meanings arrived at by completely different processes; and again, the sex value comes in as a complication in other languages that scarcely exists in the English language. Those who have had occasion to spend some years in different Continental countries become thoroughly impressed with the difficulties introduced by the Babylonic effect of varying speech, and the extreme difficulty of producing an exact translation from one language to another. Much more difficult must it be to add a word intended to be used with equivalent value in several languages.

Mr. Baker.

The proposal to adopt the word "Kelvin" must be most gratifying to British engineers, and generally to engineers all over the world. This again, however, is an example where the questions of spelling and pronunciation are evident. In Southern Europe the letter "k" hardly forms part of the alphabet, although in French it is accepted in words of foreign origin. The proposition is, however, so meritorious that it might perhaps be considered desirable for the Institution to hold an informal meeting of members, on the lines of that held last session, to discuss the advisability of adopting and promoting the use of the term "Kelvin."

Dr. Silvanus Thompson.

Dr. SILVANUS P. THOMPSON (*in reply*): With regard to Professor Miles Walker's criticisms I would it were possible to abandon the use of actual letters as symbols of operation, and instead of writing "log" in front of a number or an algebraic symbol to tell us we have to take the logarithm, to invent a new symbol which is not a letter at all but which means that we should take the logarithm. Such a symbol we have in the sign for taking square root. But we should have to have a name that could be read: that is, it should have a name. Such symbols, however, except in a very few cases, have not yet been invented, and it really rests with mathematicians to invent them. There is one principle that they must necessarily conform with, namely, they must be capable of being printed, and of being printed by a type-setting machine. In these days of machine type-setting all mathematical printing is bound to be reformed sooner or later, because the expense of the hand-setting of displayed mathematical symbols will be very great compared with that of ordinary letterpress, simply because the symbols do not run on all in one straight line. Anything we can do towards the simplification of mathematical printing by making the formulæ run along one straight line, instead of up and down over the page, will be to the good of future mathematics.

The Commission are not entrusted with the making of new words. The electrical Press can help us immensely—they will help us I have no doubt—but they will not assist us if we decide to do things which are premature or violent in the usages of language. I am quite certain that those who are in the industry may help us greatly here, but will they support such authority as we already have?

Will Professor Miles Walker and the manufacturers replace *direct current* by *continuous current* and write C.C. instead of D.C. in the catalogues of their machines? That is what we want. Have we authority to compel that? Has the editor of the *Journal* of this Institution the authority, whenever a paper comes in with the word "direct" current in it, to replace it by "continuous" current, or to substitute C.C. instead of D.C.? That is the authority we want, and which we think we have.

As regards the technological dictionary, at our very first sitting we had that matter before us; and we found it to be absolutely hopeless simply to take a commercial dictionary and to go through it word by word. When one term is being considered it cannot be considered

Dr. Silvanus Thompson.

alone. The related terms have to be considered ; and we found it much better to take them in groups, to group the words, and settle them together rather than to deal with isolated words in alphabetical order. We have now had a good deal of practice, and the work is going on much quicker than at first.

Mr. Hammond will find the decisions come to at Turin not only in the Third Annual Report and in the Special Report of the Turin Congress, but he will find them officially reported in the *Journal* of the Institution, and they were mentioned in the last Annual Report of the Council.

Proceedings of the Five Hundred and Forty-seventh Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 9th January, 1913 —Mr. W. DUDDELL, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 19th December, 1912, were taken as read, and confirmed.

The list of candidates for election and transfer approved by the Council for ballot was taken as read, and it was ordered that it should be suspended in the Hall.

Messrs. F. T. Hall and R. W. Weekes were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows :—

ELECTIONS.

As Graduates.

William Collier Bexay.
William Brodie.
Charles Edward Covell.
Bertie Drane.
Harold George Furlong.
Lewis Bradford Giles.
Fred Hicks Jennings.
Israel Ernest Poyser.

As Students.

Adelmar Alves.
Abd El Gowad Ammar.
Stuart Gordon Anderson.
Alfredo Arias.
Grahame George Dawson.
Vincent Gerard S. Z. de Ferranti.
Ernesto Alves do Rio.
William Oliver Fenwick.
George Victor Ross Fraser.
Ronald Gray.
Ernest Alfred Guthrie.
George Haigh.
Arthur Tyler Hitch.
John Cyril Holden.
Douglas Harold Jaques.
Walter Whitworth Keen.
William Hutton Parker Kerman.
Daniel James McCourt.
Gordon Spencer Marston.
Arpelices Morales.
K. Venkata Ramana.
William John Rawlings.
Benjamin Rowe.
Olga Whately Sherwell.
Moses Singer.
Edmundo Wolf.

TRANSFERS.

From the class of Students to that of Associate Members—

Frederick Edward Windross.

From the class of Students to that of Graduates—

Frank Bailey.
John Ernest Blair.
Charles Malcolm Gillies.
George Eagar Francis Graham.
Charles Bernard Gresham.
Gilbert Kelsey Loveday.
Francis Lewis Otter.
Nasserwanji Beramji B. Patel.
George Henry Noel Reay.
Nagendra Nath Roy, B.Sc.
Atma Singh Sarkaria.
Charles Wallace Saunders.
Georg Friedrich R. Schmidt, B.Sc.
William Charles Stewart.
Herbert Michael Theaker.
Conrad Vandermin, B.Sc.
Henry Percy Young.

Donations to the *Library* were announced as having been received since the last meeting from J. W. Barber, W. R. Barclay, A. E. Berriman, Professor H. Bohle, Messrs. W. T. Glover & Co., Ltd., Sir R. Hadfield, C. H. Hainsworth, W. G. Kent, Messrs. Mather and Platt, Ltd., S. G. Neiler, Messrs. Newton & Co., G. Quincke, S. Rentell, W. E. Schall, United States Bureau of Standards, and Dr. H. Wilde; and to the *Museum* from G. Stoney, K. Hedges, A. W. Isenthal, and Professor A. Jamieson, to whom the thanks of the meeting were duly accorded.

A paper by Professor Miles Walker, Member, entitled "The Design of Apparatus for improving the Power Factor of Alternating-current Systems" (see page 329), was read and discussed, and the meeting adjourned at 9.38 p.m.

THE DESIGN OF APPARATUS FOR IMPROVING THE POWER FACTOR OF ALTERNATING-CURRENT SYSTEMS.

By Professor MILES WALKER, Member.

(*Paper received* 8*th November*, 1912; *read before the* INSTITUTION 9*th January*, *and before the* BIRMINGHAM LOCAL SECTION, 8*th January*, 1913.)

It has been thought that it would be well to discuss at our Institution in London the whole subject of the improvement of the power factor of alternating-current systems. The present short paper may serve to open the discussion.

In January, 1909, the author read before the Manchester Local Section of this Institution a paper * which gave a short survey of the methods that had been suggested for improving the power factor, and he described some experiments with Leblanc's method. During the last few years several manufacturing firms have put upon the market apparatus for enabling induction motors to run at unity power factor, or even to take a leading current. It has been pointed out by Mr. W. M. Mordey that electric condensers can now be so cheaply manufactured, and are so efficient, that they can compete with the synchronous motor in cost and beat it in efficiency.† Moreover, Professor Kapp has recently described an entirely new kind of electric machine, a sort of dynamo-electric condenser, which has several features that commend it for the same kind of duty. ‡

To describe all the apparatus which has been invented and developed for this purpose and for kindred purposes during the last few years would be a very great task, and it is doubtful whether one author could do justice to these new machines, intimate acquaintance with any one of which can only be possessed by the particular designer who has developed it.

This paper will be confined to a short statement of the principles involved and a description of the phase advancer built by the British Westinghouse Company.

We may look at the cause of the lagging current in the following way: The energy stored in any magnetic field consists of two factors:

(*a*) The total flux;

(*b*) The magneto-motive force driving that flux.

* *Journal of the Institution of Electrical Engineers*, vol. 42, p. 599, 1909.

† Ibid., vol. 43, p. 618, 1909.

‡ *Electrician*, vol. 69, pp. 222 and 272, 1912.

Now the total flux when created at a certain frequency produces a certain back E.M.F. in each turn encircling it, and the magneto-motive force requires for its production a certain number of ampere-turns. Thus, from the two factors—flux and magneto-motive power—we arrive at certain factors necessary for the production of the alternating magnetic field at any given frequency. These factors are:—

1. Electromotive force per turn;
2. Number of turns;
3. Amperes per turn.

Grouping (1) and (2) together, we arrive again at two factors—volts and amperes—whose product represents the idle component of the power required to generate all the alternating fields in the distribution system.

The higher the frequency of the supply, the greater will be the idle component of the power required to produce the alternating field. If the field can be produced by a rotating magnet (as, for instance, in a synchronous motor) excited by continuous current, the frequency of the current being zero, no idle component is necessary. Indeed, by supplying more continuous current turns than are necessary to produce the magnetic field in any particular machine, it is possible to create in the system a leading current, which will compensate for a lagging current in another part of the system. The field current in the magnet of an alternator may be said to have two functions:—

1. To produce the magnetic field which generates the electromotive force in the alternator itself.
2. To supply an additional magneto-motive force, which by means of a wattless current is communicated to all the machines in the system that have not got continuous-current excitation themselves.

If we are to reduce the wattless current we must either use machinery requiring weak magnetic fields, or we must provide independent means of magnetizing the fields. A modern transformer does not take a large wattless component in proportion to its output, because though the total flux generated may be great, the magnetomotive force required to produce that flux is very small. An induction motor, however, having necessarily an air-gap and coils of considerable magnetic leakage, has in it a magnetic field which requires a wattless component of 25 to 30 per cent of the kilovolt-ampere rating of the motor. For this reason the induction motor is the main cause of low power factors on our alternating-current systems.

If now the magnetizing current of an induction motor can be supplied at the frequency of the slip (say, one cycle per second) instead of at the frequency of supply (say, 50 cycles per second), the wattless magnetizing kilovolt-amperes are very much reduced.

In 1895, M. Leblanc proposed to supply the magnetizing current to the rotors of induction motors and generators by means of special

exciters, which consisted of commutating alternating-current generators whose magnets were excited by the rotor currents.*

The method can be most easily followed when applied to a rotor wound for two phases. In Fig. 1 W_a and W_b represent the windings in two phases of the rotor. E_a and E_b are the armatures of the two exciters. These exciters may be made like single-phase series motors, provided with compensating windings. The current from the phase W_a passes from the collector ring through the compensating winding C_a, through the armature E_a, and then through the field F_b of the exciter in phase B, and thence to the star-point O. The current from phase W_b passes through the compensating winding C_b, through the

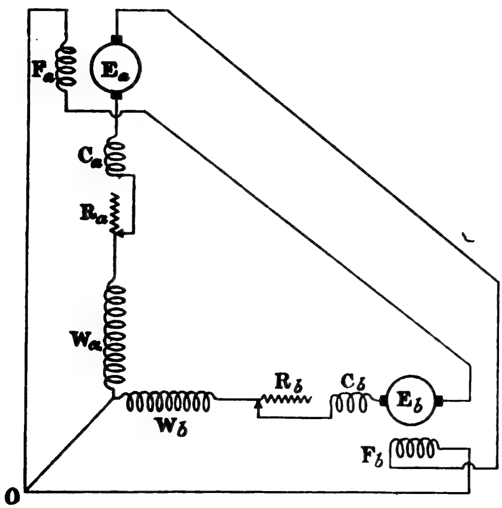

FIG. 1.—Leblanc's Exciters for Two-phase Rotor (1895).

armature E_b, and then through the field F_a of the exciter in phase A and to the star-point. The induction motor can be started up by means of the resistances R_a, R_b, the exciters being short-circuited. When the motor is running, the armature E_a has generated in it an E.M.F. which is in phase with the current in phase B; and if the polarity of the poles is properly arranged, this E.M.F. will be leading 90° on the current in phase A. Similarly the armature E_b can be made to supply an E.M.F. whose phase would lead by 90° on the current in phase B. This has the effect of making the currents in the rotor take up a phase in advance of the E.M.F. produced in the rotor; so that not only can the magnetic field of the induction motor be created by the rotor currents, but the current in the stator can be made to lead on the

* Patent specification, No. 15470 of 1895.

E.M.F. of the supply. The clock diagram, Fig. 2, shows the phase relations between the various currents and the E.M.F.'s. O W_a represents the E.M.F. generated in the rotor circuit by the slip. $W_a D_a$ represents the E.M.F. generated in the armature of the exciter. $D_a C_a$ represents the drop due to the impedance in various parts of the circuit, and C_a O represents the drop in the resistance of phase A of the rotor winding. The current in phase A has, of course, the phase position O C_a; thus it is leading on the E.M.F. O W_a, the clock diagram being supposed to be rotating in the direction indicated by the arrow. It will be seen that the line $W_a D_a$ is parallel to the line O C_b, because the current O C_b excites the armature in phase A. Similarly the E.M.F. $W_b D_b$ is parallel to the line O C_a. By increasing the speed of the exciters, the E.M.F. at right angles to the current can be increased, and the lead of the rotor current increased more and more. It will be seen, however, that with the connections made as shown in Fig. 1, the more the rotor current leads the more acute becomes the angle D W O;

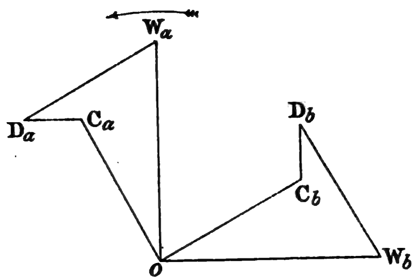

FIG. 2.—Diagram of Electromotive Forces in Circuits of Fig. 1.

i.e. the E.M.F. generated in the exciter comes to have a greater component opposing the E.M.F. generated in the rotor circuit, and this has the effect of increasing the slip of the motor.

The paper above referred to described certain experiments made with independent exciters, after the manner indicated in Fig. 1.

The main objection to the method, as described in Leblanc's early patent, is that it requires two or three exciters, and as the currents to be dealt with would in general be large, the cost of these exciters becomes excessive. Leblanc has described * an exciter which embodies in one machine all phases, and is of a very simple nature. This is illustrated in Figs. 3 and 4, which show two pole armatures, one arranged for two-phase and one for three-phase.

The armature is made like an ordinary drum-wound continuous-

* *Éclairage Électrique*, vol. 29, p. 113, 1901, and *Comptes Rendus*, vol. 109, p. 172, 1889. See also Maurice Latour, *Éclairage Électrique*, vol. 29, p. 294, 1901; British Patent Specification, No. 13402 of 1901; A. Heyland, *Éclairage Électrique*, vol. 29, p. 328, 1901; H. Poincaré, ibid., vol. 30, p. 77, 1902.

current armature. It is surrounded by a simple ring of laminations, having inwardly projecting poles, but without any field windings. The notches in the field are to aid commutation. If such an armature, as illustrated in Fig. 3, be provided with four brushes, placed at 90° to one another on the commutator and connected to the four slip rings of a two-phase rotor of an induction motor, and is run at a speed which is high as compared with the frequency in the rotor circuits, it will have the effect of producing leading currents in the rotor. The beauty of this exciter is that the armature currents themselves excite the field, and produce a flux in the armature which is in such a phase as to generate an E.M.F. in each circuit, exactly at right angles to the current carried by that circuit. For, at the instant when the maximum current is going into the armature by brush A_1 and out at brush A_2, the armature will be magnetized with one pole at the top and another pole at the bottom, so that no E.M.F. is generated in phase A, but a

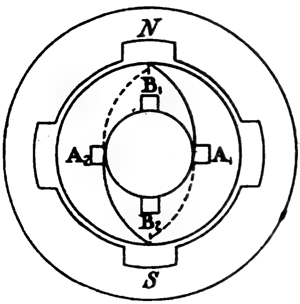

Fig. 3.—Leblanc's Two-phase Exciter.

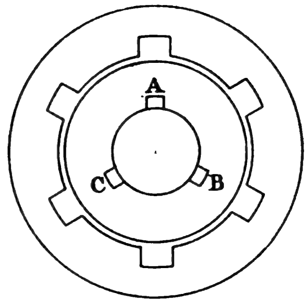

Fig. 4.—Leblanc's Three-phase Exciter.

maximum E.M.F. is generated in phase B. Thus we see that the E.M.F. in any phase is always at right angles to the current in that phase. The question whether the E.M.F. leads or lags behind the current depends only on the direction of rotation. Such an exciter can be built for three phases as illustrated in Fig. 4, and would be much cheaper to build than three separate exciters. By proper design, and by using carbon brushes, the commutation can be made sufficiently good; but in view of the fact that the rotors of induction motors of large power usually carry very heavy currents, the commutator of such an exciter, say, for a 1,000-h.p. motor, would be of considerable dimensions.

A. Scherbius, in a recent letter to the *Electrician*,* gave an illustration of a phase advancer of this type made by Messrs. Brown, Boveri & Co., capable of bringing to unity power factor a 600-h.p. motor. This machine is illustrated in Fig. 5. The overall dimensions of this set are 50 in. by 22 in. by 25 in., and its weight 750 lb. The cos ϕ

* *Electrician*, vol. 69, p. 582, 1912.

curves of a 400-h.p. 32-cycle motor running at 160 revs. per minute both with and without the advancer are given in Fig. 6.

It will be recognized that the stationary iron frames in Figs. 3 and 4 are not really necessary, except in so far as they may reduce the magnetic reluctance of the magnetic circuit when an open slot-winding is used on the armature. If a winding with closed slots is used, the magnetic circuit may lie wholly within the armature. While the armature rotates, the field remains stationary as long as a continuous current is supplied to one set of brushes. If currents slowly alternating, such as those from a rotor winding, are supplied, the field slowly revolves in space while the rapidly revolving armature conductors cut across this field and generate the necessary leading electromotive forces. An exciter of this kind, having no external field, and made by Messrs. Brown, Boveri & Co., was recently illustrated in the *Electrician*.* The use of the external frame, however, seems to possess several advantages: it enables open slots to be used on the armature, and by fixing the position of the field independently of the currents carried by the armature enables the commutation to be performed in a thoroughly satisfactory manner.

It should be pointed out that the main reason why the phase advancer has a fair chance of commercial success is that it is a machine of small output in comparison with the amount of change of wattless load which it is capable of effecting when used in conjunction with an induction motor of suitable size. A phase advancer of only 30-k.v.a. capacity is capable of changing the power factor of a 1,300-k.v.a. motor from 0·88 lagging to 0·95 leading. That is to say, the motor instead of requiring to be fed with lagging wattless current to the amount of 600 k.v.a. will relieve the generators supplying the system of a wattless load of 400 k.v.a., making a total change in the wattless power of 1,000 k.v.a. to the good. The reason is that the phase advancer stands in the same relation to an induction motor as an exciter does to a synchronous motor. An exciter of comparatively small capacity can over-excite a synchronous motor so as to make it supply a wattless load fifty times as great, measured in k.v.a., as the rating of the exciter. Now if for some mechanical work a large induction motor must be employed, the extra cost of making that motor run at unity, or even at a leading, power factor is not very great. It is merely a question as to the cost of an advancer whose rated output is some 3 to 6 per cent of the rating of the motor, and the cost of a three-phase double-throw switch for putting it in and out.

The general theory of the phase advancer is given in the paper referred to above.†

The armature may either be of the open-circuit star type (see *Journal of the Institution of Electrical Engineers*, vol. 42, p. 612, Fig. 10, 1909), or of the closed-circuit type. Both kinds of armature commutate well.

* Loc. cit.

† A more extended graphical theory was given by Professor Gisbert Kapp in the *Electrician*, vol. 69, pp 222 and 272, 1912.

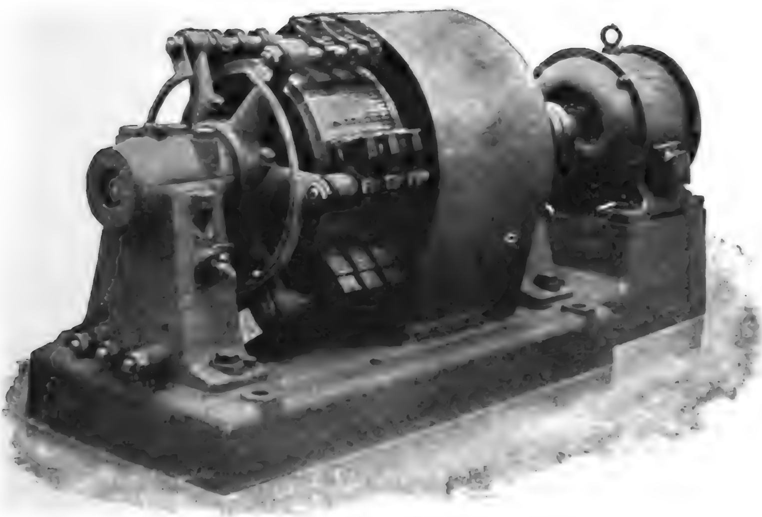

FIG. 5.—Scherbius Phase Advancer, built by Brown, Boveri & Co.

The first (see Fig. 7) is suitable when the current to be collected on the commutator is very great and the voltage to be generated is small, say not more than 15 volts. It enables a very wide brush (extending over

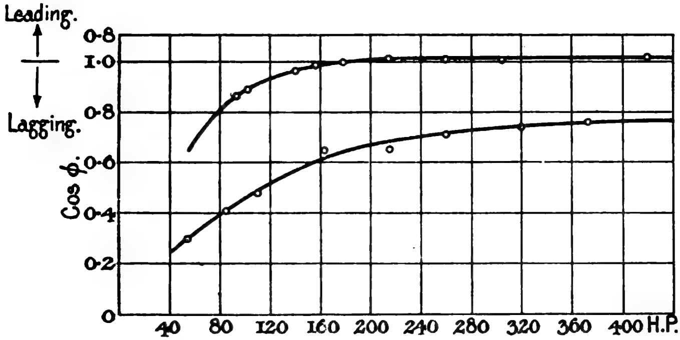

FIG. 6.—The Lower Curve gives the power factor of a 400-h.p. motor when run without the Phase Advancer. The Upper Curve shows the power factor of the same motor with the Advancer in circuit. The wide range of load at unity power factor is obtained by saturating the iron of the Advancer.

0·7 of the pole pitch) to be used. The second type (see Fig. 8) is suitable when the current is not very great and the voltage is higher. The illustrations given in Figs. 14 and 15 relate to a machine with a

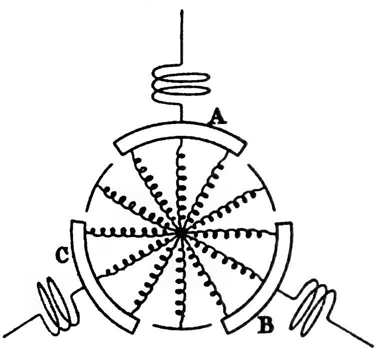

FIG. 7.—Diagram of Open-circuit Armature with several Branches in Parallel under Wide Brush belonging to each Phase (see *Journal of the Institution of Electrical Engineers*, vol. 42, p. 612, Fig. 10, 1909).

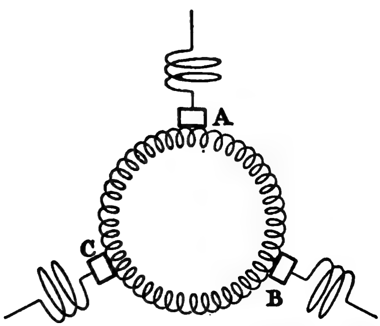

FIG. 8.—Closed-circuit Armature forming a Mesh Connection between the Phases.

closed armature winding. The design is identical with that of some smaller machines of 6-k.v.a. rating now running in commercial service, but the dimensions shown are suitable for an advancer of 30-k.v.a. output.

In designing the rotor of an induction motor we have a fairly free hand in the choice of the stand-still voltage. We may choose a low voltage and a large current, or a higher voltage and a smaller current. It is usual even on very large motors to keep the stand-still voltage below 1,000 volts so as to avoid excessive stresses on the insulation; but it is probable that if phase advancers are much used in the future the stand-still voltage of the rotors of very large motors will be somewhat increased. There is no difficulty in insulating the revolving winding to withstand considerably higher voltages, particularly as the running voltage is exceedingly low.

The cases that will be found most suitable for the addition of phase advancers to induction motors are those where the motors are intended to run continuously in one direction throughout the greater part of the day. If a motor is intended to be started and stopped

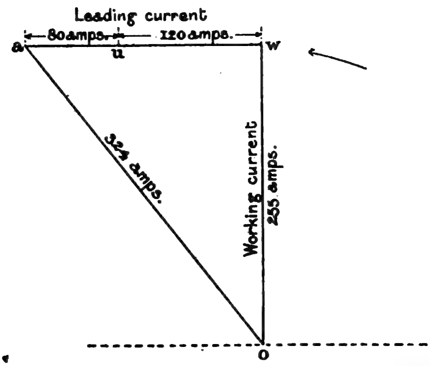

FIG. 9.—Construction for finding value of Rotor Current required to produce a given leading Power Factor.

frequently, or reversed, then it is not suitable. Large induction motor-generators, whether the continuous-current load is steady or not, might very well be fitted with phase advancers to improve the power factor of the system to which they are connected; also large induction motors driving fans for mines or driving other machinery which runs continuously in one direction.

Let us suppose that we wish to design a phase advancer to be direct connected to an 800-h.p. induction motor driving a continuous-current generator. If the motor has already been built, it is necessary to inquire whether its rotor is provided with a winding brought out to slip rings; and if so, whether the slip-rings and brush gear are designed to carry the full-load current continuously. One could adapt a phase advancer to almost any normal rotor winding (except, of course, a short-circuited winding); but if the current is very high and the voltage low, the cost of the advancer will be greater than where the current is fairly small and the voltage higher.

Suppose that the rotor has a three-phase star-connected winding having a stand-still pressure of 800 volts per phase. The working current (that is to say, the current in phase with the voltage) will then be about 255 amperes, which can be collected on a comparatively small collector. To find the rotor current necessary to make the motor run at 0·95 leading power factor, proceed as follows:—

Set off a vertical line representing 255 amperes, as shown in Fig. 9. The power factor of an 800-h.p. 50-cycle motor running at 490 revs. per minute might be about 0·88, so that without the advancer one would have a lagging current equal to 47 per cent of the working current. If the advancer caused the rotor to take a leading current of

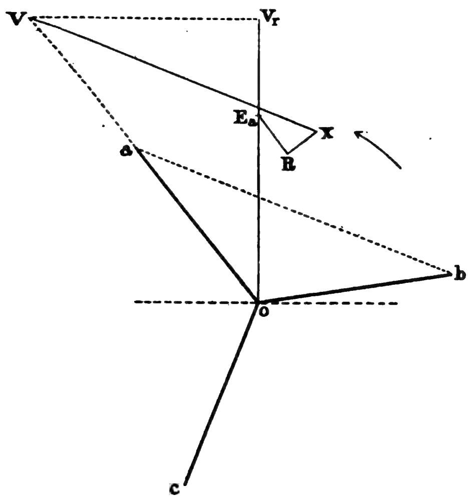

FIG. 10.—Construction for finding the Voltage required to be generated by Advancer.

47 per cent (that is, 120 amperes) the power factor at the stator terminals would be nearly unity. If now it is desired to make the power factor at the stator terminals 0·95 leading, we must supply to the rotor an additional 31 per cent of leading current, making 200 amperes wattless in all. Adding as vectors the 200 amperes wattless to the 255 amperes working current we get 324 amperes per phase for the rotor when running under these conditions. This is the current for which the advancer must be designed. If we had made the rotor with a voltage per phase of 400 we should have had 650 amperes, which would have made a somewhat more expensive, though perfectly possible, phase advancer.

Next as to the voltage to be generated by the advancer. As the armature of the advancer is to be mesh connected, it is simpler to take the voltages across the slip rings than the voltage per phase

of the star winding. Indeed, as the motor would work the same whether it were mesh connected or star connected, we may, if we like, consider it mesh connected, as we have done in Fig. 11. If the normal slip of the motor at full load be 1·45 per cent, the E.M.F. generated by the slip will be 20 volts measured between rings. Lay off as in Fig. 10 the vertical line $O\,E_a$ to represent this voltage generated by the slip in phase A. In Fig. 9 we have found the angle by which the current must lead on this voltage, so we can set off the line $O\,a$ to represent the current in phase A (see Fig. 11). Similarly $O\,b$ and $O\,c$ represent the currents in the other phases. We should allow about 6 volts for pressure drop in brushes and in the resistance of the advancer. This will be represented by $E_a\,R$ in phase with $O\,a$. Then there will be some reactive drop in the field coils of the advancer. We may provisionally

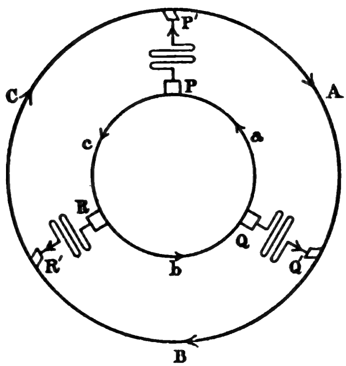

FIG. 11.—Diagram of Mesh-connected Phase-advancer Armature *a b c*, Field Connections P, Q, and R and mesh-connected Rotor A B C of Induction Motor.

allow 5 volts for this, and after the machine is calculated we can make a check calculation to see if it is enough. This is represented by R X. There is no reactive drop in the armature because the compensating winding wipes out its field. We see that if we add a voltage X V, parallel to $b\,a$, we shall get a resultant voltage O V in phase with $O\,a$; and this is what we want. If, therefore, we excite the advancer with a current which is in phase with the sum of $O\,a$ and $-O\,b$ (shown by the dotted line $b\,a$) we can make the current lead by the right amount. The voltage to be generated by the advancer is therefore given by X V, which when scaled off gives us 33 volts. It will be seen that the projection of O V on the vertical line gives us $O\,V_r$, which is greater than $O\,E_a$. If this voltage $O\,V_r$ is greater than is necessary to drive the working current through the rotor circuit, the only effect will be that the slip of the rotor will be reduced until we get the right working current for the load. If it should come out that

OV_r is not sufficient to drive the working current, then the slip of the motor will be increased.

From Fig. 10 it appears that with 33 volts generated by the advancer the slip will be slightly reduced. We thus arrive at the rating of the advancer, namely, 33 volts between terminals and 324 amperes per phase.

We have next to decide what type of advancer to build. In this case it is not necessary to adjust the amount of leading current taken from the line at all loads, nor is it necessary to control at all loads the boosting effect of the advancer. It will therefore not be necessary to install a separately-excited advancer. It will be found that the series-wound advancer will have more suitable characteristics for the case in hand than a shunt-wound advancer. With a series excitation the amount of leading current taken from the line increases with the load, so that the power factor of the motor remains more nearly constant than where the excitation of the advancer remains constant. We will therefore decide upon a series winding. Next as to the type of armature. When the voltage to be generated is of the order of 30 volts or higher, and the current is reasonably low, as in this case, the best kind of armature is that with a closed winding just like an ordinary continuous-current armature.

Theoretically, three salient poles (equivalent to two magnetic poles) are quite enough for a machine of the rating required in this case, but a machine of six poles (equivalent to four poles magnetically) is more likely to fit in with standard frames and standard punchings. We will therefore decide on six poles. This will give us six brush-arms, two in parallel in each phase. There will be 162 amperes per brush arm, and $162 \div 1{\cdot}73 = 94$ amperes per conductor.

As the speed of the main motor in this case is quite high, 490 revs. per minute, it is quite a good plan to couple the advancer directly to it, just as one would an exciter to a high-speed synchronous motor. We will consider later the arrangements which can be made for low-speed motors. We shall therefore take the speed of the advancer at 490 revs. per minute.

It will not be worth while to cut down a machine of this type to the smallest possible size, because the addition of a little superfluous material will not increase the cost by a very large percentage, and when we are making a machine we might as well make it so that without much further development it may be used in a large variety of cases. If we take a large D^2L constant of $9{\cdot}5 \times 10^5$ cubic cm. it will not be excessive, though very ample. A diameter of 46 cm. is suitable for a speed of 49 revs. per minute, and the length of iron may be 18 cm.

The easiest way of designing a phase advancer of this type is to proceed as if it were a continuous-current machine whose voltage is 1·41 times greater than the virtual voltage called for in the specification. The armature need not differ in any particular from a continuous-current armature. The field winding will be provided with

series exciting coils and compensating windings connected to the various phases in the manner described below.

The main points to look to, that are not found in a continuous-current design, are:—

1. The machine though having six salient poles is a 4-pole machine magnetically, and we must remember this when fixing the dimensions of the iron behind the slots.
2. The voltage to be generated as a continuous-current machine is 1·41 times greater than the virtual voltage called for.
3. The fluxes in the salient poles which constitute magnetically a pole-pair are 120° apart in phase, so that the voltage generated in an armature coil which lies partly under one pole and partly under another is only 0·86 of the voltage that would be generated if the two poles were carrying the maximum flux at the same time.
4. It is necessary to arrange the series winding on each pole so as to cause the flux to lead, by the right amount, ahead of the current carried by the armature conductors passing under the pole.
5. It is desirable to arrange the compensating winding so that its effect is equal and opposite to the armature winding adjacent to it, and for this purpose it is necessary to have regard to the phases of the currents in the armature and field.
6. It is desirable to provide a commutating flux which shall be proportional to, and in phase with, the current to be commutated.

We begin, then, just as we would on a continuous-current generator. The voltage to be generated is $33 \times 1{\cdot}41 = 46{\cdot}5$ volts. There are six ways through the armature, each carrying 94 amperes. If we choose 72 slots with 4 conductors per slot we get 288 conductors, and these multiplied by 94 give us 27,000 ampere-wires, a fairly easy current-rating for an armature 46 cm. in diameter.

If we denote the area of the cylindrical working face of the armature by A_g and the maximum flux density in the gap by B, then we may take the magnetic-loading as proportional to $A_g B$. If we have a pole arc equal to 0·72 of the pole pitch, then as there are 48 conductors in series and the speed is 8·2 revs. per second—

$$46{\cdot}5 \times 10^8 = 0{\cdot}72 \times 8{\cdot}2 \times 48 \times A_g B \times 0{\cdot}866.$$

Observe the multiplier 0·866, which comes into the equation on account of the circumstance mentioned in paragraph (3) above.

Thus we arrive at the magnetic loading $A_g B = 0{\cdot}189 \times 10^8$. If we work the iron in the teeth at 18,500 lines per sq. cm., we shall require a total mean cross-section of all the teeth of 1,020 sq. cm. Our conductors, to carry normally 94 amperes and 25 per cent overload, may be made 0·23 by 1·27 cm. Four of these will require slots about

0.77×3.7 cm. To provide room for 72 slots and give the necessary cross-section to the teeth we shall require a net length of iron of 16·4 cm. Allowing 11 per cent for paper on the punchings and 0·6 cm. for a ventilating duct, we arrive at a gross length of iron of 19 cm. The rest of the calculation of the armature is the same as for a continuous-current machine, except in the matter of commutation, which we will consider later.

We must now consider how we are to wind the field poles so as to give to the excitation its proper phase. The first point to note is that the six armature circuits are connected in mesh, while the leads from the brush-holders are connected in star.

In Fig. 11 we have a diagram of connections as they would be if the machine had only three brushes. Obviously this diagram applies equally well to the machine with six brushes where brushes at opposite ends of a diameter are in parallel with one another. The inner circle of Fig. 11 represents the closed winding of the armature of the advancer. The small letters *a*, *b*, *c* show the three phases mesh connected. Three brushes—P, Q, and R—bear on the commutator and convey the currents to the outer circle, A, B, C, which represents the winding of the rotor of the induction motor taken as mesh connected. It does not matter in practice whether the rotor of the induction motor is star or mesh connected, but for our diagram it is convenient to connect it in mesh. The arrowheads show the direction along each conductor which is taken as positive for the purpose of our clock-diagram, Fig. 10. P, Q, and R are in star, and it is only in series with them that we can connect the series exciting coils. The voltage in phase A of the rotor is the voltage we would measure by connecting a voltmeter to the collecting brushes P′ and Q′. In order to make the current in this phase lead, it is necessary to generate a leading electromotive force in the part *a* of the armature circuit. From Fig. 10 we found that a suitable E.M.F. to inject into phase A was the E.M.F. XV, which is in phase with $(a-b)$. From Fig. 11 we see that the current in Q is $(b-a)$, so that $-Q$ is $(a-b)$. We will therefore excite the poles under which coils *a* are passing with $-Q$. The span of the armature coils is almost a pole-pitch, so that the coils in phase *a* will be passing under two adjacent poles, which we will call pole P″ and pole Q″. Now it is not convenient to use only the conductor Q to excite P″ and Q″, because we have to arrange for return paths and also for a compensating winding, and we want to make a fairly simple mechanical arrangement of the coils. We therefore take advantage of the known fact that currents $P+Q+R=O$, $\therefore Q=-P-R$. Let us make an arrangement of exciting windings and compensating windings like that indicated in Fig. 12. There the exciting conductors which pass between poles P″ and Q″ are $+Q$, $+Q$, $-P$, $-R$. That is to say, they are equivalent to 3 Q. The question whether the excitation $+Q$ gives a forward or a backward E.M.F. in a coil depends upon the direction of rotation, and also upon the question whether the armature is wound right-handedly or left-handedly. It will be seen that

this arrangement of conductors lends itself to form mechanically a simple barrel winding as shown in Fig. 13. The conductors lie in two layers, and all the end connectors of one layer are bent to the right and all the end connectors of the other layer are bent to the left. This figure seems fairly complicated, but is made up by connecting, according to the scheme of Fig. 12, a number of groups of coils forming part of a simple barrel winding.

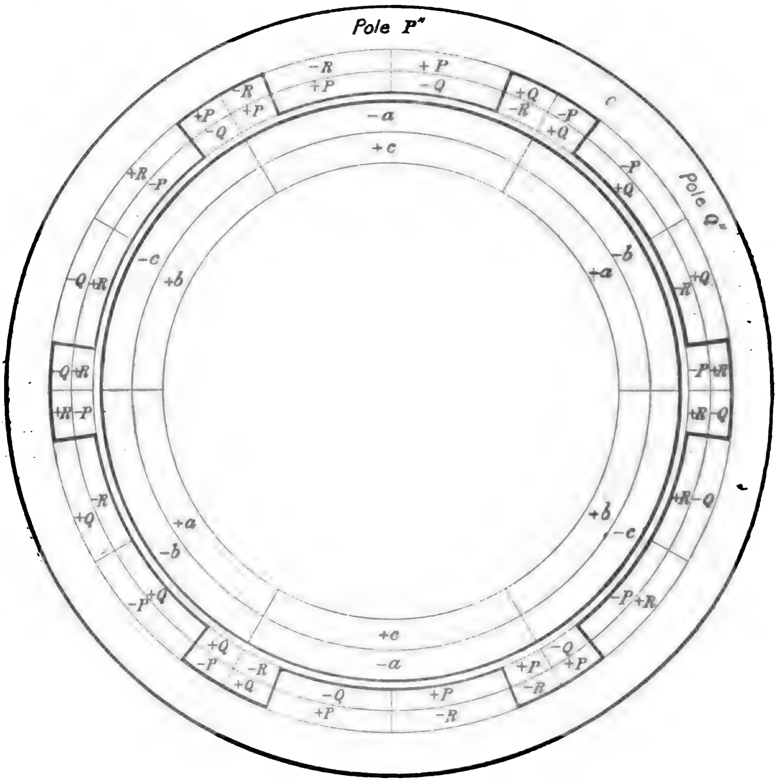

Fig. 12.—Showing Relation of Exciting Windings and Compensating Windings to Armature Windings.

The letters in Fig. 12 which are placed on the salient poles represent the compensating windings. It is easy to prove that these are in direct opposition of phase to the currents in the armature under the pole. For instance, take the pole P''. The compensating winding on this is, + P + P − R − Q, or + 3 P. Now the armature coils which lie under P'' are *c* and − *a*, and we know that $a - c = + P$. Moreover, the 16 conductors in the pole face carrying the currents P, Q, and R are equivalent to 12 conductors carrying the P current.

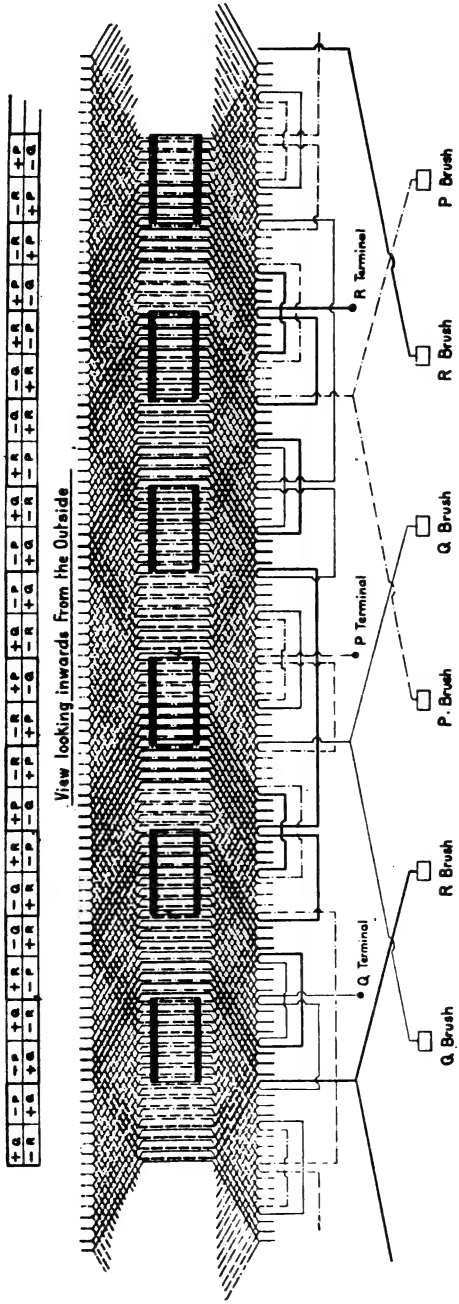

FIG. 13.—Development of Series Windings and Compensating Windings showing Connections to the Brushes.

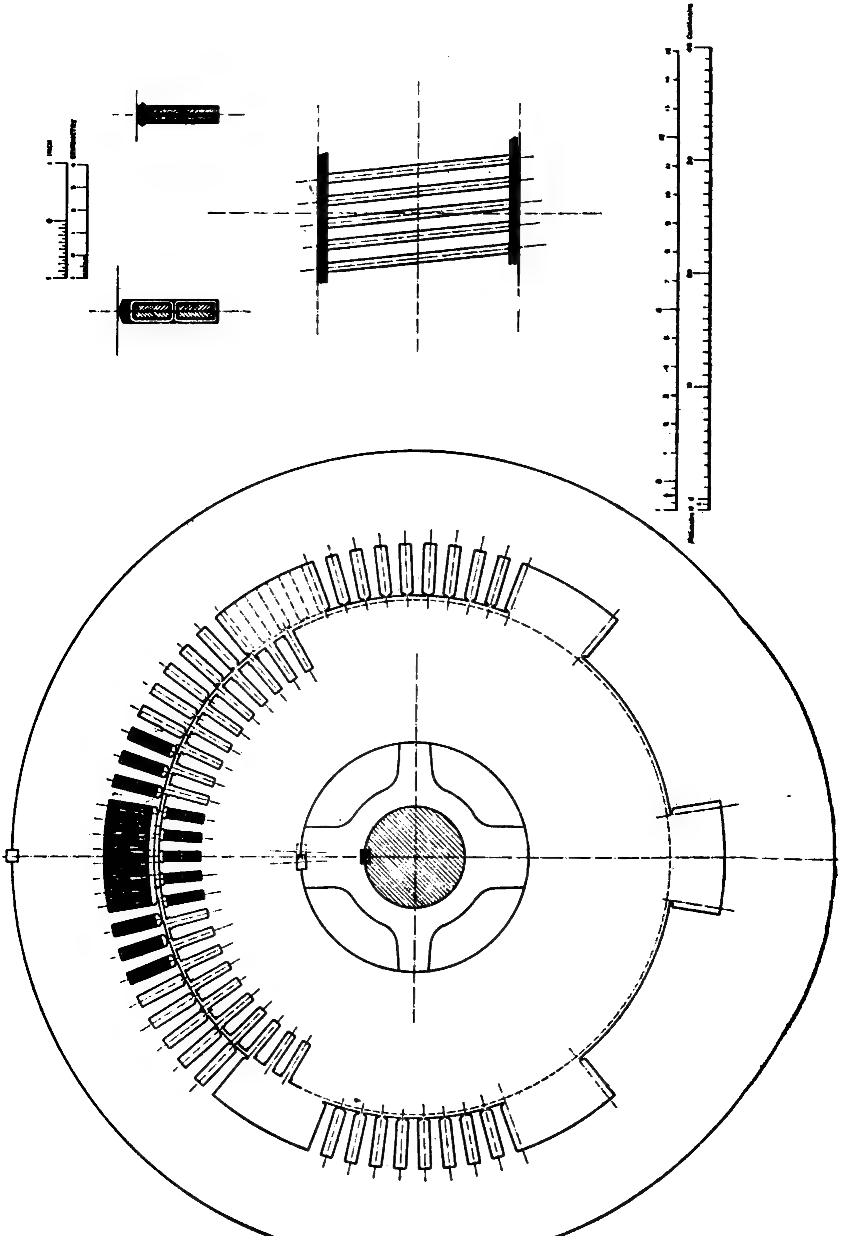

Fig. 14.—Showing Slots of Rotor and Stator and the Skewing of the Slots in the Rotor.

Opposite the pole P″ are 12 armature slots each carrying —2 *a* and 2 *c*. When we remember that there are two paths in parallel per phase in the armature we see that the currents in these 12 slots are exactly balanced magnetically by the 12 P currents in the compensating winding.

It will be found that an air-gap of 3 mm. will have an apparent length of 3·6 mm. when we take into account the opening of the slots. The flux density in the gap obtained by dividing $A_g B$ by A_g is 6,950; so that the ampere-turns on the gap will be 2,000. The ampere-turns on the armature teeth will be 510, and on the rest of the magnetic circuit about 190; so that the ampere-turns per pole will be about 2,700, or 5,400 per pair of poles. These ampere-turns are provided by the 16 conductors which thread between the poles P″ and Q″, for the 16 conductors carry current equivalent to 3 × 4 P. At its maximum P is 324 × 1·41 amperes, which multiplied by 12 gives us 5,500 ampere-turns per pair of poles. In practice it will be found unnecessary to adjust the speed exactly, because the particular power factor at which the motor runs is not a matter of importance. It is not usually necessary to make any provision for the adjustment of the power factor during running; it is sufficient that the motor shall take a leading current from the line at all loads. If it should be necessary to adjust the power factor, this can be done either by changing the speed of the advancer or by diverting some of the field current from the series coils.

In cases where the speed of the motor is not great, it is more economical to belt the advancer to it so as to obtain a higher speed. In other cases the advancer may be run from any convenient counter-shaft in the mill where it is used, or it may be directly connected to an independent motor.

Commutation.

The most important consideration of the design of the phase advancer is the obtaining of good commutation. It is chiefly for this purpose that the field frame and winding described in this paper are provided. Where in a continuous-current generator the voltage between the bars is small, the commutation can generally be forced by the resistance of the carbon brushes; but it is very much more desirable to provide a commutating E.M.F. which shall at all times be proportional to the current to be commutated. In the machine here described this result has been effected by giving each armature coil a span of somewhat less than the full pitch and arranging the positions of the brushes so that one of the limbs of each coil is moving in the fringing field of a pole excited by a current which is at all times proportional to the current under commutation. The currents in the two branches of the armature, *a* and — *c*, which combine to form P, are out of phase with one another and are not directly under control of the commutating flux; but the rate of change of the current in the coil under commutation ought at all times to be proportional to P. Now the

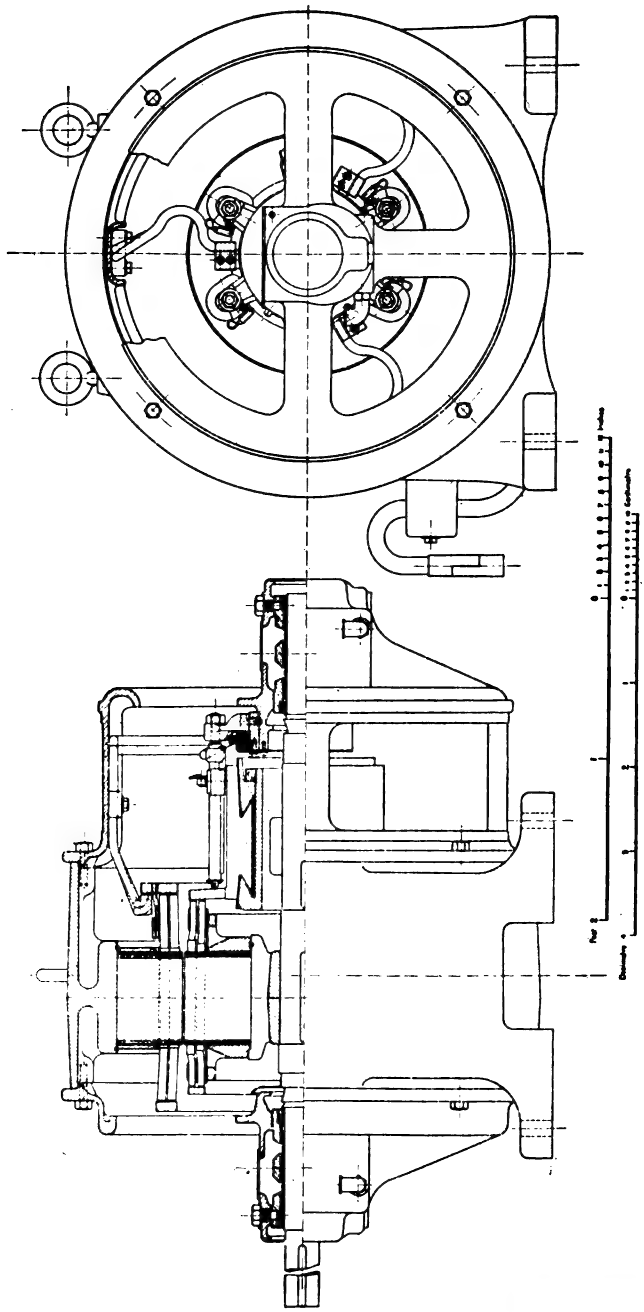

FIG. 15.—Sectional Elevation of Phase Advancer.

pole P″ (Fig. 12) is excited so that the fringing field in which the left-hand limb of the coil *a* is moving is at all times proportional to P. By making the coil with a short throw the right-hand limb can be taken out of the influence of the pole Q″. The exact position for the brushes is, of course, obtained by trial; in practice it is found that the commutation is perfect. The alternation of the current in the armature and field causes a harmful E.M.F. to be set up in each coil under commutation; but as the frequency is so very low (say one cycle per second), this E.M.F. is not sufficiently great to create any disturbance. In the machine under consideration it only amounts to one-sixth of a volt.

Performance.

In a certain mill in the north of England there is a 250-kw. generator which on account of the low power factor of the motors connected to it is somewhat overloaded. As it would be a rather costly undertaking to install a new generator, the alternative proposition was put forward of connecting a phase advancer in circuit with the rotor of a certain 140-h.p. motor in the mill. It was seen that this at least would help matters, although the capacity of the motor and its phase advancer were not great enough to bring the power factor up to unity. A 5-k.v.a. advancer was installed, with the result shown by the following figures :—

Total Load on Mill.

	Amperes per Phase.	Volts.	Power-factor.
Advancer cut out ...	325	440	0·70 lagging
Advancer connected in ...	240	440	0·92 lagging
Motor Load only.			
Advancer out	105	440	0·74 lagging
Advancer in	97	440	0·96 leading

Whenever the advancer was switched in the volts of the generator rose from 440 to 470. The figures in the above table were taken after the rheostat had been adjusted to make the voltage normal.

Another case that might be quoted is the case of three 400-h.p. motors installed for pumping water for the Port of London. As the speed of the motors is low the normal power factor at full load is only 0·55. These machines have been fitted with phase advancers, with the result that they run on a slightly leading power factor.

The following table gives the result of tests upon a 750-h.p. motor running under various conditions with and without a phase advancer.

Reading.	Volts.	Kilowatts.	Power factor.	Remarks.
1	1,840	280	86 per cent lagging	Phase advancer not running
2	1,840	300	Unity	Phase advancer running
3	2 200	375	Unity	Phase advancer running
4	2,600	375	80 per cent leading	Phase advancer running (increased speed)
5	2,000	20	50 per cent leading	Phase advancer running (speed as before)
6	2,600	312	40 per cent leading	Phase advancer running (increased speed)
7	2.560	20	5 per cent leading	Phase advancer running (speed as before)

In the case of readings 1 and 2 the voltage was maintained constant by hand regulation. The rise of voltage between readings 3 and 4 is due to the phase advancer yielding a leading magnetizing current. It will be seen that when the first reading was taken the phase advancer was not running, and the power factor of the motor was then 0·86 lagging. The next two readings show that with the aid of the phase advancer the power factor has been brought up to unity, whilst in the case of some of the lower readings, with the aid of the phase advancer, the power factor has been changed to 0·5 leading.

The motor on which these experiments were tried formed part of a motor-generator set, and it was therefore possible to make it run as an induction generator by speeding up the continuous-current machine. When this was done the terminals of the phase advancer were reversed so as to give the right rotation phases relatively to the rotor winding. It was found that the induction generator could be run so as to supply a lagging wattless load, the magnetization of the motor being carried out by means of the rotor current. In another case a phase advancer was connected to the rotor winding of a 750-h.p. motor driving a rolling mill. The load on this mill was very unsteady, and for considerable intervals of time was so light that the motor was running on an extremely low power factor. The effect of the phase advancer was to make this induction motor take a small leading current at all loads. Before the installation of the advancer the whole works took a load of about 1,000 k.v.a. at 0·64 power factor; when the advancer was started up this was changed to 800 k.v.a. at 0·8 power factor. In cases where generators are overloaded and it is not desirable to install large machines, a considerable advantage can sometimes be obtained by the use of a phase advancer in conjunction with a large motor on the system.

Discussion.

Dr. Kapp.

Dr. GISBERT KAPP: Some four years ago Professor Miles Walker read before this Institution his paper on the phase advancer* shown in

* *Journal of the Institution of Electrical Engineers*, vol. 42, p. 599, 1909.

Dr. Kapp.

Fig. 7 of this paper, and at that time I had some doubts whether the machine would work sparklessly. These doubts were dispelled later when Professor Miles Walker showed me the machine at work. It was quite a revelation to see a machine run sparklessly that had an open coil winding and such very wide brushes. The author in his new machine uses a closed coil winding in mesh connection, and he introduces a most ingenious method of producing a field which makes the commutation induced. I consider this one of the most valuable features of the new design, and I think the author is correct when he says that there is no limit to the size of motors to which his phase advancers may be applied. I think Professor Miles Walker might have laid more stress in his paper on the commercial importance of his machine. A motor fitted with a phase advancer is able to carry a higher overload, but the author says nothing about that. Yet this is an important point to the user of electric power. Take the case of a manufacturer whose business grows to such an extent that he may have to buy a new motor. If the author can give him a phase advancer costing a mere fraction of the cost of a motor, and increase the peak load of the old motor by 20 or 30 per cent, he will save that consumer a large outlay, and at the same time improve the power factor. He will also improve the yearly efficiency, since motors are generally not loaded evenly all the year round. It is probable that the average load on a 100-h.p. motor will be about 50 h.p. for the whole period of 3,000 hours. But it is also probable that the 100-h.p. motor may at times get a peak load of 130 to 150 h.p., so that to be quite certain that the motor would not be overloaded and the circuit-breakers tripped it would be necessary to buy, say, a 120-h.p. motor. Such a motor would work very much underloaded for the greater part of the time, consequently its efficiency would not be so good as it might be. Moreover, that motor would be an undesirable load for the power company, owing to its low power factor. By installing the phase advancer not only will a 100-h.p. motor do safely the work of a 120-h.p. motor, but it would also do it with a greater yearly efficiency and a very much better power factor.

Any existing motor can be improved by adding a phase advancer to it; but if a new motor has to be built to work with a phase advancer, the design of the motor may be specially adapted for that purpose. Thus, an "iron machine" may be built instead of a "copper machine." An "iron machine" will be cheaper, but it will have a worse power factor. This defect, however, can be corrected by the phase advancer. If a very high power factor is desired it will be cheaper to build an "iron-machine" with a phase advancer than a "copper machine" without a phase advancer; and if unity power factor is wanted the phase advancer becomes absolutely necessary. Let us compare an ordinary induction motor built for a power factor of, say, 0·9 with a motor fitted with a phase advancer. The former must be what I have termed a "copper machine," meaning that the cost is high owing to the considerable amount of copper required to

Dr. Kapp. get this power factor, whilst the latter may be an "iron machine," meaning thereby a machine requiring little copper. This latter machine will be cheaper, and the saving in the cost of the motor itself will go a long way towards paying for the phase advancer. In other words, a specially designed motor combined with a phase advancer to give unity power factor will not be materially dearer than a motor designed to give by itself a fairly good power factor. Moreover, if both machines are designed for the same power factor of, say, 0·9, the set consisting of a motor combined with a phase advancer will actually be the cheaper. As an example I may cite a motor of 25 h.p. fitted with a Scherbius phase advancer which I have in my laboratory at the Birmingham University. Its synchronous speed is 1,000 revs. per minute, and its weight, including the phase advancer attached, is 800 lb. The catalogues of the best English makers of induction motors show that for the same power and speed the average weight of a plain induction motor is about the same. The efficiency is also about the same, namely, close on 90 per cent; but there is this difference, that the Scherbius set has a power factor of unity at full load, whereas the ordinary motor has a power factor of something under 0·9.

The commutation of the Scherbius phase advancer is perfect; there is absolutely no sparking. On the Continent a good many phase advancers, mostly of the Scherbius type, are in use; the largest of which I know is for 1,200 h.p. This, however, differs from the phase advancer mentioned above in so far as it is provided with a stator. For large powers the type without stator is, as the author mentions, unsuitable. A good power factor is of primary importance to the power companies. Generally the power factor is bad, and something has to be done to improve it; so the companies install a "rectifier" for this purpose. It would be wrong to put the rectifier side by side with the generators, because it then does not relieve the line of wattless current, but only the generators. The right place to put a rectifier or a phase advancer is on the consumer's premises. The companies should go to one of their big customers and say, "Allow us to fit a rectifier to your motor. It will help you a little and it will help us considerably, because we are relieving our line." That would be a sensible proposition; it would benefit the consumer because his motor would become more efficient. It would also benefit the power company because it would cheapen generation and enable them to take on more customers. Why do not the companies do that? I believe it is for the very simple reason that nobody likes to put capital in another man's premises when he has not the control of the apparatus. It may be said, "The customer has an advantage in using a phase advancer, therefore he should pay for it." He will if he is a clear-sighted business man and knows all the intricacies of cos ϕ. But very few owners of works are acquainted sufficiently with these matters. Supposing, however, it can be shown to such a customer that he will save money by adding a phase advancer to this old motor, he will buy one; but not otherwise. A very easy way would be for the

company to give the customer a special discount in respect of a good power factor. But again power engineers point out the difficulty that if an extra discount is given to one consumer other consumers will grumble. Further, the amount of discount must have some relation to the amount by which the power factor has been improved; and an attempt to settle those matters by a mere estimate, which must necessarily be uncertain, would be unsatisfactory. Dr. Kapp.

These are serious difficulties, but fortunately they can be avoided by using a method of metering invented by one of our foreign members, namely, Professor Arno of Milan, who has designed a meter which will discriminate between the good and the bad customer. This meter is made by Messrs. Landis and Gyr of Zug, and Messrs. Olivetti of Milan. The system is applicable to any meter, as it merely involves a slight alteration in compensating coils. The invention grew out of an investigation which Professor Arno and Mr. Conti, engineer to some Italian power companies, made throughout Italy with respect to working cost as affected by the power factor of their customers. It was found that the cost to the power company may be expressed by the formula—

$$\text{Cost} = \kappa \int_0^t (a\,\mathrm{E\,I} \cos\phi + b\,\mathrm{E\,I})\,d\,t.$$

The reading of Professor Arno's meter gives the cost; and by using such meters there is no need for special discounts or any arbitrary discrimination between good and bad customers. From a very large number of actuarial investigations Professor Arno found that a will vary a little but not much: an average practical value is $\frac{2}{3}$, and b is $\frac{1}{3}$. From these figures it will therefore be seen that for a customer who is taking no wattless current (that is, $\phi = 0$) the cost $= \frac{2}{3}\,\mathrm{E\,I} + \frac{1}{3}\,\mathrm{E\,I} = \mathrm{E\,I}$. That means that the meter registers true kilowatt-hours. On the other hand, if it were possible to take current and no power at all (*i.e.* $\phi = 90°$), such a customer would have to pay $\frac{1}{3}\,\mathrm{E\,I}$, *i.e.* he pays one-third of what he would have to pay if the current he took were power current and not wattless. That seems a fair arrangement. By using meters of this kind it is unnecessary to differentiate between consumers, the meter allowing the good consumer to get his power at a somewhat cheaper rate. I have taken the case of the motor of which particulars are given by the author in his paper. This motor has at full load a power factor of 88 per cent. It will not work at full load all the year round, but the yearly consumption of energy may be taken as corresponding to, say, about half-load for 3,000 hours. The input will then be a million units. The power factor at half-load is 0·8 without, and unity with, a phase advancer. An Arno meter would then register 1·08 times the amount recorded by an ordinary meter. So that 8 per cent represents the additional cost of the wattless current and also the saving by installing a phase advancer. Taking current at 0·5d. per unit, 8 per cent on a million units equals £170. This saving might be divided between the consumer and the

Dr. Kapp. company; the division would come about quite automatically by the tariff being lowered to 0·96 × 0·5d. = 0·48d. for all customers using the Arno meter. The difference of 0·02d. means for the consumer in question a saving of £85. I estimate the cost of the author's phase advancer at about £170; so that even if the consumer pays for the phase advancer it is a good investment. The position of the company is better still, as they have not incurred any capital outlay, and they can sell the extra current represented by sin ϕ to somebody else. That is, it has enabled them to extend their business maybe 10 or 20 per cent without increasing the amount of plant.

The author has mentioned a phase advancer designed by me, and it may therefore be of interest to mention some experiments which have

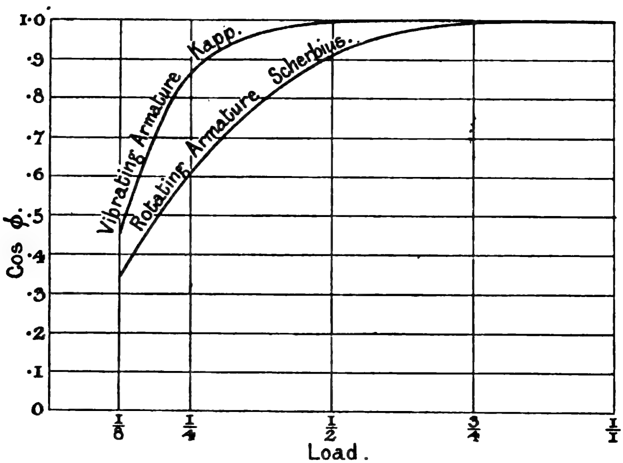

FIG. A.

been made with it. In the first place I wish to point out that there is a difference in principle between the rotary machine such as the author has made, or that of Mr. Scherbius, and the vibrating machine I have made. The difference in principle shows itself in a certain effect which I will try to explain in a very few words. Broadly speaking, the magnitude of the injected E.M.F. in a machine driven by power does not vary much with the slip frequency. It is only slightly greater at small slip, but nearly proportional to the rotor current. The scientific investigation of the vibrating machine shows that the magnitude of the injected E.M.F. is proportional to $\frac{\text{current}}{\text{slip}}$. The effect is that at light loads the injected E.M.F. will be greater with the vibrating than with the rotary phase advancer. This theoretical consideration is entirely borne out by the practical test. A vibrating phase advancer (for brevity I call it a vibrator) was built to my design by the Sandycroft

Foundry Company, and tested first on a Hunt 60-h.p. cascade motor, and Dr. Kapp. then on an ordinary slip-ring motor of 20 h.p. I take the latter test as more suitable for comparison with the Scherbius set of 25 h.p. In Fig. A the curves show the power factor as a function of the load. At full load both types give unity power factor, but at quarter-load there is a difference; whilst with the rotary machine the power factor has come down to 0·7 it is still 0·86 with the vibrator. In Fig. B the curves of the 20-h.p. motor are given. The tests were made by Mr. Hunt, chief engineer of the Sandycroft Foundry Company. There are two sets of curves of primary current and power factor, one set without and the other with vibrator.

The vibrator in connection with the 60-h.p. Hunt cascade motor gave the same kind of power-factor curve, but the power factor did not quite

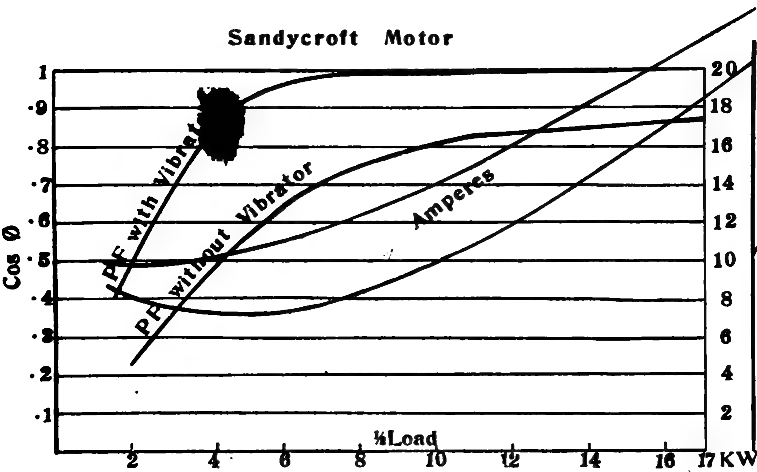

FIG. B.

reach unity. The maximum reached was 0·98 at half-load, and with the primary voltage reduced from 500 to 400 volts Mr. Hunt's cascade motor has in each phase two pairs of tappings, so that a vibrator with six armatures ought to have been used, but we only had one with three. We therefore left two tappings out or short-circuited them, and put the vibrator on to the remaining three pairs of tappings. It was not quite a fair test, but we improved the power factor as follows: With 500 volts and a frequency of 50 in the primary we obtained—

$\cos\phi = 0·92$	0·92	0·93	0·93
at 20	30	40	50 kw.

This test was not made to find out what could really be done in the case of a cascade motor, but merely to satisfy a railway expert who threw doubt on the question that the vibrator would improve the

Dr. Kapp.

power factor if a motor were driven above synchronous speed so as to act as a generator and return current to the line. In a 3-phase locomotive it is important that this should be done. We found that by driving the cascade motor with 5 per cent negative slip it returned into the line 35 per cent of the power, with a power factor of 0·94. The three armatures of the vibrator may be coupled in star or in mesh; the latter is preferable with motors of large power.

Mr. Patchell.

Mr. W. H. PATCHELL: Dr. Kapp, in touching on the commercial aspect of the matter, said that it was necessary to go to the Continent to find the Scherbius motor in general use. I believe that the first phase-corrector ever used was on the Continent at the Bockenheim works, where a synchronous motor was run over-excited. When a phase advancer was required I have approached one of the patentees, and unfortunately have generally found it was just beyond the range of commercial practice. I tried to find out from the paper exactly what it would cost to use the author's machine, and the only clue I could get to the problem is on page 334, where he uses a 30-k.v.a. machine to correct a 600-k.v.a. machine. That is in the neighbourhood of 5 per cent. A machine of that size ought to be reasonably cheap. I strongly support Dr. Kapp's opinion that the phase-corrector should not be placed in the power station but on the consumer's premises, so as to get the benefit on the transmission line. If a commercial machine can be obtained to put on the consumer's premises it will solve the difficulty. If a new motor is being installed and continuous current is available a bigger synchronous machine can be put in and over-excited so as to do what is required. But continuous current is not always available, and it is frequently necessary to deal with induction motors. A Scherbius machine of 1,000 h.p. by Messrs. Brown, Boveri is used for fan-driving at Gelsenkirchen; and if we had been able some years ago to get for fan-driving by induction motors such a machine as the author's, or as Dr. Kapp has now put before us, it would have made a very considerable difference. In the list of machines that Professor Miles Walker has given he has not mentioned the Krämer system, which is very much like the Scherbius. I think I correctly describe the two systems if I say briefly that the Scherbius motor is connected electrically, whilst the Krämer system is connected mechanically. In other words, if the end of the shaft of the motor of which the power factor is to be corrected is available, the Krämer motor can be connected to it either direct or by a chain drive; whereas if there is no shaft available, then tappings must be taken off the machine so as to connect it electrically. I think that correctly describes the main difference between the two systems.

Mr. Ashton.

Mr. A. W. ASHTON: In connection with the adoption of apparatus for power-factor correction, I should like to mention the competing apparatus, namely, the condenser. I have looked through the paper with the idea of ascertaining the total cost of installing and running a phase advancer, and of comparing such figures with what I know to be the cost in the case of the condenser. From the information given on page 334 of the paper for the 1,300-k.v.a. motor, in which the power factor

was to be corrected from 0·88 to 0·95, I have worked out a comparison of the total cost in the case of the phase advancer and for a condenser. In the case of the condenser the initial cost is considerably higher than it would be for the phase advancer; but on the other hand the running cost of the condenser is almost negligible compared with that of the phase advancer. In order to estimate the actual total costs we may take the cost of supplying 1,000 k.v.a. of wattless current by means of condensers at 50 periods to be about £2·8 per k.v.a.; that is, the initial cost of installing condensers to deal with 1,000 k.v.a. of wattless current would be about £2,800, and would be practically independent of the voltage at which the condensers were operated. On the other hand, I have had to assume that the initial extra cost of fitting the phase advancer to the motor would be about £250, and I have also taken the extra loss incurred due to the installation of the phase advancer as 25 kw. Assuming 6¼ per cent as the total capital charges, and also that the cost of energy is £4 per kilowatt of maximum demand plus 0·4d. per unit, the comparative costs for the condenser and the phase advancer then come out roughly as follows: **Mr. Ashton.**

At 100 per cent load factor the annual cost of the condenser is £249 per annum, and for the phase advancer £485 per annum. At 50 per cent load factor the condenser costs £220 and the phase advancer £302. At 25 per cent load factor the condenser costs £206 and the phase advancer £211; whilst at 20 per cent load factor the cost of the condenser is £203 and of the phase advancer only £193. It thus seems that, taking the total cost of correcting for power factor, condensers come out less than phase advancers for load factors of 25 per cent and over.

What I should like to know is, What prevents the introduction of the condenser into more general use for this purpose? I must admit that, as far as actual installations are concerned, the phase advancer has been adopted to a certain extent in preference to the condenser. I suppose there are two reasons which might act in that direction. One is the high first cost of the condenser compared with the first cost of the phase advancer; and the other reason is that the condenser, which as a piece of engineering apparatus is more or less new, is naturally looked upon with a certain amount of diffidence by power station engineers. In that connection I should like to point out that paper cables have been used for a large number of years, and as far as I can see paraffin paper condensers should at any rate give no more trouble, if as much, as ordinary paper cables. It must be remembered that the dielectric used is more impervious to moisture in the case of paraffin paper than in the case of the paper cable mixture; secondly, that the condenser is not so likely to be subjected to mechanical damage as is a paper cable; and, thirdly, the distribution of the dielectric in the condenser makes the dielectric, for the same thickness, two or three times as strong as it would be in the case of the paper cable. Further, comparing the condenser with other electrical apparatus as far as tests are concerned, I should like engineers to consider what part of their

Mr. Ashton. apparatus would stand the same comparative tests as the condenser does, namely, a pressure test of four times the normal working pressure applied to the main terminals of the condenser—not to earth—secondly, an overload of 300 per cent in the working current; and, thirdly, a temperature rise of about 30° C. above the normal full-load working temperature.

Communicated: In estimating the increased running loss due to the use of a phase advancer with 1,300-k.v.a. motor, the increased copper loss in the rotor is the most important item, and in the design worked out by the author amounts at full load to more than 60 per cent. On the other hand, the stator loss is reduced 14 per cent, giving a net increase of nearly 25 per cent of the total copper loss of the motor, say 7 kw. for the 1,300-k.v.a. motor. For the same motor, the copper loss and brush I^2R loss in the phase advancer would amount to about 6 kw. Besides this we have considerable brush friction loss, armature iron loss, bearing friction and windage loss in the phase advancer, as well as extra I^2R loss in the motor slip-ring brushes and leads. I have taken these as 3 kw. in all, thus making the extra loss at full load 16 kw. An important point which affects the running losses is the fact that, if wattless current is to be supplied to the line, the losses will be almost as great at light loads as at full load. The effect of this at load factors considerably less than 100 per cent is the same as if the full-load loss were increased. At 25 per cent load factor the plant would probably run for 10 hours at an average load equal to 60 per cent full load, and the extra energy used would be equivalent to 26 kw. for 6 hours. It appears to me, therefore, that the allowance I have made, viz. 25 kw. at full load, and proportionately less at lower loads, will give results approximately correct for all load factors likely to occur in practice. While the phase advancer is no doubt the best apparatus to use in the case of motors specially designed for it, and in order to supply the wattless current of the motor itself, it does not appear to be economical to supply wattless current to other parts of the system through the rotor and stator of an induction motor. In such cases the condenser would be more economical and would give more satisfactory compensation.

Dr. Silvanus Thompson. Dr. Silvanus P. Thompson: There are, in every one of the various successive developments of electric engineering, points that we may look back upon as having been turning-points in the development of each particular branch. We have now come to such a turning-point, it seems to me, in the use of alternating-current induction motors. Their bad power factor, especially when they are not running at full load, and their limited capacity for standing up against an overload have not prevented them from being very largely employed. And now we have the means given us of making those machines correct their own vices, so that the worse machine may become the better and the cheaper; and the cheaper design may, with a phase advancer added to it, even surpass the dearer machine. I want every one to realize what a very important subject this is, and how greatly it may add to the development of the electrical industry in the immediate future.

Dr. S. P. SMITH : I think these polyphase commutator machines are really the most ingenious apparatus that electrical engineers have ever produced, but their application to the induction motor seems a wrong step. It scarcely seems right to charge up against the induction motor one of its vices when it has so many virtues. It is quite true that low power factor is a vice of the induction motor, but I do not know how any station engineer can expect a consumer, especially a large consumer, to sacrifice the ideal simplicity and reliability of the machine in order to rectify the power factor. If the station engineer wants the power factor rectified let him do it himself. The method is simple. He can easily apply condensers or a synchronous motor at a suitable part of the system. Let us consider the customer's point of view. The most general case for the use of phase advancers will be that of a large motor working off high-tension mains, which alone are suitable for large power transmission. Such a user has in the induction motor the safest possible machine he can get, especially if a squirrel-cage rotor is used. Yet a commutator machine, which cannot be used for large powers because of the high voltage necessary, is brought to him in the form of a phase advancer, so that after making him accept the limitations of alternating current the further limitations of the commutator are to be forced upon him. Speaking as a designer, and a designer generally endeavours to minimize trouble, then I cannot see how the combination of an induction motor with a phase advancer can be put in the hands of unskilled workmen. I believe that in all these large installations the factor of safety has been largely reduced—indeed it might be risky to cut down the size of mains—and the maintenance costs will be much higher. Another small point I want to mention—small compared with the fact that I think it is a fundamental fault to install such a machine—is the effect on the efficiency. If auxiliary apparatus is installed it must have its own losses, and these, of course, add to the cost. With regard to sending a leading power factor into the line, it is expecting too much of the consumer to ask him to provide something for nothing; and as for making him pay for what he does not get, that is to say, according to his power factor, I am afraid this is not the country to adopt that system. Personally I do not think that central stations will refuse to supply power to consumers who decline to install phase advancers. I think the present supply authorities will only be too glad to get the load. I quite agree that in special cases it may be quite advisable, and indeed the only solution of the problem, to put in a phase advancer; but for the supply of power in general I think it is the wrong thing to do. A better plan is for the station engineer to consider the generators. If instead of specifying a generator to regulate well he was willing to have a badly regulating generator, he could get a larger output from the same size of machine. More regulation would certainly be needed, but if he had an automatic regulator that would look after itself. In this way the station engineer can help himself, and indeed is being compelled to help himself at the present day by using large units. With these he dare not have good regulation,

Dr. Smith. because he is afraid of the effects of a short-circuit. After all if the machine can only give 18 per cent regulation, I do not see that the lamps are very much more endangered if the machine gives 28 per cent regulation, for the lamps will burn out in both cases. By sacrificing regulation the station engineer would get a much larger k.v.a. output from the same machine, and thereby be able to supply the motors with the lagging current they need. That, I think, is a much sounder system than running the risk of breakdowns due to the proposed complicated auxiliaries, and the further risk of overloading the whole system from generator to motor in case such breakdowns occur.

Mr. Burnand. Mr. W. E. BURNAND (*communicated*): It is well known that the power factor of electricity supply systems is often as low as 0·6 in the daytime, and the trend is still downwards. If 1-10th of the load is lighting, etc., at unity power factor, and, say, 4-10ths averages 0·8 power factor, the remaining 5-10ths must have a power factor not higher than 0·36. This neglects vectorial phase-differences, which would make the figure still lower. I do not think that many alternating-current central station engineers realize that on about one-half their motor load they only get paid for 1 kilovolt-ampere out of every three supplied. Whilst energy is charged for at the kilowatt-hour rate there is no inducement for a consumer to improve his power factor by using a phase advancer. I suggest, as Professor Kapp has done, that the best way of meeting the requirement is that the consumer's meter should go at a rate intermediate between that of an ampere-hour meter and a watt-hour meter. I would suggest, however, that the wattless component should be charged at bare cost and the true watt component at cost plus profit. This simply means an ordinary induction meter with the shunt adjusted not for exact quadrature, but so that with full-load amperes at zero power factor the meter would run at about one-fifth full-load speed instead of standing still, and also that the meter should register correctly on full load at unity power factor. This would not be difficult, as one of the chief difficulties in the development of the induction meter was to make a meter that had not this characteristic. In this way the adoption of phase advancers would be ensured for most large motors. Even so, however, it leaves the main cause of the low power factor untouched, as for one motor of over 100 h.p. on the mains there are hundreds of smaller motors. These small motors not only in the aggregate total a very much greater horse-power, but individually have lower power factors than larger motors. To deal with them I have developed an arrangement styled, for want of a better name, a "power regulator." This is a transformer with a 3-way and off-position switch arranged to supply different voltages to the motor according to the load. The primary object of this regulator, as far as the user is concerned under present conditions, is to economize power and reduce electricity bills; power-factor should not be mentioned to a non-technical customer in this connection. The regulator depends for its success on the fact that an efficiently designed transformer to reduce the voltage supplied to a

small motor has a smaller loss than the reduction effected in the motor losses. This applies particularly to the small single-phase machine, the advantage of the transformer being somewhat less with the two- or three-phase machines, and decreasing gradually as the horse-power increases. Under special conditions the arrangement effects a saving for motors up to 100 h.p.; but as a general rule it is not advantageous in this respect above about 20 h.p., although there is material improvement in power factor. On machines of, say, 3 h.p. I have usually found a reduction in the electricity bill of at least 10 per cent, and 25 per cent is not at all uncommon. This looks at first unlikely with a motor rated to have an efficiency of about 80 per cent, but it must be remembered that this figure is for full load, the efficiency decreasing to zero at no load, so that there is plenty of scope for big reductions at the lower loads, which, on account of the low power factors prevailing, it is obvious are the rule and not the exception. Mr. Burnand.

As is well known the exciting volt-amperes taken by a given motor vary as the square of the applied voltage, the torque for a given slip varying at the same rate. The copper losses vary as the square of the total current passing through the motor, and the iron losses at a slightly lower rate. If, therefore, the voltage supplied to the motor is reduced with decreasing load in proportion to the square root of the load, the efficiency—so far as the electrical losses are concerned—would remain nearly constant. The mechanical losses, which remain at nearly their original value, and the transformer losses, combine to reduce the efficiency on light load; but there is still a substantial balance to the good with the regulator. This introduces no extra complications whatever, but merely consists of a strengthening up of the weakest link—the transformer of the auto-starter.

Moreover, the manipulation is simplified. If a motor shows any tendency to slow down, the switch is simply moved up another notch. This is quite instinctive, and no special instructions are issued to the workmen. I find a suitable arrangement is about half voltage on the first notch, about three-fourths on the second, and full voltage on the top notch, the transformer being then cut out of circuit. I have also tried four and five "live" notches; but the great majority of cases are quite as well met with the three usually supplied. If this, or any other arrangement, is to be of service on the power-factor question it must fill three requirements: (*a*) It must actually give the results claimed; (*b*) it must be readily saleable; and (*c*) it must be used in practice efficiently. A few instances and figures in this connection may be of interest. In the first place, outside my own works I tried one of these regulators; it was put in circuit with a 15-h.p. motor obviously much underloaded; we overshot the mark in voltage reduction, however, with the result that although the amperes taken from the supply were reduced to one-third their former value the meters only showed about 5 per cent reduction instead of about the 25 per cent anticipated. I also ran as an experiment a 40-h.p. motor in my works on a load averaging about 2 h.p. and usually taken by a 5-h.p. motor. Supplying

Mr. Burnand.

about one-third full voltage to this motor it used, including transformer losses, only about 6 per cent more energy than the small motor, based on a week's run; without the voltage reduction, however, it could hardly have failed to double the cost. Another instance is a 36-h.p. machine which has to deal with a very fluctuating load, running up to 80 h.p. for a few minutes (this load altogether being on for probably not more than one week per annum) and averaging about 8 h.p. On the load that may be taken as a fair average I find that the current taken from the mains is reduced by no less than 12 k.v.a., and the energy recorded by the supply meter by 1¼ unit per hour, by running on the lower notches of the regulator. Assuming 1,500 hours per year on these lower notches, this means £8 7s. per annum, which is not a bad return on the £5 extra cost that this regulator means over a plain auto-starter. If the 12 k.v.a. reduction in non-revenue-producing amperes is worth only 1/10d. per k.v.a.-hour to the supply department it still comes out at £7 10s. per annum. As to saleability, of 100 motors sent out from my works, and of sizes ranging from 120 h.p. to 1 h.p., the average being 5 h.p., I find that no less than 92 motors were supplied with these regulators. In fact, I can scarcely remember an instance where we have recommended these regulators, even only for trial, where they have not been used and retained. I can also testify to the fact that they are actually used in practice. I recently got an assistant to call at a few works, not picked in any way, and of 17 machines he saw at work 5 had the regulator on the first notch, corresponding roughly to a reduction of magnetizing current of about 70 per cent; 8 were on the second notch, corresponding to about 40 per cent reduction; and 4 were on the top notch. It must be remembered that this system is no competitor of the phase advancer; but if it were more generally adopted for small motors, and the phase advancer for large motors, there would be an end to low-power-factor troubles. With regard to the meter suggested, the point may be raised that the units registered are not Board of Trade units, so that no definite legal charge can be based thereon. This may be so; but how many Board of Trade units are there in a charge based on the telephone system or on the rateable value of a house? The meter suggested would charge the 0·5 power-factor consumer in the ratio of 12 to 10, compared with the unity-power-factor consumer. This can scarcely be called excessive, and the Board of Trade would not be likely to hold out long against that charge if it agrees to the other tariffs.

Mr. Krämer.

Mr. C. Krämer (*communicated*): The cascade systems invented by me, viz. (1) rotary converter with continuous-current cascade motor, and (2) three-phase system with commutator cascade motor, have been developed primarily for speed control. Both can be used for power-factor correction, and the rotary converter with cascade motor especially affords an easy means of adjusting the power factor by merely altering the exciting current of the converter. Fig. C shows this correction for a 1,500 b.h.p. motor carrying a constant load of 1,030 kw.

Mr. Krämer.

The synchronous speed of the motor is 230 r.p.m., and by means of the cascade control the speed can be reduced to 150 r.p.m., the test shown in the diagram being made at about 205 r.p.m. As will be seen from Fig. C, the characteristic of the cascade set is very similar to the V characteristic of a synchronous machine, and it will be noticed that in this case with 7 amperes exciting current the power factor is 0·8 lagging, and with 30 amperes exciting current 0·8 leading. The speeds of the main motor, which are of course dependent on the excitation of the auxiliary motor, are not appreciably influenced by an altera-

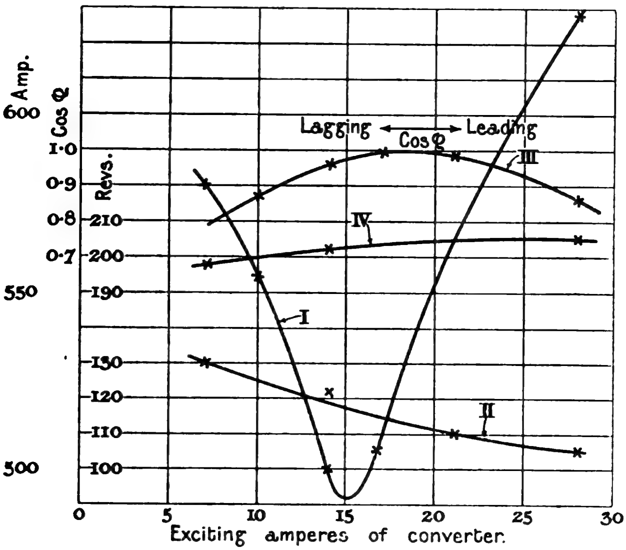

FIG. C.

Curve I. Amperes of converter. Curve II. Revs. of converter.
Curve III. Cos ϕ of induction motor. Curve IV. Revs. of induction motor.

tion in the excitation of the converter. The power factor of a commutator cascade motor, alternative (2) above, can be controlled in a similar manner. The A.E.G. Electric Company usually design these sets for unity power factor, the power factor not being much altered by variations in load or speed. It is interesting to note that the same objections which were raised against cascade-speed-control sets are now applied to power-factor-correcting sets, namely, that the simplicity and reliability of the installation are impaired by introducing additional apparatus. In my opinion this is not a sufficient reason for abandoning the use of power-factor-correcting sets if substantial saving is effected by their use, particularly as the main plant is not disabled through any defect in connection with the auxiliary apparatus. It seems to me that the standpoint frequently

Mr. Krämer. taken up by electrical engineers, namely, that an increase in the number of apparatus should be avoided under all conditions, is not always justified. Steam engineers do not seem to be so afraid of introducing additional gear. Imagine two steam plants, one without feed-water heaters and with single-cylinder non-condensing engines, and the other with all modern devices for feed-water heating and with triple-expansion condensing engines. Which is the simpler plant and which type is being adopted? Experience shows that the economical conditions of speed control and similar features are being much more appreciated now than they were only a short time ago, in spite of the apparent complication. The A.E.G. Electric Company have supplied and are building three-phase variable-speed plants aggregating 49,000

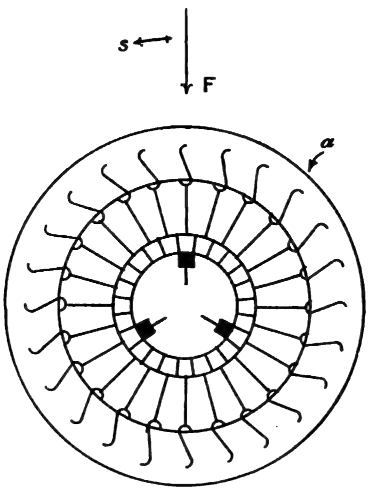

FIG. D.

b.h.p. output, among which are commutator cascade sets of 3,000 b.h.p. individual output, 200/100 r.p.m. No doubt similar development will take place in regard to power-factor-correction sets. For sub-station plant the synchronous motor will always be preferred, but no doubt the phase advancer will find a useful field of application in connection with low-speed induction motors driving winding engines and mine pumps.

DISCUSSION BEFORE THE BIRMINGHAM LOCAL SECTION ON 8TH JANUARY, 1913.

Dr. Wall. Dr. T. F. WALL: The study of the phase advancer is of great interest from a scientific point of view, apart from the particular application to the induction motor, because it shows us how to

Dr. Wall.

produce a condenser effect by machines other than the synchronous motor. Professor Walker has referred to what is known as the Scherbius arrangement; I find that the action of this type of phase advancer may be explained in a simple manner as follows: The apparatus may be represented as in Fig. D, where *a* represents a ring of iron provided with holes in which a winding is arranged, in a manner similar to that of a ring wound armature of a continuous-current machine with embedded active conductors. (In practice the winding is really a drum type, but a ring winding is easier to illustrate in a diagram.) The ring and commutator are fixed on a shaft which may be driven. Three brushes are provided on the commutator, displaced relatively to one another by 120 electrical degrees, and these brushes receive the currents of slip frequency from the secondary of the induction motor. If the ring be stationary it acts like a 3-phase choking coil, the currents producing a field F which revolves in space at a speed of, say, *s* revs. per second, corresponding to the frequency of the currents supplied to the brushes. The vector diagram for the phase E.M.F., E, at the brushes

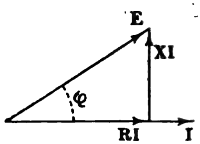

FIG. E.

FIG. F.

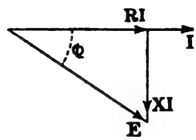

FIG. G.

and the current, I, fed to each brush will be as shown in Fig. E, in which R I is the component to overcome resistance and X I is the component to overcome the E.M.F. produced by the revolving field F (*i.e.* the E.M.F. of self-induction). Now suppose the apparatus be driven in the same direction as that in which the field F revolves, the speed of rotation of F in space will remain unchanged, being independent of the speed of rotation of the ring because the points at which the currents are led into the winding are fixed in space. Hence when the ring is driven at the speed *s* revs. per second in the direction in which F revolves, the relative motion of the field F and the winding becomes zero; this is equivalent to the disappearance of the self-induction effect. The vector diagram of the phase E.M.F., E, at the brushes and the current, I, fed to each brush becomes as shown in Fig. F, that is, the current is now in phase with the E.M.F. If the ring be driven at a higher speed than *s*, the X I component of Fig. E becomes reversed in sign, and the vector diagram will be as shown in Fig. G., that is, the current will lead on the E.M.F. The angle of lead may be adjusted by adjusting the speed at which the apparatus is driven.

Mr. Shuttleworth.

Mr. N. SHUTTLEWORTH: I should like Professor Walker to enlarge upon Fig. 10, as it does not by any means agree with my view of the case, which I will endeavour briefly to explain. For simplicity we may assume that the phase advancer has no resistance or reactance, in which case the point X will coincide with the point E_a. I understand that O E_a represents the resistance drop of the full-load watt current in the main rotor; if then we draw (Fig. H) a horizontal line from E_a to cut O*a* in Q we should have E_a Q as the ideal voltage to inject, since it represents the resistance drop of the wattless current required. This voltage is at right angles to the rotor voltage, which is variable according to the slip, hence it will control the wattless current entirely; in other words, we must not introduce a horizontal component greater than E_a Q when full-load watt current is flowing,

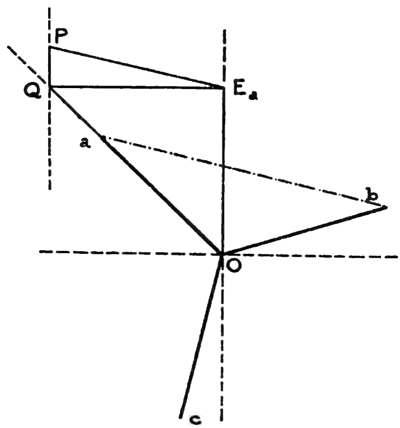

FIG. H.

otherwise we shall have a larger wattless current than is desired. We may now represent the phase of the advancer E.M.F. by drawing from E_a a line parallel to *a b*, and by cutting this in P with a vertical from Q, it is clear that the magnitude E_a P has the correct horizontal component for producing the wattless current necessary; and the other component Q P will be immediately neutralized by a speeding up of the main motor. It would seem, therefore, that E_a P is the correct magnitude of E.M.F. for this particular phase to introduce. For any other phase relation it is simply necessary to draw from E_a a line representing that phase to cut the vertical through Q. According to Professor Walker's diagram the required magnitude is obtained by the phase line cutting O*a*. Professor Walker also lays down certain rules to be observed in the design of a machine of this kind. Paragraphs 5 and 6 refer to compensation of the armature, and to the provision of a commutating flux in phase with and proportional to the current

to be commutated. There is no further mention of this, and it would naturally be inferred that the design adopted complies fully with these conditions. Mr. Shuttleworth.

Again, I should like the author's opinion of what I conceive to be the true operation of his machine. While the compensation provided on any one mechanical pole is opposed to the armature magnetomotive force in its total effect, it is not perfect from point to point along the armature periphery; this allows of local fluxes along the pole face, and at the ends the fringes spoken of are formed. It is interesting to note, however, that the fringes at the tips of the same mechanical pole are not of the same phase. By using an armature of slightly less than 120° pitch, the author uses these fringes as a commutating flux, depending upon the instantaneous sum of the two to give the proper phase. Now this would appear correct, provided the sides of the coil undergoing commutation were moving in a density uniform along the periphery in its distribution. The fringe density, of course, is not uniform, hence the desired phase is not obtained; there is always another disturbing component. This is immediately evident by considering the beginning of commutation when the coil is moving in one fringe only, and the end of commutation when the coil moves in the other fringe; neither of them separately is of the right phase, and the amplitude and phase of their resultant varies from moment to moment. In spite of this, I think that an improvement is to be obtained by such a device, and I have no doubt that the machine as laid out would commutate sparklessly, for the simple reason that the reactance voltage of commutation is only of the order of 0·5 to 0·6 of a volt. I estimate that in this design the author is using rather an excessive fringe density, especially so in view of the fact that it is difficult to make any adjustment on test, as the movement of the brushes into different parts of the fringe would make no material difference. In conclusion, I would ask Professor Walker why he has not adopted the Leblanc machine with a slight modification. In spite of the fact that there are six commutating spaces per pair of poles, it can be built with an output coefficient not less than that of his machine; and with a suitable concentrated compensation in the commutating zone it has theoretically perfect commutating properties.

Mr. G. SHEARING: The advantages to be obtained from the use of a phase advancer to improve the power factor of an induction motor are illustrated by the following results of tests made on a Scherbius 3-phase induction motor. The motor is of 25 h.p., 50 frequency, 450 volts, having a synchronous speed of 1,000 revs. per minute. The rotor is the primary and the phase advancer is fixed on one end of the rotor shaft. The advancer itself is a 3-phase commutator dynamo with no stator field, the exciting flux being produced by the armature itself. The stator terminals are connected to three brushes on the commutator; in this way the necessary leading E.M.F. Mr. Shearing.

Mr. Shearing.

is injected into the secondary winding. Tests were made at 505 and 460 volts. In one test at 505 volts the advancer was cut out, in the other the advancer was in circuit, as also in the test at 460 volts. The relation between the power factor and kilowatt input is shown by the curves, Fig. J; and the conclusions to be made from the test are favourable to the use of phase advancers. The motor is very light for the output. Its net weight is 798 lb., representing 14·5 kg. per horse-power of rated load. From makers' figures I find that as an average value for a motor, having no advancer, of the same output, frequency, and speed, and a power factor from 86 to 88 per cent, the weight is from 14 to 15 kg. per horse-power.

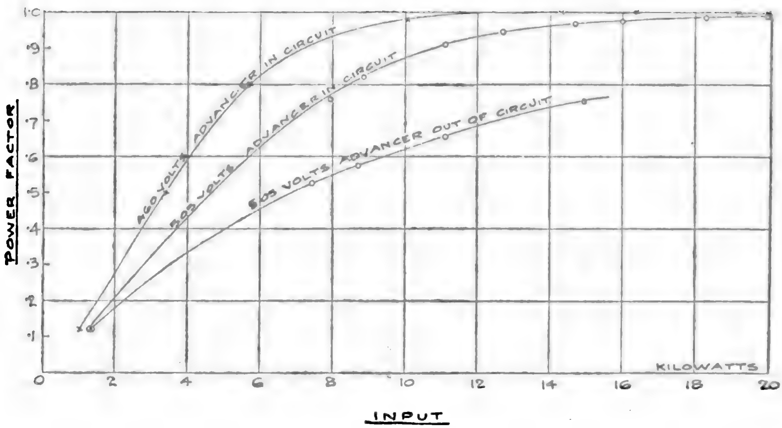

FIG. J.

Thus the decrease in weight of the motor resulting from the use of the advancer compensates for the increase due to the weight of the advancer itself. Without an advancer, to secure a high power factor the air-gap has to be very small. With its use the air-gap need not be so small, and the mechanical difficulties associated with a small air-gap disappear. For these reasons the cost of the motor and phase advancer should not necessarily be greater than that of a motor with no advancer.

The efficiency of the motor is 89 per cent at full load. The additional losses due to the brushes and ohmic resistance of the advancer are thus small; and the gain resulting from the reduction of the current for a given output is greater than the additional losses. There is no sparking at the brushes at full load when the line current to the advancer is about 130 amperes. The magnitude of the leading E.M.F. is 5·4 volts at full load, at half load 4·9 volts, and with motor running light it is 3 volts, showing the tendency to saturation of the magnetic circuit of the advancer. It is interesting to note that the

power factors obtained were less at the higher voltage. This follows from the vector diagram, assuming, as an approximation, that the current diagram is a circle. In Fig. K, A B C is the current circle for the lower voltage, and A′ B′ C′ that for the higher voltage. For a given input as represented by the height of the horizontal line, I I, the phase angle is greater for the higher voltage over the working range of the motor. The greatest improvement in the power factor is at the higher loads. When lightly loaded the power factor is somewhat low; and the advantages of the advancer would be increased if by some means the power factor could be made higher at light loads with no additional complications. The greater part of the decrease of the leading E.M.F. of the advancer takes place between no load and about one-third full load. For these loads the E.M.F. is approximately propor- Mr. Shearing.

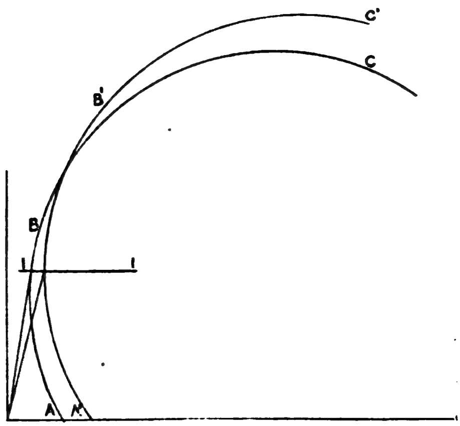

Fig. K.

tional to the current, and the power factor decreases considerably. The author will perhaps say whether, with advancers of this type, it is possible to obtain a high power factor at low loads as well as at the higher loads.

Mr. A. M. Taylor (*communicated*): The question of cost is of the greatest importance. The difficulty, as has already been pointed out by Dr. Kapp, is to find out who is to pay for the phase advancer—the customer or the power company. The following appear to be approximately the costs of the different methods now available for improving the power factor: A large synchronous motor costs something like £1 10s. per kilowatt of power dealt with; Prof. Walker's arrangement probably comes to about 5s. or 10s. per kilowatt; whilst the use of Moscicki condensers, according to the figures given by Mr. Mordey, costs about £5 10s. per kilowatt where the voltage is 5,000 and the periodicity 25, and the power factor has to be raised from 0·8 to unity. This seems to put the condenser entirely out of the running when compared with the Mr. Taylor.

Mr. Taylor. two other arrangements. It seems possible to reduce the cost of the condenser considerably, if of the glass-insulated type, by raising the voltage at which it is worked. I suggest that for this purpose a static transformer be employed, whose secondary is wound to give a very high voltage, the transformer discharging into the condenser. As the size of the condenser required varies inversely with the square of the voltage, there may be possibilities in the arrangement if practicable. A difficulty with the author's phase advancer is that it appears necessary to use it with an induction motor; and I believe it cannot be installed on any part of the system by itself.

Professor Walker. Professor MILES WALKER (*in reply*): Professor Kapp has rightly called attention to many advantages which the phase advancer gives us besides those mentioned in this paper, but I must remind him that I referred at the beginning of the paper to one written four years ago, of which this paper is only a continuation. I think if Professor Kapp looks at the first paper he will find that the question of the maximum torque of the motor being increased is fully dealt with, and also the question of efficiency is gone into with some minuteness. One point of great importance is the fact that the stator copper loss, which may amount to 1¼ per cent, can be reduced to only 1 per cent, while the rotor copper-loss, which is somewhat increased, can be very easily provided for by having a little more copper in the rotor. It will pay to do that, because as a matter of fact if we consider the interest on the money spent on the extra copper in the rotor it will be found to amount really to very little indeed. There is always room on the rotor, whereas on the stator there is no room to introduce the extra copper unless the size of the motor is considerably increased. Another most important matter affecting efficiency is the fact that a smaller frame can be used and a smaller motor constructed, and on account of the higher output torque the motor can be worked at a higher rating, that is to say, the same frame is worked all day long at a higher rate. I think it is quite possible that, even with these advantages, in the case of comparatively small motors there will still be a slight loss to set against the phase advancer. I admit that may be the case, but I believe that with properly designed large motors the total loss of the whole plant will be rather less than if a phase advancer is not used and the same amount of material only employed. I also went into the question of the use of an iron machine as against a copper machine in my earlier paper, and commercial questions in general were considered, particularly the saving of copper in the mains, etc., which is after all the greatest thing we are aiming at. I take it that the main reason why we want to get our power distributed at unity power factor is on account of the capital cost of the mains. That is far greater than the cost of machinery. The cost of the power house and the electrical plant is practically nothing compared with the cost of the copper in the mains, and what is wanted is to raise the power factor to unity. The way to do that is not, as Dr. Smith says, to install generators that will take the load,

Professor Walker.

but to take care that the customer gives unity power factor. If one customer must have a lagging power factor, then if some other customer next door can be persuaded to give a slightly leading power factor and thus equalize the leading and lagging currents, that is a good policy. This brings me to the use of the phase advancer when the motor is working on a light load. It is quite true that with the Scherbius phase advancer with which Professor Kapp dealt the power factor is not so good at light load. The machine is designed to give unity power factor, say, from half load to full load. But we are not obliged to have that characteristic. We can by choosing the right

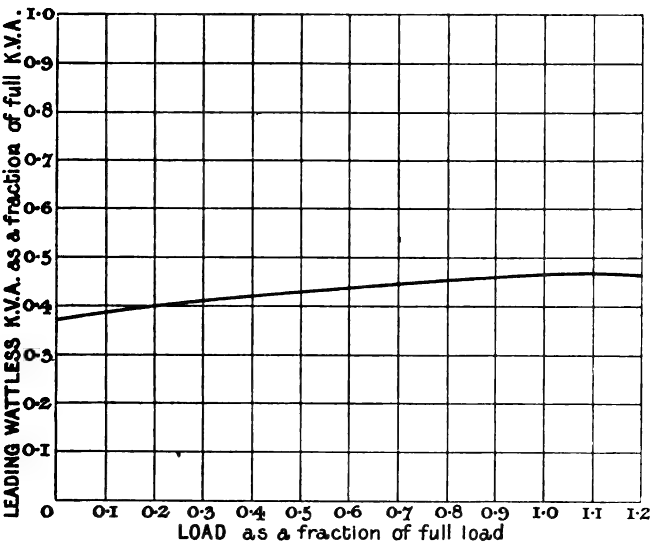

FIG. L.

kind of excitation, as I have shown in the previous paper, make the induction motor take a fairly large leading current at no load. We are not so much concerned with the figure which expresses the power factor at various loads as the figure which gives us the amount of leading current taken at various loads. A phase advancer provided with a series winding such as that described in the paper has the characteristics of an ordinary series generator. Below a certain speed it will not excite itself. Above that speed it excites itself and runs up in voltage until the field is saturated. Such a machine run at a high enough speed over-excites the rotor even at no load and makes the stator take a leading current. The power factor is then fairly low and leading. As the load comes on, the leading current increases slightly, but the power factor comes nearer to unity. This has been done. (See the table of tests given in the paper.) It is quite possible

Professor Walker.

to have a phase advancer with a characteristic like that shown in Fig. L. That is to say, at light loads the machine is running with a very low power factor, all leading. We have plotted instead of power factor the amount of leading kilovolt-amperes. Let the abscissæ be load and let the ordinates be leading kilovolt-amperes. The right characteristic to get is the one shown, and that is quite easily obtained with a phase advancer provided with a field winding. When the motor is running light it is feeding a leading current of a certain amount. Perhaps an 800-h.p. motor could feed 300 k.v.a. leading into the mains, neutralizing the bad power factor of other motors in the vicinity; and as the load comes on (notwithstanding that the leading current increases slightly) the power factor becomes nearly unity. That characteristic is, I think, one that is to be aimed at in many cases, and it can be easily obtained by means of the right phase advancer, either of the Scherbius type or the British Westinghouse type.

With regard to Mr. Ashton's remarks about condensers, I am very glad to hear that condensers can be built so cheaply, and that they can give such a good performance. He must not suppose for a moment that I am sorry to hear that he is in competition. I think there is room for everybody to deal with the question of the improvement of the power factor. He says that for a 50-cycle circuit we can buy condensers at £2·8 per k.v.a. I think he must be taking the theoretical price at which they ought to be supplied. On the other hand he puts the price of a 30-k.v.a. machine running at 490 revs. per minute at £250, surely a very high price. It is more than sufficient to cover the cost of the advancer and 60 per cent more copper in the rotor circuit (a matter of £55), which if put in would make the total losses in the motor some 20 per cent less than they are without the phase advancer. If this extra copper were added (and one is never pinched for room for copper in the rotor in the design of an induction motor) the saving in the motor would entirely compensate for the losses in the advancer, so the £485 per annum falls to zero. While I think that Mr. Ashton has put the case of the advancer rather unfairly, I am prepared to admit that the condenser, if it can be supplied cheaply enough, is an excellent proposition. One advantage—and it is a very big advantage—that the condenser has over the phase advancer is that it can be used without any induction motor. The factor of safety, however, is a very important matter. Condensers to be acceptable to the station engineer must be built with a very large factor of safety; there must be no danger of their heating up and short-circuiting.

In connection with this we must remember that the phase advancer is a machine which is absolutely independent of the motor. It can be shut down at any time desired. If anything were to go wrong with it, it is simply short-circuited and shut down, the motor running on without it. In that respect it is even better than the exciter of a synchronous motor. No one thinks of installing a synchronous motor without fitting an exciter to it, and a synchronous motor can be

Professor Walker.

run on the mains perfectly well without an exciter. Why does not every man who has a synchronous motor say, "I will not have a commutating machine in my station. I will scrap my exciter and draw the magnetizing current from the mains." Many synchronous motors will run without any exciter at a very low power factor. As soon as it is demonstrated that the phase advancer commutates as well as the average exciter it will be used for improving the power factor of an asynchronous motor just as the exciter is used to improve the power factor in the case of the synchronous motor.

Mr. Shuttleworth's statement with respect to Fig. 10 is quite correct; the smallest injected E.M.F. which will give us the required leading current is an E.M.F. at right angles to $O E_a$, such as the E.M.F. $E_a Q$ in his figure. Now the E.M.F. $E_a P$ is one which is very easily obtained in the manner described in the paper, and it does not differ from $E_a Q$ by a very great percentage. The effect of the additional component Q P is to reduce somewhat the slip of the motor so that we get rather more power out of the motor, and this compensates for the extra power taken to drive the phase advancer by reason of the existence of the component Q P. In reply to the observations upon commutation, I would point out that the main flux from the horn of the pole which is used for commutation is strictly in phase with the current to be commutated. The more one rocks the brushes forward the stronger the effective commutating field becomes. The limb of the coil which is not in this field is put as near as possible in the neutral part of the field between two poles. The brushes can be rocked over an arc quite great enough for adjustment without fear of this limb getting into too strong a field of the wrong phase. Even if we rock the brushes too far forward the unused limb of the coil only comes into a very weak field which differs by 60 degrees from the phase of the correct commutating field. The fact that the E.M.F.'s of the armature and the compensating winding do not balance one another at every point is of little importance; it introduces a disturbance which is a small quantity of the second order. If phase advancers can be built to commutate without compensating windings at all, a fortiori they can be made to commutate with windings which only give 90 per cent of the required field correction. I would advise the use of the simple Le Blanc advancer without field windings up to sizes which admit of good commutation. As the size is increased it will be found that the more complex machine with a field winding is required.

Proceedings of the Five Hundred and Forty-eighth Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 23rd January, 1913—Mr. W. Duddell, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 9th January, 1913, were taken as read, and confirmed.

The list of candidates for election and transfer approved by the Council for ballot was taken as read, and it was ordered that it should be suspended in the Hall.

Messrs. H. C. May and G. Y. Fraser were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

Julian Cleveland Smith.
John Joseph Stewart.

As Associate Members.

James Allan.
Amos Aspinall.
Charles Tertius Astbury.
Frank Birch.
John Wesley Burleigh.
Henry Talbot Byford.
George Cardwell.
Alfred Clark.
Hugh Cochrane.
W. Roylands Cooper.
Harry Thornber Cort.
William John Cross.
Edward Lancelot Eastgate.
Francis Thomas C. Emberton.
Frederic Andrew Fitzpayne.
Frank Marshall Fletcher.
Lewis Vivian Fox.
William Charles Fripp.
Ernest Arthur Gatehouse.
William Percival Gauvain.
William Grant.
George William Harris.
Samuel Trail Jameson.
Edward Cecil Jennings.
Walter Lawson.
Walter Cecil Moore.
Robert Leechman Morrison.
Ernest Müller.
John Campbell Murray.
William Ferrier T. Pinkney.

ELECTIONS (*continued*).

As Associate Members—continued.

John Thomas Turney Randles.
Clifford Clare Rattey.
Albert Robert Reeves.
Henry Neville Rodgers.
Charles William Salt.
Hjalmar Rudman Schultz.
Robert Scrivener.
William Shead.
Frederick George Shrewsbury.
Bertram Hardy Smith.
George Herbert Spittle.
Harry Edgar Street.
George Sykes.
William Edmund Turner.
Reginald Weaving.
Thomas Welch.
Frank Herbert Wigner.
Douglas Marshall Williamson.
Percival Wood.
William Wood.

As Associates.

Herbert Laidlow Downes.
Sir Henry Norman, M.P.
Percy Fitz-Patrick Rowell.
Eric Sinkinson.

As Graduates.

William Henry Griffin.
Cecil Cyprian Higgens.
Selby Baldock Howard.
Thomas William Howard.
George Edward Pearson.
Sydney Charles Potts.
Edwin Borton Roscoe.
U. P. Roy.
John Osborne Spong.
Douglas Gordon Thomson.
John Wilfred Treherne.
Thomas Eaton Ward.
Matthew Montagu Wyatt.

As Students.

John Thornton Bedford.
Aubrey Edward Walter Butler.
Vernon Lionel Frederic Davis.
Albert Henry Deimel.
Edgar Edminson.
Frank Frost.
William Gwennap.
Harold Vienna Higby.
Harold Honey.
George Alexander Keith.
Roy Goodwin Kilburne.
Harold Willoughby Looker.
Arthur Thomas Mahon.
Victor Henry Maurel.
John Harper Meacock.
James Norman Metcalf.
Abd el-Khalic Motawi.
Stephen Edward Pritt.
John Renton.
Harold Riley.
Basil Samuel Saunders Rockey.
John Robert Rutherford.
Mehemed Sadick.
Chandra Sekhar Sarkar.
Cecil Arthur Smiles.
Herbert Robert Sparrow.
Leslie Craven Speed.
Adrian Francis Sykes.
Kenneth Baring Taylor.
Harold Richard Tuppen, B.Sc.
Randle Henry Neville Vaudrey.
Charles Norman Vernon.
Anthony Joseph Waugh.
Stanley Birkett West.
George Backhouse Whitaker.
Arthur Willcock.
Cyril Victor Clarence Wright.

TRANSFERS.

From the class of Associate Members to that of Members :—

Frederick Arthur Bond.
Percy Vivian Gray.
Joseph Josephs.
Frederick Edward Kennard.
Walter Lockhart Maxwell.
Arthur James Mayne.
Frederick Steell Robertson.
Lewis Anderson Smart.
Rupert Stanley.
George Stevenson.
Harold Dalrymple Symons.
Joseph Patrick Tierney.
Reginald Norman Torpy.
William Bradley Woodhouse.

From the class of Associates to that of Members :—

Frederic Horton Clough.
Edward Henderson Freeman.
Charles Woodward Neele.
David Thomas Powell.
James Boyd Shield.

From the class of Associates to that of Associate Members :—

George Bradwell.
Edward George Brown.
Herbert Dean.
Alfred Eddington.
Thomas Gillies.
Algernon Coste Gilling.
Frank Clements Knowles.
Percy Wright Paget.
Thomas Gregory Smith.
Frank Thomas Wright.

From the class of Associates to that of Graduates :—

Lewis Oswald Monson.

From the class of students to that of Associate Members :—

Douglas Betts.
Valentine Frederick Bush.
William Edward Flower.
Daniel Harrop.
Herbert Ferrier Jefferson.
Laurence Walter Johnson.
Reginald Otto Kapp.
Harold Kingsbury.
Arnold Lewin.
Percy Tindell Maybury.
Eugene Orloff.
John Henry Palmer.
Gesner Clovis C. Sharples.
Benjamin Spalding Smith.
Herbert Alexander Stewart.
George Henry Taylor.
Morice Ord Teague.
Richard Alan S. Thwaites.
Leslie Newton Vine.
Arthur George Warren.
James Warren.
Edgar Whitehouse.

From the class of Students to that of Associates :—

Harry Greenwood.

From the class of Students to that of Graduates :—

Montagu Barrington Baker.
Edwin Guthrie Bowers.
William Bridger.
William Galloway Conner.
Angus Dow Mackinnon.
Ronald William Manifold.
James Miller.
Ronald Atkinson Nuttall.
David Victor Oppenheim.
David Edwin Parton.
Arthur Cobbold Smith.

A paper by Mr. F. H. Whysall, Associate Member, entitled "The Use of a Large Lighting Battery in Connection with Central Station Supply" (see page 376), was read and discussed, and the meeting adjourned at 9.40 p.m.

THE USE OF A LARGE LIGHTING BATTERY IN CONNECTION WITH CENTRAL STATION SUPPLY.

By F. H. WHYSALL, Associate Member.

(*Paper first received 7th December*, 1912, *and in final form 3rd January*, 1913 ; *read before the* INSTITUTION *23rd January, and before the* MANCHESTER LOCAL SECTION, *28th January*, 1913.)

SYNOPSIS.

This paper is for the most part based on the results obtained over two complete years' working of the 12,000 ampere-hour battery installed at the Manchester Corporation Electricity Works, Dickinson-street, in March, 1910. This battery was at the time of its installation the largest ever constructed, and has a maximum discharge capacity of over 15,000 amperes.

INTRODUCTION.

In times gone by, this Institution has dealt repeatedly with the question of secondary batteries ; but most of such papers have been concerned with the construction or behaviour of batteries without special reference to the particular object for which they may have been installed. It will readily be realized that before so large a battery as that at the Manchester Corporation Electricity Works, Dickinson-street, could be recommended, the saving to be effected by its use had to be most thoroughly and carefully examined. Not only had the initial cost of the battery to be compared with that of an alternative increase of generating plant, but the method of using a battery, whether on lighting or traction and over what periods, had to be decided.

It is now two years since the battery was installed, and the object of the present paper is to show to what extent the predictions as to its use have been fulfilled, and what relief has been obtained in the cost per unit supplied. The author thinks it will be admitted that the battery has thoroughly justified itself during this period, and the figures in this paper should tend to encourage other engineers of central stations to go and do likewise.

Although details of the battery arrangements have already been published, it is necessary, in order that this paper may be complete and clear, to put down for reference the more important data of the equipment.

Technical Description of the Battery.

The battery consists of 210 cells, each cell containing 38 positive plates and 39 negative plates of the following dimensions:—

Positive plates, 20¾ in. wide × 29 in. deep × 0·4 in. thick.
Negative plates, 20¾ in. wide × 29 in. deep × 0·31 in. thick.

The positive plates are of the Planté formation, cast in one piece, but the negative plates are of the improved box type composed of half grids securely riveted together, the spaces between them being filled with active material. Specially treated wooden separators are employed between adjacent plates, and a free space of 8 in. is left at the bottom of the cell for the accumulation of deposit.

The cell boxes are of pitch-pine lined with lead, the outside dimensions of each box being as follows:—

Length, 6 ft. 1½ in.; width, 2 ft. 2½ in.; height, 3 ft. 4½ in.

The following are the guaranteed performances of the cells:—

Maximum discharge rate, 15,000 amperes.
One-hour discharge rate, 8,400 amperes (3,000 kw.).
Charging rate, 4,100 amperes.
Maximum charging rate, 6,500 amperes.
„ „ voltage, 2·75 volts per cell.
Ampere-hour efficiency, 90 per cent.
Watt-hour efficiency, 66 per cent. at 1-hour rate (8,400 amperes).
„ „ 75 per cent. at 3-hour rate (3,900 amperes).
Final voltage per cell, 1·67 volts at 1-hour rate.
„ „ 1·78 volts at 3-hour rate.
Weight of each cell and acid complete, 2 tons 19 cwt.
„ complete battery, 620 tons.
Floor space occupied, 5,966 sq. ft.

Regulating Cells, Boosters, and Methods of Charging and Discharging.

The chief advantage attached to the use of regulating cells is that the output of the battery is not limited by the capacity of the boosting

plant; and in cases where a battery is used as stand-by, regulating cells have distinct advantages over boosters. On the other hand, with large batteries the switchgear for regulating cells becomes, with its connections, a very cumbersome piece of apparatus, and the usual practice is to have booster regulators, automatic or hand-regulated, except for stand-by batteries and batteries of small size. Traction batteries always have automatic boosters, and in all cases where boosters are used it should be possible to parallel the battery without boosters in circuit under breakdown conditions.

The reversible booster enables all the cells to be used equally, and avoids the necessity of cutting out regulating cells when fully charged.

With larger batteries the cost of reversible booster and battery is less than for battery with regulating cells plus charging booster, which is necessary in any case. The efficiency in both cases is about the same.

In the United States of America batteries are frequently installed large enough to supply the whole load of the system for a short time in case of a total failure of the generating plant; but this is not usual in this country. In such cases regulation by regulating cells is adopted, and in extreme emergencies the battery can be called upon to give an enormous discharge far exceeding its normal rating. It is important that the discharge should not be limited by the carrying capacity of the booster; a regulating switch can be overloaded to a much greater extent without any serious injury.

Further, when batteries are used only in case of breakdown, if a reversible booster were used either the booster would have to run continuously with no load, or else when a sudden breakdown occurred the battery would not be immediately available. Where a battery is put in to reduce fluctuations in the load on the generators it is also looked upon as a stand-by, but this is not its principal function; further, at times of heavy load the reversible booster would in any case be running.

The chief duty of the Dickinson-street battery is to take 3,000 kw. off the lighting peak. It is also looked upon as a stand-by. But its chief duty is load-levelling; and it was therefore decided to have three hand-regulated reversible boosters, as shown in Diagram A, Fig 1, and to run them in parallel at times of maximum discharge. At other times, one or two would be used as required. It may be noted, however, that such importance is attached to the question of overload in emergency that it is the universal custom on the Continent to use regulating cells in all central-station batteries.

In this country, the first large batteries with which reversible boosters were used were those installed by the Charing Cross, West End and City Electricity Supply Company some twelve years ago; in fact, these were the first important central-station batteries installed for taking the peak load. In this case the transmission is by high tension, and a battery unit giving about 400 to 500 kw. is installed in

each of the seven sub-stations. Since that time the number of important batteries has been steadily increasing; but they are all used in the same way, with one exception, where a combination of regulating cells and reversible booster has been adopted so that the battery can be dis-

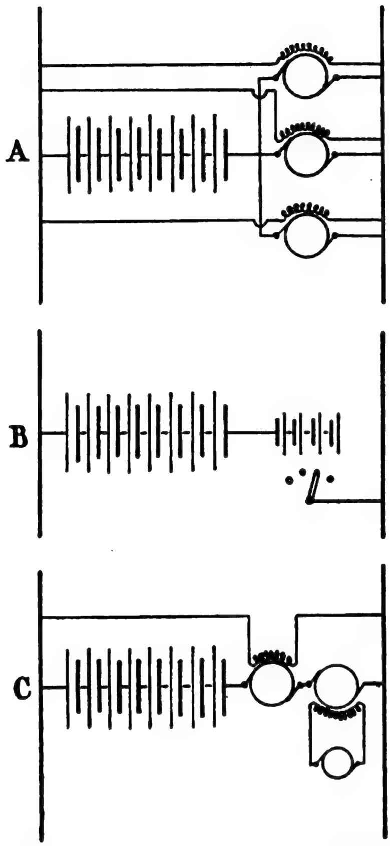

FIG. I.—Diagram showing alternative methods proposed for Battery Regulation.

charged and regulated to a limited extent, even when the booster is not running.

At Dickinson-street the battery was also arranged for use on the traction supply; and for this duty a series-parallel arrangement was made of an extra busbar and change-over switches for one booster.

Short-circuiting switches are provided for all booster busbars, these switches being used to cut out the boosters under breakdown conditions, or when boosters are not required, for instance on Sundays, when

the battery does the whole of the lighting load for the greater portion of the day.

The lighting load is much bigger than the traction load, and for some time it has been found more economical for that reason to confine the use of the battery to the lighting load. The boosters worked quite satisfactorily on traction with automatic exciter regulation as shown in diagram C, Fig. 1, but the saving effected in works costs was not so great as when the battery was used on lighting alone.

It was originally intended to charge the battery on traction, thereby providing a constant load for one 1,800-kw. traction generator, and enabling it to do the whole of the traction load for the city area by discharging over the traction peaks; also to change the battery over to lighting for the lighting peak. It was found that at certain times of the year the traction and lighting peaks were coincident, and for this and other reasons stated, its use is now confined to lighting. When the traction and lighting peaks are not coincident the battery now virtually does both, because it is made to take up the duty of the set which is changed over from the lighting system to traction to supply the traction peak load.

The boosters are of the Turnbull-McLeod automatic reversible type, and were manufactured by the Lancashire Dynamo and Motor Company. They have equalizing rings to every turn of the armature windings, on account of the heavy circulating currents, and the yokes of the boosters are laminated. Each booster is capable of a maximum boost of 5,600 amperes at 80 volts for a few minutes.

Switchgear.—The switchgear consists of 10 panels:—

One main battery panel;
Three booster generator panels, 1, 2, and 3;
Three booster motor panels;
One diverter panel;
One booster short-circuiting panel; and
One exciter panel.

A diagram of connections is shown in Fig. 2.

Two substantial circuit-breakers are inserted in the main cables as close as possible to the battery house. These circuit-breakers are of the magnetic blow-out type, and are capable of carrying 15,000 amperes continuously. Each is enclosed in a separate concrete cubicle so as entirely to shield it from all other parts of the station, and each is electrically and independently operated from the switchboard by means of solenoids.

The circuit-breakers are non-automatic, and are provided with a suitable controller to show by means of signal lamps on the operating panels whether the circuit-breaker is closed or open. The large circuit-breakers on the main switchboard are of a similar type, but mechanically operated from the front of the panels. The circuit-breakers themselves, however, are also enclosed in concrete cubicles. The switchboard circuit-breakers are automatic,

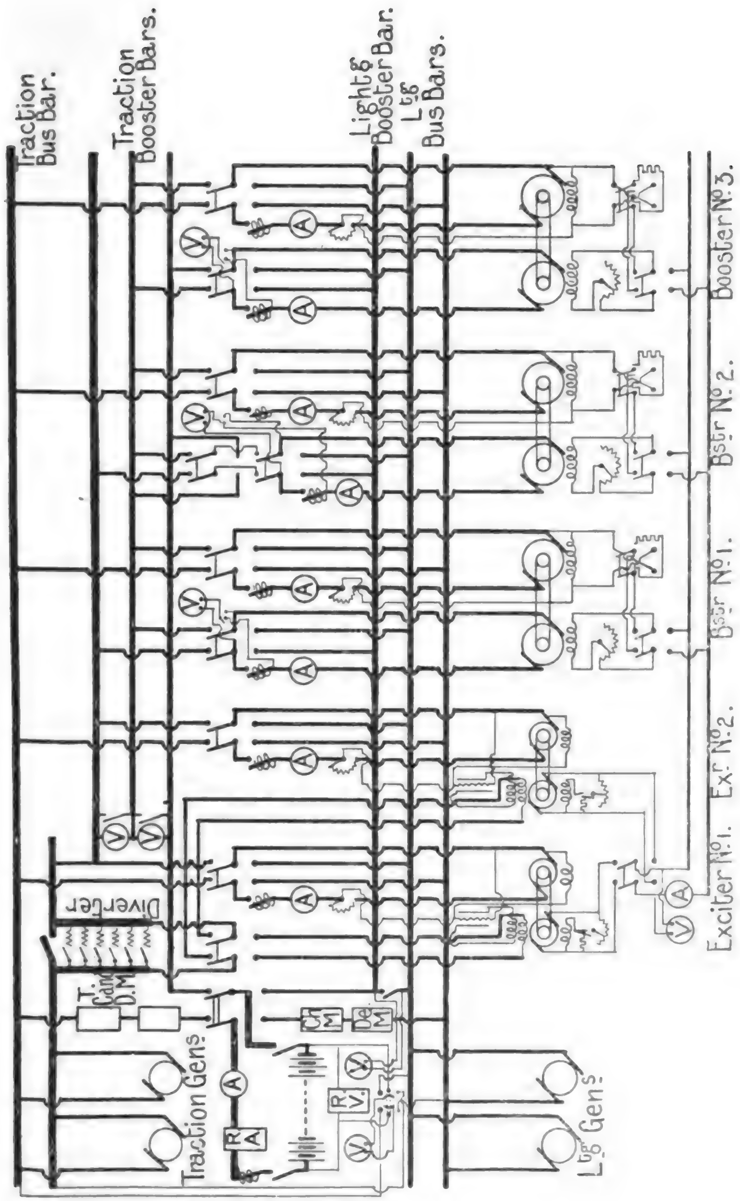

FIG. 2.—Diagram of Battery-board Connections, Dickinson-street.

The busbars and connections are carried on a very substantial iron framework at the back of the main board; and here, again, the lay-out has been designed with the object of separating the various busbars by enclosing each in a concrete trough. Busbars of aluminium have been employed throughout.

The main 15,000-ampere change-over switches are constructed of a number of parallel strips clamped together with suitable distance-pieces between, and these interleave with contacts of similar construction. Bolts pass through both contacts, and these can be tightened up when the switch is closed, making excellent contact.

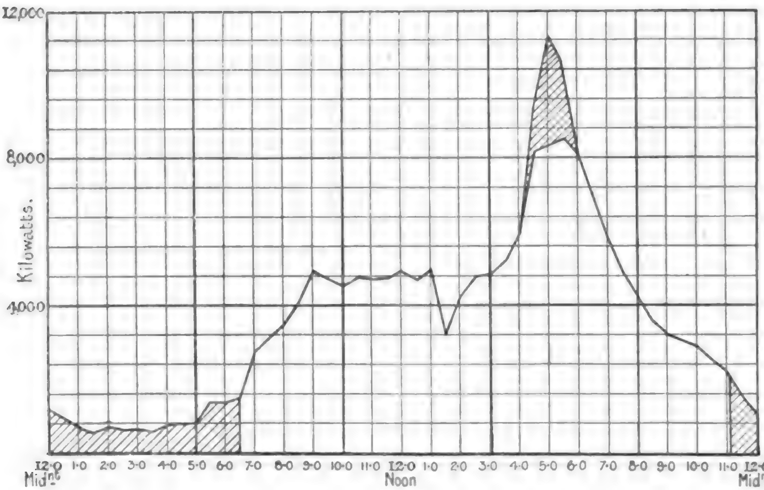

FIG. 3.—Load Curve (without battery).

The proposed use of a battery is shown by the "hatched" portions of the curve.

Load factor = 32 per cent. $\left(\text{Load factor} = \dfrac{\text{units generated} \times 100}{\text{max. demand} \times \text{time}}\right)$.

FIELD FOR ECONOMIES IN CENTRAL-STATION PRACTICE.

The improvement effected in the load factor of steam plant really covers everything; but a list of items under this head may be given in detail as follows :—

- Saving in stand-by boilers required : Equivalent to 1-hour rating of battery, owing to extra time available to raise steam, and owing to possibility of applying load gradually.
- Saving in capital expenditure, and corresponding annual charges thereon.
- Saving in wages of running staff.
- Saving in cost of peak-load units.
- Saving due to "buffering," *i.e.* load-levelling, enabling all units to run at their most economical load.

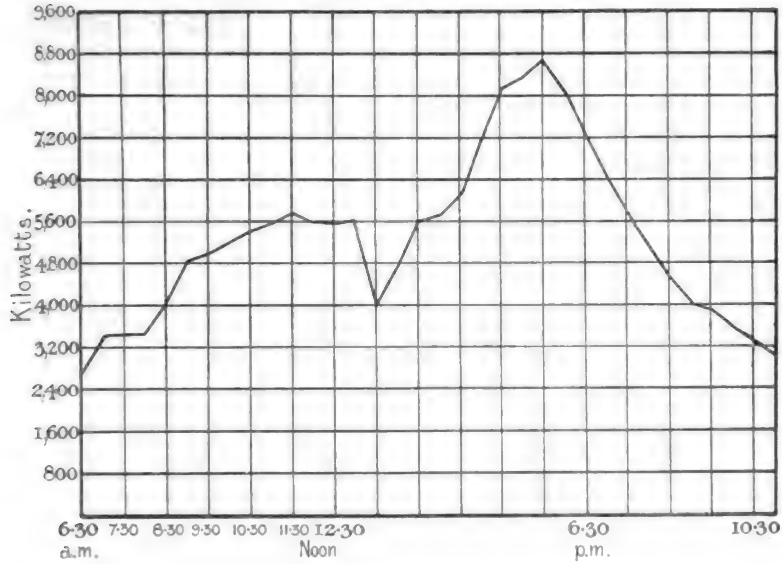

FIG. 4.—Load Curve (with battery in use).

This curve shows the load on the steam plant.
Station load factor = 43·5 per cent.

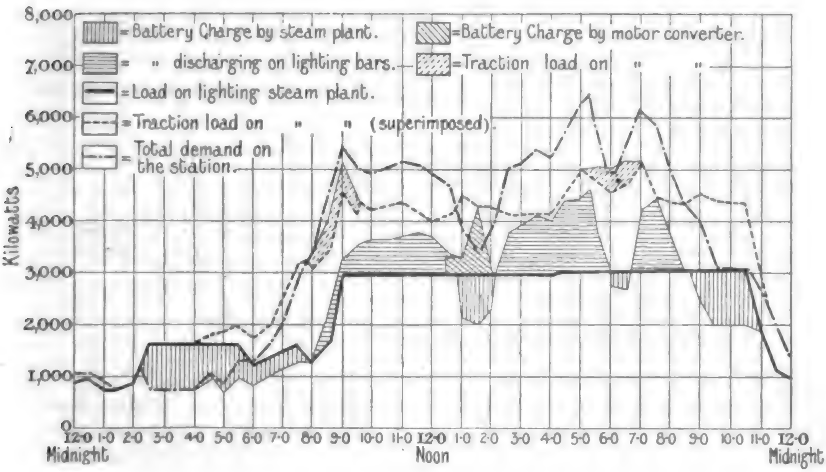

FIG. 5.—Summer Load Curve.

Load factor on steam plant = 67·5 per cent.
" " max. demand = 54·4 " "

Improvement in load factor = 13·1 per cent. (all due to battery).

Number of units generated = 83,500.
Max. load on steam plant = 5,150 kw.
Max. demand on station = 6,400 kw.

The values of all these items can be variously ascertained as detailed in the following text, and usefully checked against each other. It will also be seen how engineers may safely estimate the possible saving to be expected of any proposed battery installation.

In Fig. 3 is shown a typical winter-load curve at the time of the installation of the Dickinson-street battery, the hatched portion of the curve representing the duty proposed. Fig. 4 shows the expected resultant load curve for steam plant.

The load factor in Fig. 3 works out at 32 per cent, and is improved to 43·5 per cent in Fig. 4. In Figs. 5 and 6 we have typical summer-

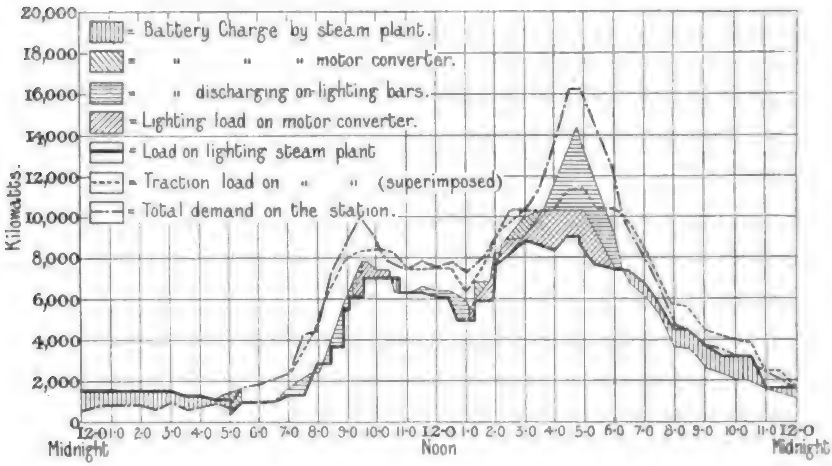

FIG. 6.—Winter Load Curve.

Load factor on steam plant = 49·1 per cent.
" " on max. demand = 34·4 " "

Improvement in load factor = 14·7 per cent. (Proportion due to battery alone 9·2 per cent.)

Peak discharge = 11,000 ampere-hours. Max. demand = 9,000 amps.
Number of units generated = 134,080.
Max. load on steam plant = 11,375 kw.
Max. demand on station = 16,250 kw.

and winter-load curves since the installation of the battery, and it will be noted that the load factors obtained are actually much greater than those anticipated, and are respectively 67·5 per cent and 49·1 per cent.

In Fig. 7 a chart is given showing the effect of load factor on coal consumption and works costs at Dickinson-street and Bloom-street works. This chart has been plotted from actual results obtained over a period of eight years, from 1904 to date, and checked by over 100 monthly observations. This process has eliminated very largely any irregularities which might have occurred in the curve due to abnormal conditions apart from the effect of load factor.

Dickinson-street is now considered an old station, and the largest units at Dickinson-street and Bloom-street stations are of not more than 1,800-kw. capacity. Attention should be drawn to the fact that the observed coal consumption per unit—of the combined stations—has been down to the low figure shown on the chart. The lowest figure for a monthly observation per unit generated since the installation of the battery is 2·66 lb., and per unit sent out 2·79 lb., the difference being accounted for by units used at the Works and units lost in the

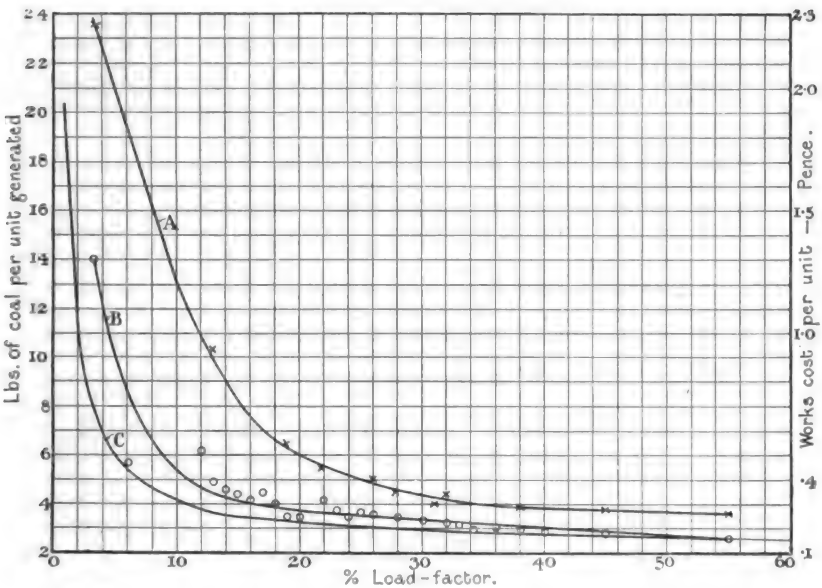

FIG. 7—Curves showing effect of Load Factor on Coal Consumption and Works Costs at Dickinson-street and Bloom-street Electricity Works, Manchester.

Curve A : Works cost per unit generated in pence.
Curve B : Pounds of coal per unit generated (from actual observations).
Curve C : Pounds of coal per unit generated (by formula).

battery. The commercial efficiency of the battery was for the first year 70·6 per cent, and for the second year 71·1 per cent.

It is quite unfair to consider a battery capable of saving only the stand-by represented by its 1-hour rating capacity. In the storm-load chart, Fig. 8, the maximum demand (ignoring traction) was 44·5 per cent greater than the boiler capacity at the commencement of the darkness; and the battery was able to take care of the rising load as shown, ahead of the extra boilers which had to be got into commission. Without the battery it would have been impossible to get these extra boilers up in time. In other words, it would have been impossible to

keep steam, and would have meant practically a total failure of supply unless some portion of the load could have been cut off. What might be described as the elasticity of the system due to the battery makes the question of boiler stand-by much easier, and less boiler plant is required to meet a given load when such load can be put on gradually.

It is possible to get up 20 boilers in the same time as one, given the necessary men, and what happens in practice is this: When it is seen that extra boilers will be required they are lighted up, and the battery is eased by the running plant until the steam pressure begins to fall. The battery is then called upon to supply the rising load until the extra boilers are steaming. This process may be repeated and other boilers got away, the constant effort of the engineers-in-charge under these circumstances being to get the battery off discharge and to keep it fully charged in reserve to meet the final peak of the load.

Capital Cost.

The increased use of large batteries in central stations during the last few years has been chiefly due to reduction in first cost owing to improvements in design, *i.e.* a greater percentage of lead employed is active material; for instance, positive plates have probably two or three times the surface of plates of the same weight made twenty years ago.

There is also a reduction in capital cost of manufacture, principally in the formation, the present rapid processes taking only a few days, instead of many weeks, as formerly. Other improvements in manufacture are the use of better methods and tools, in keeping with general industrial progress.

There is also a reduction in cost of upkeep and improved reliability; but these come under another heading.

When the Dickinson-street battery was proposed, the estimated comparison of capital costs was as follows:—

The hatched portion of Fig. 3, previously mentioned, shows the portion of load which it was thought desirable to transfer from the generating plant to the battery, representing the peak load on a two-hour base, or all load of less than 8 per cent daily load factor. By measuring the mean area of this hatched portion, we arrived at the "units of output," *e.g.* on the curve shown the figure is 3,000 kilowatt-hours, equivalent to about 7,000 amperes for one hour. With this datum as a basis, it was easy to estimate the size of battery required. Thus:—

Rate.	Output.	Equivalent in Amperes.
1 hour	3,000 kw.	8,400
2 hours	2,000 "	5,000
3 "	1,500 "	4,000
Emergency,		
½ hour	4,500 "	11,500
5 minutes	6,000 "	15,000

APITAL COST OF GENERATING PLANT FOR 3,000 KW.

	£
3,000-kw. steam plant at Stuart-street (excluding buildings but including all else)	36,000
High-tension cables for 3,000 kw.	5,000
3,000-kw. sub-station at Dickinson-street	10,000
Total	£51,000

	£
3,000-kw. battery plant at Dickinson-street (at 1-hour rate of discharge, and including boosters and switchgear)	18,000
Buildings	2,000
Total	£20,000

Difference in capital expenditure: £51,000 — £20,000 = £31,000.

INTEREST AND SINKING FUND.

Steam plant: £51,000 to be repaid in, say, twenty years:—

Interest	3½ per cent.
Sinking Fund ...	3¾ „ „
	7¼ per cent. = £3,698

Battery: £20,000 to be repaid in, say, ten years:—

Interest	3½ per cent.
Sinking Fund ...	8¾ „ „
	12¼ per cent. = £2,450

Difference in favour of battery... ... ... £1,248

The actual costs came out as follows, using the same figures for steam plant for Stuart-street, high-tension cables, and sub-station as before.

ACTUAL COST OF BATTERY PLANT, DICKINSON-STREET.

3,000-kw. battery at 1-hour rate of discharge, including boosters and switchgear.

	£	Loan Period.
Storage battery	15,034	7 years.
3-motor booster combination	2,776	15 „
Switchgear	1,757	15 „
Buildings	2,000	20 „
Total	£21,567	

Estimated steam plant expenditure, as above	51,000
Actual cost of battery plant	21,567
Difference in capital expenditure	£29,433

Difference in Interest and Sinking Fund.

Steam plant: £51,000 to be repaid in, say, twenty years:—
£3,698 (as above)

£21,567 to be repaid in ten years:—

Interest	$3\frac{1}{2}$ per cent.
Sinking Fund	$8\frac{3}{4}$ " "
	$12\frac{1}{4}$ per cent. = £2,642

Saving in capital charges per annum in favour of battery £1,056

Comparison of Cost per Kilowatt of Capacity.

	Estimated.	Actual.
Generating plant	£17 0 0	£16 16 6
Battery	6 13 4	7 3 9

In these days of turbo-driven units, the capital cost might be taken at a lower figure.

Below are given estimated costs by Mr. J. F. C. Snell in his book on "Power House Design."

Lay-out of Proposed 120,000-kw. Power House at Barking Creek for Bulk Supply to the County of London.

Rated output of Power House, 120,000 kw.; Overload, 150,000 kw.

	Capital Cost.	Cost per Kilowatt.	
		Rated.	Overload.
	£	£	£
Land	50,000	0·41	0·33
Buildings	344,000	2·87	2·30
River work and pier	102,000	0·85	0·68
Coal and ash plant	59,000	0·49	0·40
Boilers and economizers ...	199,000	1·66	1·33
Pipework and pump	105,000	0·88	0·70
Turbo-generator and condenser	430,000	3·58	2·86
Switchgear, etc.	67,000	0·56	0·44
Engineering expenses and contingencies	115,200	0·96	0·77
Total	1,471,200	12·26	9·81

General.

Average cost of power house buildings in large cities, £6 11s. per kilowatt.

Average cost of power house buildings in smaller provincial towns, £3 5s. to £6 per kilowatt.

Average cost for larger power houses outside cities, £2 5s. to £3 per kilowatt.

Generally speaking, the cost of plain substantial buildings may be reckoned for estimating purposes approximately as follows :—

£					
5·00	per kw.	for	2,500	kw.	installed.
4·00	,,	,,	5,000	,,	,,
3·25	,,	,,	10,000	,,	,,
2·75	,,	,,	25,000	,,	,,
2·50	,,	,,	50,000	,,	,,
2·33	,,	,,	100,000	,,	,,

Costs compiled from the latest actual figures available are estimated by the author as follows :—

	Total Cost per Kw. £	s.	d.
Land	0	5	6
All buildings (including offices)	3	13	0
Land for railway, railway complete and locomotive	1	9	0
Coal and ash plant	0	4	0
Marine boilers with superheaters, including economizers, coal and ash chutes, scraper and stoker driving	2	13	0
Foundations and flues for marine boilers	0	9	0
High pressure steel pipes and covering, including valves. (Main range not included)	0	2	6
Turbo-alternator and condensing plant, pipework for condensing plant and motor-driven fans	3	5	6
Foundations for turbo-alternator and air ducts	0	4	0
Pipework for turbo-alternator, including circulating water pipes, atmospheric exhaust pipes and valves	0	5	0
Natural draught cooling towers	0	13	0
Foundations for cooling towers	0	5	0
High-tension cable	1	8	0
Converting plant, including switchgear	2	0	0
Total	£16	16	6

Total plant installed ... ... 56,500 kw.

It should be noted that Mr. Snell's figure of £12·26 per kilowatt does not include items in the author's estimate amounting to £5 4s., which would bring up the cost per kilowatt on his estimate to £17·46.

It would seem, therefore, that for peak-load duty of two hours

(which averages out at the 1-hour rate of discharge for the battery) the cost per kilowatt can be safely estimated at £17, which leaves a margin of £9 16s. 3d. in favour of the battery.

Running Costs.

It is necessary when considering the cost of generation of peak-load units to take into consideration the fact that the most uneconomical units are reserved for this duty, and quite rightly so, also that these peak-load sets will not be run at their most economical load; consequently the steam consumption will be high. Bare fuel cost per 1,000 kw.

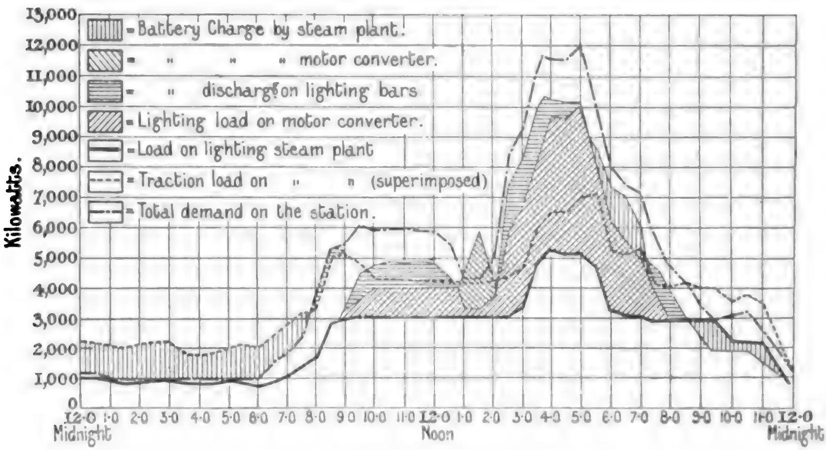

Fig. 8.—Storm Load Curve.

Load factor on steam plant	= 55·0 per cent
" " on max. demand	= 32·6 "
Improvement in load factor	= 22·4 per cent.
Number of units generated	= 93,700.
Max. load on steam plant	= 7,100 kw.
Max. demand on station	= 11,980 kw.

stand-by for this peak-load duty the author has estimated at £7 per week, or reckoning five days per week, £1 8s. per day. This latter charge becomes in the case of a 3,000-kw. battery £4 4s. per day for the bare 1-hour rating of the battery. But we have seen previously, when considering the effect of the battery on load factor, that it is unfair to credit the battery with stand-by savings on its bare capacity only; and in Fig. 8, showing the storm-load curve, it is demonstrated that the battery saved stand-by to the extent of 44·5 per cent on the total boilers under steam at the beginning of the day, also that more boilers are necessary to meet sudden demands than can comfortably cope with an equivalent load applied gradually. We may safely assume, then, that on this occasion a 3,000-kw. battery saved stand-by

for its own capacity plus 44·5 per cent of the boiler capacity prior to the storm, which was 7,200 kw. On this, 44·5 per cent is 3,200 kw. Calculating at the same rate without allowing anything for the increased price of coal, we see that our 3,000-kw. battery at this time of the year, say, the five summer months from 31st March to 1st September, is equal to steam stand-by at the rate of £8 8s. per day. Reckoning on the basis of five days per week gives £924. Adding £630 to this on account of the stand-by saving for its own capacity for the other seven months, we have a yearly stand-by saving of £1,554.

Below are some observations at the city stations in corroboration of the author's estimate of stand-by charges over some months when steam stand-by had to be maintained. These figures were obtained by comparing the corresponding periods of the year previous when stand-by was not maintained.

Month.	Tons of Coal.	Cost.	Stand-by.	Cost per Week per 1,000 Kw.
		£.	Kw.	£ s.
April, 1911... ...	488	255	8,000	8 0
May, „	203	105	5,000	5 5
June, „	388	200	5,000	10 0
July, „	504	263	8,000	8 0

It must be admitted that it is only fair to charge the battery with bare fuel costs corresponding to the input, because there is an actual saving on running fixed charges on the station when a battery is put in. The author supports this statement by saying that at Dickinson-street there are now twenty-two fewer men for an increased demand since the installation of the battery. Comparing bare fuel cost per unit generated, and crediting the battery with a commercial efficiency of 70 per cent, we get a cost per unit (reckoning 2·5 lb. of coal per unit at 12s. 5d. per ton) of 0·237d. against a cost of 1·55d. taken from the actual results shown on the load-factor curve for 8 per cent load factor. The peak-load units discharged by the battery since its installation, calculated at the rate of five complete discharges per week—31st March, 1910, to 31st March, 1912—are 1,560,000, and represent a saving of £8,534. This takes no account of the morning discharges and load-levelling duty during the rest of the day, commonly called "buffering," whereby each set on load is kept running at its most economical output, and sets do not need switching in and out to meet the vagaries of the demand. This can also be estimated from the load-factor chart.

The improvement in load factor on the units generated, observed monthly over two years, is approximately 7½ per cent, and the value

obtained from the chart is 0·08d. on 30¼ million units; this represents a saving of £10,166.

We have, then, two distinct annual savings on running charges due to the battery: (1) Stand-by boiler fuel costs; and (2) difference between steam generation at 8 per cent load factor and fuel bare cost—and as a check on these we have the saving indicated on the load-factor curve due to the improved load factor, which to be accurate should be reckoned for each month separately. This is the actual improvement due to the battery, and should cover both the other estimates and include the saving due to the buffering effect as well, because the later observations shown in Fig. 7 were made after the installation of the battery. In fact, estimated savings from the load-factor curve should cover all savings on running costs when taken on the whole output. It should be noted here that there is a theoretical curve drawn in on the load-factor chart, based on a test figure obtained for the city stations of 2·5 lb. per kilowatt for 100 per cent load factor, and the known stand-by fuel charges as previously detailed. It is necessary also to mention that when Stuart-street works act as stand-by to the city stations allowance is made and included in the fuel costs, from which the actual curve of cost is drawn. The whole of any improvement shown is therefore due to the battery alone. These remarks do not apply to the actual coal load-factor curve, which benefits to some extent from the use of the sub-station, and accounts for its nearness to the theoretical curve. The lowest observations, however, on both the coal and cost curves were of course made some years before the installation of the battery, and the lowest one of all was made when the plant was running non-condensing at Dickinson-street (on account of alterations to the circulating water culvert). For condensing, of course these two points would be lowered 33⅓ approximately per cent.

TABLE SHOWING SAVING ON WORKS COSTS FOR ONE YEAR.

Year.	Output to Feeders.	Coal used.	Average Price of Coal.	Increase in Coal Cost, 1910.	Total Cost per Unit Output to Feeders.	Saving in 1910 against 1909.	Total Value of Saving effected by Battery with Coal at 10s. 11d.	Commercial Efficiency (see p. 395).
	Kw.-hours.	Tons.	s. d.	£.		£.	£.	Per Cent.
1909. No battery...	25,818,992	40,681	10 11	—	0·368	—	—	—
1910. With battery	29,042,985	39,418	11 0	164	0·305	7,620	7,784	70·6

NOTE.—Calorific value varies between 13,500 and 14,000 B.Th.U.'s per lb.

ACTUAL RESULTS WITH DICKINSON-STREET BATTERY.

In the table the actual saving on works costs for one complete year's working is given, showing the saving actually made. Nothing

has been said about battery maintenance so far; but it should be noted here that the makers have signed a contract to maintain the battery at its rated capacity for a term of fifteen years for the sum of £1,250 per annum, which is 8·3 per cent per annum on the purchase price. This maintenance is included in the works costs.

In Fig. 9 the works costs for the Manchester city stations (Dickinson-street and Bloom-street) are given for ten years. The improvement shown for the last year is entirely due to the installation of the battery.

Summary.

Capital Cost.

Saving in favour of battery: First cost, £29,433; annual charges, £1,056.

Annual Running Cost.

Saving in favour of battery:—

	£
As per first year's working under favourable conditions	7,620
Total annual saving in favour of battery	8,676
The estimated saving from calculated standby value was per annum ...	1,554
From difference in cost of generation between fuel cost and cost at 8 per cent load factor	4,267
(Does not include saving due to buffering)	£5,821
The estimated saving from load-factor improvement was	£10,166

and it must be admitted that the two different ways of estimating the saving check very well with the actual results.

Load-factor Curve.

A load-factor curve showing fuel values can be constructed for any works providing the stand-by fuel charges are ascertained for a representative boiler, and the fuel consumption per kilowatt-hour at 100 per cent load factor is known for a representative unit.

The formula used by the author is as follows:—

$$x = \frac{c}{y} + k,$$

where—

x = lb. of coal per unit,
c = stand-by fuel per kilowatt capacity,
k = running fuel per kilowatt capacity,
y = load factor.

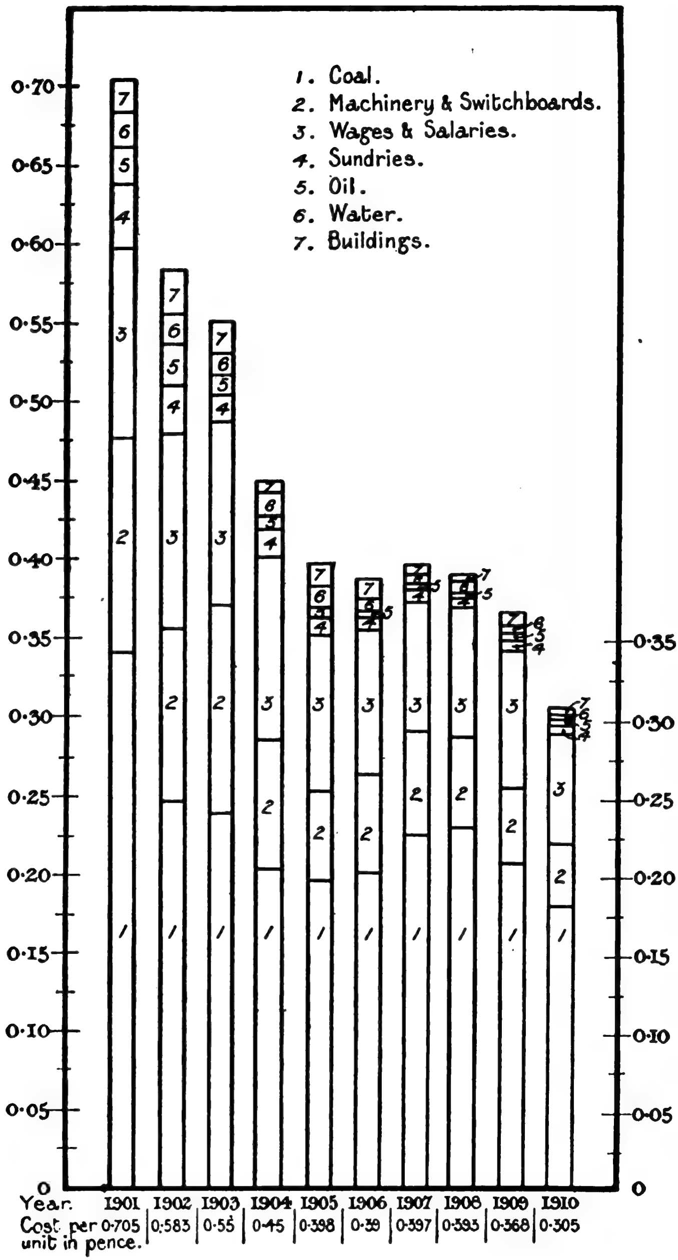

FIG. 9.—Dickinson-street and Bloom-street combined Works Costs.

Example for city stations :—

$$c = 0{\cdot}18 \text{ lb.}, \quad k = 2{\cdot}32 \text{ lb.}, \quad c + k = 2{\cdot}5,$$
$$y = 50 \text{ per cent.} = 0{\cdot}5,$$
$$x = \frac{0{\cdot}18}{0{\cdot}5} + 2{\cdot}32 = 0{\cdot}36 + 2{\cdot}32,$$
$$= 2{\cdot}68 \text{ lb. per kilowatt-hour.}$$

Records and Care in Use.

In the first place, a recording ammeter and a recording voltmeter are essential in order to see that overcharging or overdischarging does not take place; the former is shown by the voltage line at the end of charge, and the latter can be ascertained by comparing the discharge rates shown on the ammeter chart with the corresponding final volts shown on the voltmeter record. Overcharging is, of course, a matter of definition, and depends on the state of the cells. Charging is continued under normal conditions at Dickinson-street 10 minutes after gassing point is reached, before both daily discharges, and for 2 hours once each week, generally on Saturday, and preceding the day (Sunday) when a complete discharge at the 10-hour rating is usually taken out of the battery.

It is the custom to construct daily load curves at Dickinson-street like those shown in Figs. 5, 6, and 8, except that colours are used to indicate the various areas. These charts have a very important moral effect, and are daily placed in a frame on the battery switchboard for inspection and for future guidance.

The efficiency of the battery for the day is worked out thus :—

$$\frac{\text{Units output}}{\text{Units input, including booster units}},$$

which we term the "commercial efficiency."

The improvement in load factor on the steam plant is noted, also the ampere-hours of the peak-load discharge. These figures are put on the load curve, and it is easy to see at a glance if the battery is not being used to the best advantage. These load curves are considered the most important records taken, because the value of a battery depends more on the way it is used than on anything else.

A special battery-book is kept, giving details of cell defects and their treatment (for which a 1,000-ampere portable "milking" booster is used), daily gassing, weekly overcharge, and all specific gravities and voltages of individual cells once each week after the weekly overcharge. Three pilot cells are used to serve as a guide to the state of charge, and the specific gravities of these cells are taken every half-hour for the station log sheet, which is the general record of the working of the station usually found in use at most central stations.

The capacity of the battery varies at various rates of discharge, and

without these pilot-cell readings it would be exceedingly difficult at times to gauge the exact state of the battery.

General Conclusions.

Advantages and Disadvantages.—When considering the value of storage batteries in connection with central station supply, it has been the general custom in the past to compare the cost per kilowatt of capacity with that of steam plant and generator, to the advantage of the latter. It was argued that steam plant and generator cost less, and could be run continuously if required, whereas a battery (if the 1-hour rating was used for comparison) was not to be relied on for longer periods than one hour. It was, and is still, held that the life of a battery would be less than half that of generating plant, that maintenance is exceedingly heavy, operation difficult, and still—speaking generally—a prolific source of worry and trouble.

Batteries were installed, when central stations were first built, large enough to maintain the supply during the night, and at the week-ends, without the assistance of running plant. They were found a great convenience; but generators would be added as the demand on the station grew, and the battery would fall out of use, except for balancing purposes, until, through neglect and ill-usage, it would become of no value to the scheme. In no case would the battery be used except as a luxury, and it was generally expected to require no attention until it got into a very bad state.

Electric traction did something to raise the reputation of storage batteries, owing to the marked saving in plant required for a rapidly fluctuating load, and the resultant economies of this buffering effect.

A properly proportioned battery is supposed to give 1 per cent to 2 per cent better ampere-hour efficiency when used on traction as described, and as against the efficiency of a battery used to deal with lighting peaks, due to the greater buffering effect on traction loads.

It is now being recognized that, provided the battery is installed to reduce generating plant, it is a sound commercial proposition when considered in connection with a large lighting load.

Popular Error about Efficiency.—Too much has been made in the past of the supposed inefficiency of batteries; as a matter of fact 70 per cent to 75 per cent commercial efficiency can be maintained with care; and if this were not so, efficiency is the least important attribute of peak-load plant, and especially so in the case of a battery. Peak-load units are costly to generate, and allowance is made for this in fixing charges for lighting. Power or daylight units are much lower in price because the cost of generation is much less—so in effect the battery is charged at power costs, and discharged at lighting rates. If some of our large consumers on public supply mains only realized this, they would install batteries of their own, buy current at power rates, and cheapen their own lighting supplies by arranging with the supply authorities for a "restricted-hour" supply, *i.e.* they would take no current from the mains at peak-load time.

Batteries hitherto have usually only been installed by consumers for the sake of making supplies reliable and independent of accidents outside their own premises.

Importance of Size and Application.—It does not matter how short a period of time during the day, or the year for that matter, a supply is demanded, there must be plant installed to meet it, and it has been found that for all duty of less than 8 per cent daily load factor, *i.e.* of less than two hours' demand during the 24 hours of the day, the matter is in no doubt. It most certainly pays to make storage battery provision for this. Beyond this point we must look to advantages other than direct saving in capital cost and running charges to justify the extension of the principle.

It is often said, "What about the overload capacity of the steam plant; is not this sufficient to deal with the peak the battery is intended for?" On occasion, yes; but if reliability of supply is to be assured, the occasion must be that brought about by the failure of the battery, a possible but unlikely occasion if proper care is taken.

Booster regulating plant can go wrong, and batteries are protected normally by circuit-breakers which may open through defect. It is generally possible, however, in the event of serious trouble to parallel the battery without the boosters in circuit; and under such circumstances the battery would prevent total failure of supply under the worst conditions. From other causes of failure a battery is practically free.

It is very important that the battery should be of large enough capacity, not only for normal discharges, but for any short, heavy loads it may be called upon to give out. On the other hand, if the battery is larger than would be necessary to deal with load of the proportions indicated, the direct saving it is possible to show is correspondingly decreased.

In conclusion, the author tenders his best thanks to Mr. S. L. Pearce, the City Electrical Engineer of Manchester, for facilities allowed and permission given for the inclusion in this paper of data from official reports, and for the publication of facts and figures concerning the Manchester Corporation electricity works; also to Mr. E. A. Hilton for assistance in getting out the diagrams. The author would also like to make it quite plain that the opinions expressed in the paper are his own personal opinions.

Discussion.

Mr. J. S. Highfield: I propose only to deal with the case of batteries for very large supplies of power and light, and not with the use of batteries in smaller stations. I will not criticize figures, because the conditions vary so much in different undertakings with different conditions as to plant, load, distance from the power station, etc. In my opinion the first use of a battery is the great security provided by utilizing large batteries near the source of the supply, particularly in Mr. Highfield.

Mr. Highfield.

connection with a high-tension transmission system where the supply is given to the consumers in the form of continuous current for power, light, and traction. It is becoming increasingly important to keep the supply absolutely secure so that it is never cut off; the battery gives us an advantage in that respect which no other plant can equal. From that point of view I think the battery is of the greatest importance. Its next use is to carry some part of the peak load. It enables the power station to be run at a greater efficiency; it not only saves plant at the power station in such a system as I am speaking of, but it also relieves the trunk mains and the sub-station machinery. All those elements should be taken into account in considering whether a battery should be used; that is to say, it is necessary to set against the cost of the battery the cost of the plant in the power station, the cost of the trunk mains, and the cost of the sub-station plant. In my opinion a saving can be shown in favour of the use of batteries of considerable size on nearly every system. It must be remembered, however, that the advantage of the battery from the point of view of taking the peak load is antagonistic to its advantage from the point of view of security; because if the batteries are discharged over the peak load, if an accident happens at that time the battery instead of being fully charged is wholly or partly run down. Consequently a compromise must be made, and the battery can be used only partly for each purpose. The method of control is a matter of the greatest importance, and will permit of prolonged discussion; but for the purposes to which I refer—the use of a battery in connection with a lighting and general power load—I think the best solution is to employ a hand-controlled booster for charging and discharging, with a short-circuiting switch, so that in case of a very bad accident where the battery has to be discharged at the highest possible rate, the short-circuiting switch can be rapidly closed either by hand or automatically in order to save the booster from destruction. A great deal of skill is required to make a satisfactory battery booster, that is to say, a machine that will give good results at what may be called normal load, and will also permit of the greatest possible excess load being carried in an emergency. I have machines now of which the normal maximum load is about 1,200 amperes, but which have carried as much as 4,000 amperes without harm. Of course the load is only on for a few minutes—sufficient time to start up other plant—but for those few minutes the boosters carry that very large excess current. I quite confirm what the author says about the efficiency of a battery. I have had no difficulty at all with four large batteries in obtaining efficiencies of between 70 and 75 per cent. I hope we shall hear from the manufacturers of batteries something about the improvements they have made in recent years. I am quite sure there is plenty of scope for further improvement in both the life of the plates and the cost of manufacture. Some advance has been made in providing a larger output with less lead, which of course is in the direction of reducing the cost. Also I am anxious to have a better system of maintenance, and a better method of charging for maintenance. The

present method always handicaps those engineers who look after their batteries best. If the cost of maintenance is averaged out it follows that the careful user of a battery is bound to pay part of the maintenance charges of the less careful user. I wish battery makers would elaborate some satisfactory system so that the payment for maintenance is proportional to the actual cost. Mr. Highfield.

Mr. E. C. McKinnon: I feel that the paper is liable to cause undue optimism, as it should not be assumed that the results obtained at Manchester during the period March, 1910, to March, 1911, can readily be repeated in any station in this country. But the results are so good that they leave a large margin to warrant the more general installation of large batteries in stations still without them. As Mr. J. A. Robertson, of Greenock, once stated, the possession of a battery gives a feeling of security which cannot be expressed in £ s. d.; and Mr. Whysall also points out that efficiency is the least important attribute of peak-load plant. I am sorry to notice that although the battery will have been in use three years in March next, and although the paper is based on results obtained over two complete years' working, only the first year's results have been given. It is mentioned that the commercial efficiency of the battery during the second year was 71·1 per cent, and that coal has increased to 12s. 5d. per ton; later results would therefore be of interest and value. It is possible by heavily overworking a new battery, and stinting the charge, to obtain for a year or two much too high a commercial efficiency, with the result that the life of the plates in the battery is impaired. I do not wish to suggest that batteries have a poor efficiency, but because it has been possible to obtain 71 to 75 per cent efficiency at Manchester that figure should not necessarily be the standard laid down for all stations. It depends so much on the local conditions. The life of a set of plates is proportional to the number of charges and discharges, and cannot be expressed in terms of years. Cost of maintenance should properly be based on the extent to which the battery is used in a given period, as adopted in the United States of America, otherwise maintenance becomes a speculation. The author mentions that the Manchester battery is the largest ever constructed. He should have said the largest in this country, as numerous batteries far larger than that at Manchester were already in use at that time in the United States; in fact, we are a decade behind Americans in this country in the use of large batteries, and it is interesting to note that they employ batteries almost entirely as stand-by plant instead of for daily peak discharges. The Electric Storage Battery Company, of Philadelphia, have contracts with some of the largest American supply companies to maintain their stand-by batteries for 10 years, during which period the battery may be discharged 150–200 times, but it may not be discharged more than 40 times in any one year. In explanation of this limitation, which may appear very small to British engineers, it should be noted that most of the batteries are now of the "Exide" type, of which both positive and negative plates are of the Faure or pasted type. By the use of this type of plates the capacity obtainable from Mr. McKinnon.

Mr. McKinnon.

a battery of the ordinary Planté type can be more than trebled for the same floor-space and weight.

One of the batteries in New York city consists of 152 cells, each containing 133 plates, and has the following capacity:—

11,000 amperes for		1 hour.
22,000	,,	20 minutes.
44,000	,,	brief periods in emergency service.

The voltage is maintained by means of cell switches, two on each side of the three-wire system. The carrying capacity of each of these switches is 5,000 amperes continuously, with a maximum of 20,000 amperes, at which load they can be safely operated. Such switches have been built to carry 10,000 amperes continuously, with a maximum of 40,000 amperes. As an example of the use to which the batteries are put, the following is a quotation from a letter written by the New York Edison Company:—

"On March 2, 1911, at 3.46 p.m., New York city was visited by an unusual storm appearing very suddenly, and as suddenly disappearing, with a very short interim of unusual darkness. Our system load at the time was approximately 75,000 kw., and within less than ten minutes it increased to 125,000 kw., an addition of 50,000 kw. to the load, added at the rate of 5,000 kw. per minute. The rate of increase was so rapid that it was impossible to connect additional engines with sufficient rapidity, and the storage batteries were called upon to meet the emergency to the extent of furnishing 17,000 kw. Notwithstanding the unusual circumstances, the pressure all over the system was maintained at substantially normal value. Twenty-five batteries were discharged."

The author refers to busbars of aluminium. I should like to hear if there has been any trouble with these in the battery room. It is stated that the increased use of large batteries in central stations is chiefly due to the reduction in the first cost and the cost of upkeep, and to improved reliability; I consider, however, a very important reason is that central station engineers have realized nowadays that it pays to look after a battery. I understand that the switchboard attendants at Dickinson-street station are taught that every minute of unnecessary charging represents a loss to the Corporation of so much cash. I suggest that a recording hydrometer should form part of the recording equipment. This is used with many large batteries in America, and with some in this country, and is found most useful. It would save the half-hourly procession between the switchboard gallery and the battery room, and it would also avoid errors of reading. The author's recommendation to large consumers to install batteries and obtain power rates for charging would be warmly welcomed by the battery makers, but it would probably not have the approval of central-station engineers unless extensions to existing peak-load plant were under consideration. If such plant were already installed the engineers would probably want

to find work for it. With regard to what is said on page 397 about the size of the battery, this again is a matter for consideration from all points. A small battery, *i.e.* one just large enough to deal with the load, would be cheap on first cost, but its commercial efficiency and durability would be lower, and the cost of maintenance higher, than for a battery with a reasonable margin. With regard to weight, one tender for the Manchester battery was lower than that accepted, although the cheaper battery was 40 per cent heavier. This was considered an objection by the city architect; but I contend that the heavier a battery is, the more durable it is likely to be, that maintenance costs will be lower, and that during a fifteen-year maintenance period there will be far less disturbance of normal working conditions brought about by repairs to the battery. Mr. McKinnon.

Mr. A. M. TAYLOR: On pages 390 and 391 the author illustrates the saving of coal obtained with a battery. This is a very important point, and I am exceedingly pleased to find that the author's figures practically bear out all the contentions which I have put forward in theory and which are now proved in practice. With regard to keeping plant ready as a stand-by during the summer months, it is apparent from Fig. 8 on page 390 that the storm cloud which came on at about 2 o'clock in the afternoon did so with great rapidity. The load at that time was about 5,000 kw., and it rushed up so that within half an hour it had reached 8,500 kw., an addition of 3,500 kw. in half an hour. The author's contention in the paper is that the battery is useful to the extent of 3,000 kw. in saving stand-by boilers which otherwise would have had to be kept ready throughout the summer; and he calculates that the coal saved is of the order of £4 4s. per day. To these boilers used for stand-by purposes the author adds—and on this point those who have looked into the paper may be disposed to challenge him—the 3,000 kw. of the peak that the batteries have yet in hand; and he says the battery must be credited not only with the saving due to the 3,000 kw. required as shown on the curve, but also with the saving of coal for the other 3,000 kw. of boiler plant which would have been required for the peak. I am not quite certain about that, but I think he is right, although it is rather a debateable point. In any case we should only reduce the £924 on the seventh line of page 391 by £460. With regard to the remainder of the year, the seven winter months, the author deals with the saving on the peak-load units. He gives a figure of 1,560,000 peak-load units, and claims a saving represented by the difference between what it costs to run peak-load plant during that short time and what it costs to charge the battery at the 0·25d. rate during the night. The result he obtains is about 1·3d. per unit, or a total of £8,534. But there seems to be a slip here, as the number of units given multiplied by 1·3d. only amounts to half £8,500. In any case the saving may be summarized as follows: In coal alone there is £(1,554–460) = £1,094, and £8,550, *i.e.* something over £9,000 saved. Capitalized at 5 per cent this saving of £9,000 per year means that (neglecting maintenance charges), £180,000 might be invested in batteries instead of £22,000, Mr. Taylor.

Mr. Taylor. without incurring any positive loss if the above coal figures are correct. Even taking the author's figures, it is obvious that the saving is not only sufficient to pay for the whole of the battery maintenance and capital charges, but it will also be sufficient to allow of a second battery being installed; and the saving in the coal bill due to the second battery might even justify the installation of a third battery. The author has thus made out from the coal bill alone a very good case for a battery irrespective of the saving on capital cost of the generating plant, the saving in labour (which for 22 men would represent £1,500 per annum), and the increased efficiency of generation all round.

The battery is also of inestimable value for emergency stand-by purposes. For example, in the case of a large supply system like that of Birmingham, with rotary converters and sub-stations, and where the continuous-current supply at those sub-stations is linked back to the main station through interconnecting feeders, if anything happens to the alternating-current supply then for the moment there is a tremendous rush of continuous current from the continuous-current plant in the main station to all the sub-stations. I have known generating-plant run on several hundred per cent overload under those conditions. A battery would save the situation in such cases. The battery we are proposing to use in Birmingham will be capable of giving 17,500 amperes for three minutes, and the value of that as a stand-by will be very great. The capital cost of that battery as a stand-by at a 3-minute rating is of the order of £2 10s. per kilowatt. No steam plant, or steam plant on overload, can show anything like that value. The floor-space occupied for that overload capacity is of the order of $\frac{1}{10}$th of a square yard per kilowatt. That again is immensely in favour of the battery as a stand-by. I think some opinions that have been recently expressed by various engineers, that the battery is of no use because of the large extension of alternating-current plant, are rather beside the point. A battery is exceedingly valuable in a station that combines alternating-current distribution with continuous-current supply. And not only that: it must be borne in mind that if there is all this saving with a battery it will pay to adopt rotary converters capable of transforming in the reverse direction; that is to say, use the converters to take a large discharge current from the battery and feed into the alternating current mains, providing in that way an alternating-current stand-by. It only adds a matter of £2 or £3 per kilowatt on to the £7 per kilowatt; and if all these other savings are being made surely that additional amount will very easily be balanced by the said savings. In Birmingham we have three double-ended boosters with a motor in the middle, and we are grouping them in series or in parallel on either side of the three-wire system as required, using a solid connection at the neutral point. Since that arrangement was adopted I have discovered how to obtain with practically the same switchgear double the amount of current from the same boosters, thereby largely reducing the kilowatt rating of the boosters as compared with the early methods of employing the boosters as single units. With regard to the

Mr. Taylor.

switches mentioned by Mr. Hutt, when we were obtaining tenders for the new plant at Birmingham we had an offer from Messrs. Siemens-Schukert, but they seemed very averse to dealing with the heavy currents we required while the switch was travelling over the contacts. Eventually we had to be content with a switch that would only carry about 6,000 amperes instead of the 17,000 amperes we required.

Communicated: I consider it by no means necessary to pay the ridiculous figures for sinking fund called for by the Local Government Board. The battery can be paid for out of surplus profits, thereby avoiding the sinking fund charges. Probably something can also be saved by not arranging a maintenance contract.

Mr. Jacob.

Mr. E. S. Jacob: There are one or two figures in the paper with which I do not agree, and which are unfair to the battery so far as capital charges are concerned. The author gives the capital charges for the battery based on a period of 10 years, and the capital charges for the steam plant based on 20 years. After the lapse of 10 years the capital charges on the battery will be nil, and the saving, instead of being about £1,000, will be £3,698, although probably the engineer to-day does not like to look forward to what is going to happen 10 years later. Expressed in another way, if the capital outlay of the battery were written off in 20 years, the saving per annum instead of being £1,000 would be something like £2,000; that I think would be a fairer basis of comparison. When steam plant is scrapped it has practically no value, or very little value, whereas a battery will always have as scrap metal a value of about 20 to 25 per cent of its original cost; that again improves the comparison. It should not be necessary, therefore, to write down the capital value of the battery to nil; it would be sufficient to write it down to 20 or 25 per cent of its original value. With regard to a point raised by Mr. Highfield, that battery makers ought to take into account the fact that certain people treat their batteries well and that they should thereby profit in the cost of maintenance, I may say that all users state that they treat their batteries well, so that the result of taking these statements into account would be that everybody would get an equally low rate. It is impossible to distinguish between the users, any more than it is possible for a life insurance company to distinguish between those people who are careful with their health and those who are not.

Mr. Fennell.

Mr. W. Fennell: In the first place I should like to put the point that a 500-kw. battery in a 1,500-kw. station is really as large as a 5,000-kw. battery in a 15,000-kw. station. Referring to loan periods for batteries, any good accumulator company will make a maintenance contract guaranteeing to leave the battery at its full capacity at the end of 10 years. On the other hand, the Local Government Board tell you that the loan is fixed at 7 years on the ground that there will be no battery left at the end of that period. There is something wrong with one practice or the other; I think the Local Government Board is wrong, and that the 7-year figure is out of date. I consider it of the greatest importance that influence should now be brought to bear

Mr. Fennell. on the Local Government Board to extend the period for battery loans. Many a battery scheme has been killed by the necessity of writing off the whole outlay in 7 years. I have had a fair experience with boosters. I have used two kinds of automatic boosters, also a hand-regulated charge and discharge booster combined with a battery switch for light loads, and finally the Continental type of automatic battery switch, motor-driven through worm gearing. It seems extraordinary to me that while on the Continent these large switches are almost universally used, and boosters are hardly employed at all, yet in this country we seem to confine our attention to boosters. The reason the Continental type of battery switch was installed at Wednesbury is that two years ago we had our station shut down due to the failure of the booster, that is to say, the principal thing on which we were relying for safety itself caused an absolute shut-down. There was no time to put in the short-circuiting switch, which has been referred to both in the paper and by Mr. Highfield. One of the generating sets failed suddenly, the reverse-current switch did not operate, and the overload on the battery caused the booster and its motor to burn out and to arc to earth. I think it is a pity to limit the overload capacity of the battery to the overload capacity of a machine. With regard to the security given by a battery, there is another point which interests engineers to-day, and which has not been referred to, namely, in connection with gas or Diesel plants. I myself am interested in gas plant, and I am not giving away any secret when I say that both the above types of plant are liable to sudden failure. It is extremely rapid; and without a battery there is not the same sense of security, to say the least of it, that there is with one. The usual practice, where reliability is of importance and there is no battery, is to keep an extra set floating on the busbars. This destroys the economy of working which is expected from these engines, so that the use of a battery is even more important with gas plant than with steam engines. Recently, owing to an unfortunate delay in the delivery of plant, we have been running at times of peak load with the whole of the gas engines at full load, and in addition with the battery discharging at the 2-hour rate. On one or two occasions a set has cut itself off the busbars during the peak load, but the consumers have not known it. The automatic battery switch has enabled the battery to take up the load as it came off the generator, and to carry it while the engine could be adjusted to take load again.

Mr. McMahon. Mr. P. V. McMahon: I should like to show a lantern slide in support of the author's contention that it is worth while installing batteries. The top curve on the slide (see Fig. A) shows the working of a battery at the "Angel" sub-station on the City and South London Railway in connection with the supply of traction, lighting, and lifts. The bottom line on this curve gives the average supply of the machines in the sub-station, and the battery load is shown by the peaks, so that in this case the battery takes about 50 per cent of the total output of the sub-station motor-generators. The bottom curve shows the results obtained from a slightly smaller battery in the London-bridge sub-station, in which

the motor-generator plant is of about the same size, and the over-load output of the battery is about 47 per cent of the total output of the sub-station motor-generators. Mr. McMahon.

Communicated: As there is practically no difference between

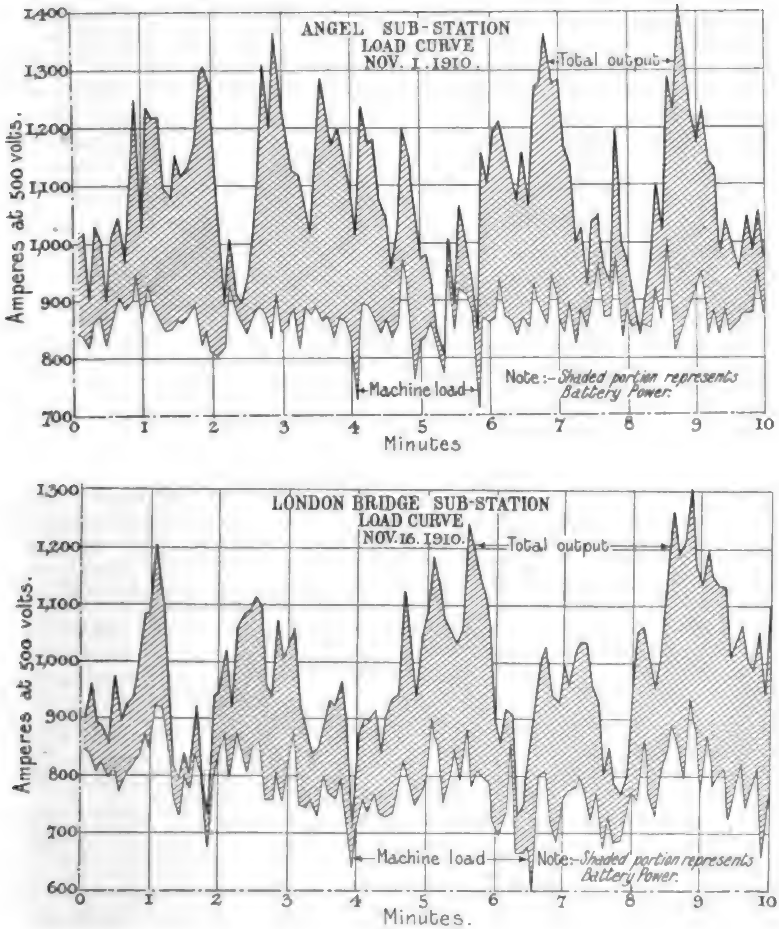

FIG. A.

the equipment of the two sub-stations to which Fig. A refers, it will be sufficient if the "Angel" sub-station only is dealt with. The equipment of these sub-stations was fully described in my paper* on the City and South London Railway in December, 1903, but it may be as well to state that the output of the reducing motor-generators

* *Journal of the Institution of Electrical Engineers*, vol. 33, p. 100, 1904.

Mr. McMahon.

is 600 kw. at normal rating, the battery having a capacity of 420 kw. on the 1-hour rating. The "Angel" sub-station is five miles from the power house. The peaks taken by the battery are 50 per cent of the motor-generator output, and the constantly recurring peaks amount to 33·3 per cent of the load on the machines. Taking the units delivered by the motor-generators and battery respectively during the test period, the battery deals with 25 per cent of the total machine load. In considering the value of the battery in reducing the capacity of the motor-generators, the constantly recurring peaks may fairly be regarded as the battery capacity. Thus without a battery the capacity of the motor-generators and cables would have to be increased by 33·3 per cent and the generating plant at the power house by an equal amount, plus the increased loss in the feeders. The battery boosters, switchgear, and motor-generators at the "Angel" sub-station cost £14,423, and the cables between the power house and the sub-station £10,300, making a total of £24,723. If the capacity of the motor-generators, cables, etc., as outlined above, is increased by 33·3 per cent, the sub-station for the same duty would cost £27,427, a difference of £2,704 in favour of the battery sub-station. The above prices relate to costs ruling about 13 years ago; but taking present-day prices somewhat on the lines of the author's figures, the sub-station, with battery, etc., would cost £17,260, and without a battery £21,727, a difference of £4,467 in favour of the battery sub-station.

The author only allows 10 years as the life of the battery, but the battery of the City and South London Railway Company has been kept up to its capacity by the makers under a maintenance contract, and it is now well into the second 10 years' maintenance period. At the end of this period, or when the battery is 20 years old, the company will still have a battery with its full capacity. Under those conditions it appears that the life of the battery should be taken equal to that of the steam plant, if not longer. In the following calculations 20 years is taken as the life of steam plant and battery, and allowing 7½ per cent for interest and sinking fund the battery sub-station shows a saving of £200 per annum, from which must be deducted the annual payment on account of maintenance, leaving a net balance of £35. In a similar manner, with modern prices for plant, the battery sub-station shows a saving of £322 per annum, and allowing for battery maintenance the net annual saving is £157. This does not really show the battery to the best advantage, as during the test the battery was only loaded to about 75 per cent of its full capacity. So far nothing has been allowed for the battery as a stand-by; and this value, although great, is difficult to estimate. With these batteries we are able to shut down all our generating plant at the close of traffic, all lighting, motors, etc., being supplied during the night from the batteries. In the case of a temporary disablement of a motor-generator the battery carries the supply over the difficulty, and on a recent occasion when the failure of a motor-generator armature affected all four machines in a sub-station, necessitating their being shut down, the battery took up

the whole load of the sub-station for three-quarters of an hour without the service being in any way affected. Mr. McMahon.

Mr. ROGER T. SMITH: The author in his paper shows the effect of a battery on a lighting load when the battery has a steady charge for a period which is reckoned in hours, and a steady discharge also for a period which is reckoned in hours. It may be interesting to give a few figures for the case of two batteries working in two sub-stations, the combined capacity of which only amounts to about half the output of the Manchester battery. These batteries work in parallel on a very infrequent traction load, where in general there is a rapid charge during a period which is reckoned in minutes, and a very rapid discharge during a period which also is reckoned in minutes. About once a week there is a long period of charge and a long period of discharge. In the accompanying table some figures are given, corresponding to the Manchester figures at the foot of Figs. 5, 6, and 8, for the Great Western Railway generating station at Park Royal for 16th January, 1913. This station supplies through sub-stations current for traction power and lighting. Figures both with and without batteries are compared in the table (see p. 408). The supply is three-phase high-tension to two sub-stations, each with a battery and automatic Highfield booster in parallel with the low-tension, continuous-current side of the auto-converters, and the figures given represent maximum load conditions just before six o'clock in the evening. Mr. Smith.

The figures have been built up from the values of the maximum loads observed on individual items of plant. Without the batteries the actual resultant load on the generating station cannot of course be checked by measurement, because the station never runs without batteries. When the station is working under normal conditions with batteries the calculated results agree with the actual results within 2 per cent, so that I think the calculations may be accepted. The results for 16th January show that the maximum load on the generating station with batteries was 2,720 kw., and without batteries 3,870 kw. The station load with the batteries was actually taken by 3 steam sets. Without batteries 5 sets would have been required. The daily load factor on the steam plant with the batteries, corresponding to the figures given by the author, was 58 per cent, and the daily load factor without batteries was 41 per cent, showing an improvement in load factor of 17 per cent. These figures correspond to those given by the author, viz. 13·1 per cent for the summer load and 14·7 per cent for the winter load. Of course the percentage gain in the load factor due to a battery depends on the percentage of the total output which goes through the batteries. That figure has not been given by the author, and I have not calculated it from his diagrams. I suggest, however, that the most economical proportion of the total output to pass through the batteries is between 15 and 20 per cent; and I should like to ask the author if he has any figures of that sort for his battery at Manchester. In Manchester the traction load is small compared with the lighting load. In the above table that I have given for the Great Western

Mr. Smith. Railway plant the lighting load slowly increases from about 200 kw. to a maximum of 550 kw., whilst the traction load varies rapidly from 300 kw. to 3,000 kw. The principal advantages of batteries in this

Particulars of maximum loads on the steam plant in the Park Royal generating station of the Great Western Railway for a typical day (16th January, 1913), both with and without batteries.

$$\text{Load factor} = \frac{\text{kilowatt-hours generated} \times 100}{\text{maximum load in kilowatts} \times 24}.$$

	With Batteries.	Without Batteries.
	Kw.	Kw.
Maximum traction load on 2 sub-stations together	2,891	2,891
Maximum traction load taken by 2 batteries ...	1,120	—
Maximum traction load taken by motor-converters	1,771	2,891
Boosters	60	—
Maximum load on sub-station motor-converters	1,831	2,891
Motor-converter losses at 10 per cent	203	321
Maximum load at high-tension sub-stations for traction	2,034	3,212
High-tension transmission losses 1·5 per cent ...	31	49
Maximum high-tension load on generating station for traction	2,065	3,261
Maximum high-tension load on generating station for lighting	530	530
Maximum high-tension load on generating station for station purposes	80	80
Total maximum high-tension load on generating station	2,675	3,871
Actual observed maximum load	2,720	—
Number of generators required for 16th January, 1913	3	5
Kilowatt-hours generated	37,244	
Maximum load on steam plant with batteries ...	2,675 kw.	
Maximum load on steam plant without batteries	3,871 „	
Load factor on steam plant with batteries ...	58 per cent	
Load factor on steam plant without batteries ...	41 „	
Improvement in load factor	17 per cent	

particular case, where the traction load predominates, appear to be, first, that batteries permit of a monthly machine load factor of 85 per cent—by machine load factor I mean the ratio of the actual output

Mr. Smith. to the output at rated load for the time during which the machines are all actually at work, and at periods of heaviest load, that is between 5 and 7 o'clock in the evening, we have obtained for the last few days a machine load factor of 95 per cent, and that with a traction load varying from 300 to 3,000 kw. in a very few moments. In the second place, batteries allow the traction load and the lighting load to be in parallel with each other on the high-tension busbars, and although the traction load varies 1,000 per cent in a very short time there is no difficulty, thanks to the batteries and boosters, in keeping within a plus or minus 4 per cent variation in voltage on the lighting busbars. For the last 6 years we have been doing exactly what Mr. Taylor proposed to do; that is, the battery is looked upon as a stand-by for the high-tension alternating side. In case of failure of the high-tension supply the auto-converter automatically reverses, takes energy from the battery, and generates high-tension three-phase alternating current. The third advantage is that without batteries 66 per cent more steam plant would be needed to take the peak loads, and if the whole steam plant was run at the average load necessary without the batteries it would result in an increased steam consumption of 13½ per cent. The advantages of a battery on a traction load are not quite the same as for a lighting load, but both are equally important.

Mr. Sparks. Mr. C. P. Sparks: So far as a general supply is concerned the position of the battery should be generally in the sub-station rather than in the generating station. In the case of Manchester this large battery has been placed in one of the power stations for special reasons. I do not wish to criticize those reasons, but I think we may be misled if we take it generally that a battery should be installed in the generating station because of the excellent results that have been obtained at Manchester. With regard to the future, this battery is giving economical results to-day, but the demand in Manchester is growing, and will continue to grow at a rapid rate. Ten years hence a 3,000-kw. battery will be a small affair compared with a 3,000-kw. battery to-day, and I very much doubt whether it will be useful or economical when we look to the future: that is always the great difficulty with regard to battery plant erected in generating stations. A battery is put in to-day of adequate size, but in a short time it becomes too small and it is difficult to obtain any economy by using it. The last point I wish to refer to is the interesting curve shown by Mr. McMahon. If he had told us more about it I think members would have appreciated the value of the battery in one of the City and South London Railway Company's sub-stations. I have to supply energy to that sub-station, and I know that, with peak loads on the sub-station rising to a maximum of 900–1,000 kw., the average supply taken is about 400 kw., the demand during the working hours not varying more than 40 kw. The load is so steady that it is difficult to believe that an irregular train service is being supplied. The battery acts as a complete buffer, and the load factor on the generating plant is of the order of 90 to 95 per cent during the day shift.

Mr. Hutt. Mr. A. Hutt: I should like to say something in favour of the cell-

Mr. Hutt. regulating switch to which the author has alluded very slightly. He has mentioned its advantages, namely its reliability, its freedom from breakdown, and its very large overload capacity, while his only criticism seems to be that for large currents these switches are rather cumbersome. I do not think this criticism is quite justified. These switches have practically never been manufactured or used in this country in large sizes, but they have been used very considerably on the Continent. For instance, one firm, Messrs. Siemens-Schukert, have made as many as 77 switches for 1,000 amperes and above during four years, so that quite a specialized type of switchgear has resulted. I propose to show a couple of slides to illustrate the construction of these switches. The first slide shows a double-arm switch for 4,500 amperes installed for the Hamburg municipality, whilst the second one shows a switch of slightly smaller capacity arranged for vertical mounting. Both switches are motor-operated and can be controlled from a distance, but a hand-wheel is provided for use in emergencies. It should be noted that by means of an auxiliary switch or "spark diverter," the main travelling brushes only carry current when resting on a contact, and are thus protected from sparking and abrasion. For this reason the contact surfaces wear very well and can be run at a high current density (about 500 amperes per square inch). This tends to reduce the size of the switch; nevertheless the brushes can carry about twice the normal current for quite long periods up to an hour, and four times the normal current for one or two minutes in cases of emergency. Another point that might be mentioned is a very common arrangement whereby the number of leads from the regulating cells to the switch can be reduced by about one-half. The cells are connected up in groups twice as large as those required for actual regulation purposes, and a small auxiliary group of half the size is automatically cut in and out by means of the spark diverter, so that the required number of steps is obtained. One of the chief features of large batteries such as that described by the author appears to be their great reliability in case of breakdown of other plant, when their large overload capacity can be made use of to the fullest extent. In this case the cell switch, in which very little can get out of order and which can be relied upon when machines fail, has a wide sphere of usefulness.

Mr. Yerbury. Mr. H. E. Yerbury (*communicated*): Mr. Pearce is to be congratulated on his boldness in installing the largest battery in England and on the satisfactory results obtained. One wonders why the Americans are so ahead of us in large battery work; possibly it is due to the majority of our supply systems being alternating-current ones. It appears to me that the space occupied by such a large battery and control gear seriously militates against its use in many undertakings. Its service as a load leveller and stand-by is undoubtedly admirable, as clearly shown by the figures and charts. I am interested to hear that this battery is now confined to the lighting load, although its adoption and use were originally intended for traction purposes. Ignoring for the moment the importance and

Mr. Yerbu

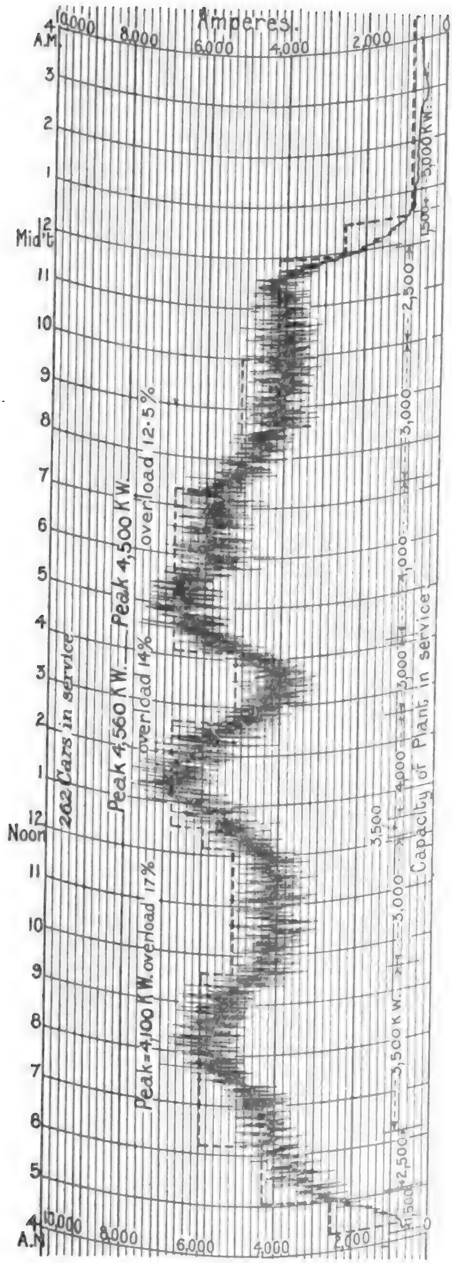

FIG. B.

Mr. Yerbury. advantages of a stand-by and reserve for lighting, especially during a storm load, I think that modern generating plant can well cope with any extraordinary traction load, and that, speaking generally, it would not pay to install such a battery for tramway supply only, as the boiler stand-by losses are not so great as they would be for a combined supply undertaking. The load chart (Fig. B) shows the abnormal fluctuations of load when about 100 extra cars are in service on one route for football traffic; it will be observed, however, that the peak is within the 2-hour rating shown in the hatched portion of the author's Fig. 3, and can readily be dealt with without heavily overloading the generating plant. Assuming the maximum demand of the tramways was coincident with the peak load for lighting, then the batteries would undoubtedly prove very valuable and economical for actual service, apart from the reserve or stand-by security. I should like to ask the author if the short loan period of 7 years is a standard period for battery loans, or whether it may be looked upon as a "test" period. As the makers of the battery agree to maintain it for 15 years for the sum of £1,250 per annum, will the author kindly state the estimated life of the positive and negative plates, and whether the battery will be entirely renewed by the makers within that period of maintenance.

Mr. Lackie. Mr. W. W. Lackie (*communicated*): It is generally agreed that if batteries could be obtained with a reasonable efficiency, and at a price comparable with that of steam plant, the supply of energy for electric lighting would receive a tremendous impetus. This does not apply of course to electrical energy for power purposes where the energy is generated and used fairly equally over the whole year and for more than 10 hours per day. I have had some difficulty in following all the author's conclusions, and further explanation appears to me to be required with regard to some of the diagrams. On page 383 it is stated that the chief duty of the Dickinson-street battery is to carry 3,000 kw. of the lighting peak, and in the very next line that the chief duty of the battery is load levelling. With the former statement I absolutely disagree, whereas with the latter I quite agree. Again, on page 382, a load curve is given of the lighting demand without the battery over 24 hours with steam plant. This gives a load factor of 32 per cent. On page 383 a corresponding load curve is given with a battery, and this shows a load factor of 43·5 per cent, but only over 16½ hours. I assume that the battery is called upon to meet the load for the other 7½ hours. A load factor with the steam plant of 32 per cent over 24 hours is better than a load factor of 43½ per cent over 16½ hours; and perhaps this is a complete answer to any proposal to install a battery to reduce the amount of plant required in a station. The other diagrams on pages 383 and 384 are far from clear. What I most object to in advocates of storage batteries for meeting peak loads and sudden demands is that they assume a central-station engineer knows beforehand what form the load curve for any particular day is to take. If one looks at the load curve shown in Fig. 5, the battery appears to be discharged from

8 a.m. until 12.30 midday; it is then charged from 12.30 until 2 p.m. One cannot foretell on a given day that fog or darkness will not descend over a city area at noon when the battery is practically exhausted and useless to meet a peak load. It is no good to say that this is a rare occurrence. If such a fog occurs once in three years, it condemns the scheme. In Glasgow we have had a fog for three days on end and a peak load from 10 a.m. until 5 p.m., or, say, 7 hours. Last September we had a sudden descent of darkness at midday on the day of the opening of the Smoke Abatement Exhibition in Glasgow; this darkness lasted until the hour of natural darkness at 6 p.m. The load rose in one station from 7,000 to 16,000 kw., and in another from 3,000 to 6,000 kw., or from 10,000 to 22,000 kw. in all. Of what use would a 3,000-kw. battery be in those circumstances? Over 15 years ago, when the electricity supply system in Glasgow was much smaller than it is to-day, two battery stations of 400 kw. capacity were erected. The total demand at that time was 1,700 kw. The batteries were charged from 11 p.m. till 6 a.m., and they failed hopelessly to meet the demand of a moderately foggy day. **Mr. Lackie.**

On page 391 Mr. Whysall states that 22 fewer men are required at Dickinson-street station since the installation of the battery. There is surely some mistake here. What were these men engaged at before the battery was installed. On the same page it is claimed that by the use of the battery there is a saving of the difference between the coal consumed of 0·237d. and 1·55d. per unit, 1·55d. being taken as the coal cost at the 8 per cent load factor. Is not the reasoning defective? Is the saving in coal not the difference between the coal consumed at the 32 per cent load factor and the coal consumed at the 49 per cent load factor as indicated in Figs. 3 and 6. About 1½ million units have been discharged from this battery; but as a matter of fact one quarter of these units would be required to meet peak loads, and therefore it cannot seriously be claimed that the battery has been installed to meet peak loads. A battery is an excellent thing, but it should be used as an equalizer of load, although it may on a rare occasion come in as a stand-by. The size of the battery that is to be installed will depend upon what one is prepared to pay for the insurance it offers. To install a battery to meet the maximum-demand load, as is said to be the practice in America, simply means that the supply cannot be continued at the rates per unit now obtaining. I have said before that everything possible should be done to encourage the large consumers to install storage batteries and operate them themselves. This would relieve the operator at the generating station from giving such a battery any attention in the case of emergency, when the time spent operating a battery might be much better utilized in getting the plant back into commission. I would suggest that instead of any eloborate booster or regulating cell arrangement, the battery might be installed on a 500-volt circuit so as to maintain a pressure of something like 400 volts in the case of a breakdown. The coal consumption does not depend upon the yearly load factor, but upon the daily load factor, and

Mr. Lackie. Mr. Whysall has shown that a daily load factor of 32 per cent is obtained on a lighting load.

Mr. Corson. Mr. F. H. CORSON (*communicated*): The paper is a record of actual achievement, and for this reason is to my mind much more convincing than the more or less speculative cases which have been recently made out for the inclusion of a storage battery in central-station equipment. The actual cash economy of some £7,800 on fuel in spite of an increased output of some 3 million units is an argument of much greater cogency than are the calculations which may be made of economies to be obtained in hypothetical cases. There is, however, one point, doubtless easy of explanation, which is not clear to those unfamiliar with the arrangement of the Manchester stations, and upon which more information would seem desirable. From the fact that, in Fig. 8, the load on the steam plant is 7,100 kw. and that the battery is discharging on lighting simultaneously with a load of 5,000 kw. on the motor-converter, it would seem to follow that the latter is drawing its supply from a source outside the station, and is thus dependent upon the steam-driven machinery of other stations. If this assumption is correct, it is scarcely just to attribute the improvement of 22 per cent in the plant load factor to the agency of the battery, the load factor of the Dickinson-street station being improved apparently at the expense of that of the other stations. The same remark would also apply in less degree to the improvement of 15 per cent shown in Fig. 6. From Fig. 8 it would seem that the Dickinson-street plant load factor might easily benefit to a much greater extent by the use of the motor-converter than by that of the battery, and some assurance that this influence has not been operative in enabling the economies shown in the table on page 392 to be realized, would be of value.

Mr. McInnes. Mr. C. F. McINNES (*communicated*): I should like to refer to the small storage battery, as this is after all of the same importance to the small station as the large battery is to the large station. I believe if we realized to a greater extent the possibilities of storage batteries it would be no longer necessary for the author to refer to their use as a luxury. Perhaps my recent experience may be interesting. Some four years ago I had to carry out an extension to supply a neighbouring district, the demand being principally lighting and beyond the limits of economical continuous-current distribution. After considering the relative merits of alternating-current supply, a Diesel sub-station, and a storage battery sub-station, the last-mentioned was ultimately adopted. The extension could not be carried out very cheaply, as it involved the purchase of a site on a main road with buildings to correspond. The total cost of the scheme, including site, buildings, a 225-kw. storage battery (one-hour rating), boosters, switchgear, etc., amounted to £3,450 and involved interest and sinking fund charges of £235 per year. The working costs, *i.e.* labour, repairs, battery maintenance, rates and taxes, insurances, etc., amounted to £195, making with the capital charges a total of £430 per annum. The battery is charged at any convenient time during the day, and is dis-

charged on the peak of the load, the practice being to load to its full capacity the generating plant running, and to supply the remainder of the peak from the battery, instead of starting up an additional set and keeping an extra boiler ready for this service. The result of this method of working was a reduction in fuel from 6·4 to 5·16 lb. per unit, representing in money £470 per annum, or £40 more than the total annual cost of the sub-station; these figures are obtained without taking any credit for the capital charges on the extra feeder which would have become necessary but for the use of the sub-station. After 3 years' use the overall efficiency of the sub-station is 73 per cent, quite as high, I venture to suggest, as would be obtained from an alternating-current supply considering the nature of the load. Mr. McInnes.

Mr. R. RANKIN (*communicated*): On page 391 the reduction of staff at Dickinson-street station is given as 22. Mr. Whysall does not say what percentage of the total this is, but it must be considerable. I estimate that such a reduction in salaries pays for the sundries, oil, water, and buildings, which is surely a good argument in favour of the battery, and put in a way easily understood. I wish particularly to deal with the question of maintenance, and in this connection Manchester appears to be a regular paradise for a battery. There are few stations where a pictorial representation of the advantages of the battery is framed daily for the benefit of the staff. The author says the value of a battery depends more upon the way in which it is used than upon anything else. I agree—it depends almost absolutely on it; and I should say that the careful and discriminating methods of using and caring for the battery at Manchester are worthy of the imitation of any engineer who has a battery under his charge. Mr. Highfield spoke of probable improvements in batteries by makers. It is not unlikely that such may be made in the near future, but why not also in the ways of working them by station engineers? It should be recognized that the sooner engineers realize the importance of keeping their batteries in a healthy condition, the sooner will maintenance premiums be reduced. I wonder if the heavy overloads mentioned by Mr. Highfield were within the limits stipulated in the maintenance agreement! A battery need not be, as is mentioned on page 396, a prolific source of trouble, if it is properly used. I know that the temptation to treat a battery roughly and unfairly is very great because of its passive and uncomplaining nature. If a machine is ill-treated it lets one know about it speedily, and often in no uncertain fashion, but with a battery it is only after severe damage has been done that trouble becomes obvious, and then all the blame is usually laid on the battery itself, not on the method of working. I am afraid that in many instances batteries are still expected to supply all sorts of night and week-end loads, not only without the needed assistance of generating plant, but also without the help of an adequate charge to begin with. Mr. Rankin.

Mr. McKinnon has dealt with some points regarding maintenance, but, without going to the expense of the new apparatus he mentions, it would be possible for those in charge of battery installations to reduce

Mr. Rankin. the worry, improve the efficiency, and obtain more satisfaction, if it were realized that batteries, like machines, have their little idiosyncrasies and require a little humouring. Extra attention paid to the taking of the ordinary readings and the keeping of records will repay itself many times over. In this connection it is sometimes a little confusing, not to say amusing, to find chief engineers writing and complaining of individual cells in a battery, while the attendant sends in maintenance reports showing the cells to be in a most wonderful state of equality and good condition. This is the sort of thing that causes trouble; trouble, it may be said, that could be easily avoided. The author has brought out clearly in Fig. 8 the reliability and flexibility in operation of the battery plant. Unlike ordinary generating plant, a properly-cared-for battery is not liable to develop sudden and unsuspected faults leading to failure of supply at the time when it is most needed. Such a thing with a battery is a very remote possibility. The sense of security engendered by this is, as has already been mentioned in the discussion, very valuable. The loads mentioned by Mr. McMahon and Mr. Roger Smith are quite different from the loads illustrated in the paper, and in those cases the provision of an automatic reversible booster is the correct thing, in fact an absolute necessity if anything like the best use is to be made of the battery. It is peculiar that although makers of cell-regulating switches must know this, they never mention it. One hesitates to think of what would happen on Mr. McMahon's railway system should his boosters be replaced by automatic switches. The conditions obtaining in the cases of railway loads and lighting loads are so different that it is difficult to compare them. As I pointed out in a paper read last session,* a coal saving of practically 30 per cent can sometimes be made on a highly fluctuating load by the use of a battery and automatic reversible booster. With regard to efficiency, the figures given for Manchester are certainly high. This is doubtless due partly to zealous attention to a new plant, and partly to the fact that efficiency is always higher in a large plant than in a small one. It is to be hoped that engineers will not all expect to get such high figures; they most likely will only meet with disappointment.

Discussion before the Manchester Local Section on 28th January, 1913.

Mr. Watson. Mr. S. J. Watson: It is worth considering where these large batteries should be placed. Should they be put down in the generating station, or should they be put down elsewhere in connection with the distribution system? In this connection we must not lose sight of the fact that multi-phase transmission schemes are being very largely adopted, so that the use of batteries in the main generating station would necessitate some sort of rotary converting plant. Where multiphase transmission schemes have been in use for some years the bulk

* *Journal of the Institution of Electrical Engineers*, vol. 48, p. 283, 1912.

of the distribution is carried out by means of continuous current from sub-stations, and a higher overall efficiency of the battery will be obtained, that is to say, the number of units supplied by the battery divided by the multi-phase units generated will be higher if the battery is placed in the sub-station than if it were placed at the generating station. The battery discussed to-night has been put into what might be termed a joint sub-station and continuous-current generating station, and it is used to level up the load on the generating plant and as a stand-by for emergencies. In considering the cost of a battery, there are several matters which have to be taken into consideration. The author has pointed out that steam plant has a capacity which will enable it to supply for 24 hours per day, whereas the battery has only a limited-time capacity. In this connection I think it is perhaps worth while considering how a battery should be rated. There are, of course, several ways—1-hour, 2-hour, 3-hour, and even 10-hour rating—but I think that the fairest is to adopt the 2-hour rating, because the primary use of a battery is to maintain the supply over the peak loads. If a 1-hour capacity only is taken, the battery will not last through the peaks, because the ordinary peak in the winter months lasts about 1¼ hours, *i.e.* between 4 p.m. and 5.30 p.m.; but if a 2-hour capacity is adopted, that will be sufficient for any peak that is likely to occur, and therefore in making a comparison between steam plant and a battery it is fairer to take this rating. The battery in Manchester has been installed at a cost of about £10·7 per kilowatt on this basis of a 2-hour rating. The author mentions Mr. Snell's figures, which are given as £12·26 per kilowatt for plant, but I think that a large power station could be built to-day at a cost of £10 per kilowatt. Therefore, on the score of cost, the generating plant and the battery are almost identical. With the battery there are no stand-by losses, but if the whole supply is to be given from generating plant a very large amount of coal has to be used in keeping boilers and steam plant ready for immediate service; it is in this direction that a battery enables a large saving to be made. Mr. Watson.

There are one or two observations in the paper which I cannot altogether make out. For instance, in dealing with the question of stand-by, the author speaks as though the boilers were not actually under steam, but might be got up from cold, should occasion arise. I do not want to misunderstand what is meant, but of course it makes a lot of difference whether the boilers are under steam. There are no stand-by losses if the boilers can be heated up on a dark day in time to deal with the peak. The question of using boosters or regulating cells is one which is always discussed in connection with battery papers, and there is a lot to be said on both sides. The use of a regulating switch is more simple, and of course it enables any sudden emergency to be dealt with which requires heavy discharges. As the author says, only one or two boosters may be running when the emergency arises, and it would then be impossible to carry the heavy load. In other ways also the use of regulating switches is better. For

Mr. Watson.

instance, suppose a very severe fault occurs on a machine, this machine will have to be instantly switched off the busbars, and if the battery is floating on the busbars without boosters in circuit there would be a serious drop in pressure due to the heavy discharge, since there is no time to switch the boosters in, whereas with regulating cells the pressure could easily be regulated. Where a large battery is provided, it should always be kept on the busbars, so that in a sudden emergency the full capacity may be relied on.

The design of switchgear for large batteries requires great consideration, and it is interesting to note that in Manchester the circuit-breakers have been placed in special cubicles, so that in the event of a circuit-breaker failing it is completely isolated in a fireproof receptacle. The question of cables is also of importance, since their size and cost are exceedingly high. It is very advisable to have the battery leads as short as can possibly be arranged. The author states that the amount of work done by the battery is gauged by means of three pilot cells. During a heavy discharge it will apparently be necessary to keep a man with a hydrometer at the cells in order to know what is happening. This seems to me a rather crude method of dealing with this very important matter. The usual practice is to provide a registering ampere-hour meter having a large dial equal to the range from zero to full discharge. On discharge the hands move forward, and backwards during charge; by automatically compensating for the loss when charging, the exact capacity of the battery can thus be seen at any time. This method would be much more convenient and definite than the information obtained by continually taking the specific gravities of pilot cells. It has been pointed out that the efficiency of batteries is somewhat higher when used with reversible boosters on tramway supply. There is no doubt that this is actually the case, and it is due to what is called the "gassing" effect, as the average pressure is raised owing to the rapid changes from charge to discharge; the efficiency is thus improved. I think most of those who started using batteries of fairly large size did so for traction purposes only, but we subsequently found that it is a much better proposition to use a battery on lighting; the general practice is now to provide change-over switches to allow of the battery being used either for traction or lighting.

As regards the size of the battery for a given scheme, I incline to the view that a battery at its 2-hour rating should have the capacity of the largest unit in the station. Batteries of very small size were used in the past, and as time went on they were not increased in size although the plant was increased, consequently the batteries became of little use. The efficiency of a battery really matters very little indeed, because the percentage of load which is supplied from a battery is only a percentage of the total, so that if a large percentage of that percentage is lost it does not affect the total to any very large extent, and is therefore almost negligible. On page 387, Mr. Whysall compares the capital cost of generating plant situated at Stuart-street station with

a battery which is being used at Dickinson-street station, and adds the cost of cables and sub-station to the generating plant. This may be fair in this particular case, but had the comparison been made between the cost of a battery and generating plant installed at the city station, it is clear that the cost of the cables and sub-station would not be included. The comparison is not really for the same thing. Finally, the author makes no mention of what I consider one of the greatest advantages of installing batteries, namely, the moral stability which it gives. The knowledge that this large capacity is behind one undoubtedly lifts a good deal of responsibility from one's shoulders. Mr. Watson.

Alderman W. WALKER: There is one point that I should like Mr. Whysall to make clear. I notice in the load curve, Fig. 6, and in other curves, that part of the input units were supplied from the sub-station, that is to say, the motors of the motor-generators were operated by the alternating current from Stuart-street station, whilst the continuous current from the generators was going into the battery; therefore such units were coming not from Dickinson-street but from Stuart-street. I should like to know whether an adjustment has been made in the author's calculations, and also what effect it has on the load factor given. There is another point; he has given the loan period as 20 years for the steam plant. Lately this term has been reduced, and 17 years now seems to be the loan period for a large turbo-generator set. Therefore, the difference between 20 and 17 years is to the further credit of the battery. The loan period for boilers is also being curtailed. I think that the question raised by Mr. Watson regarding stand-by boilers is answered by Mr. Whysall in his paper. The author states that he leaves the boilers out of commission, and as the load comes on he is able to use the battery whilst the boilers are got up absolutely from the cold; he has therefore a right to take credit for the saving. As regards the objection to the cost of the Stuart-street plant; for financial reasons this was the only comparison the department could take. I should like to ask Mr. Whysall whether when testing the pilot cells he simply takes the specific gravity at the top of the cell, or whether means are provided to obtain it at the bottom of the cell also. I have found that "gassing" is not sufficient to keep the density the same at the bottom as at the top of the cell. The author is quite justified in believing that consumers could take advantage of lower rates for charging their own batteries during restricted hours to cheapen their lighting supplies. I think that before very long there will be an opportunity of considering a particular case of this description not very far from here. Alderman Walker.

Mr. F. O. HARRISON: Taking into consideration that the makers give a maintenance period of from 10 to 15 years, the Local Government Board might reasonably increase the time allowed for the repayment of battery loans to the length of the maintenance period. If this had been done in the case of Manchester the figures given would have been even more favourable, and would give a much fairer basis of comparison with the steam plant. Mr. Harrison.

Mr. Wheelwright.

Mr. P. P. Wheelwright: The only point with which I cannot agree is the author's figure for the cost of generating plant, which is very much too high. In view of the prices of modern turbo units, the capital cost given in the paper should be taken at a much lower figure, in fact I think if reduced by 30 per cent it would be more accurate; this reduction would make considerable difference to the financial side of the case. Going over the advantages I think there is one point which might have been mentioned, namely, that in "rushing up" boiler plant a considerable amount of smoke is bound to be emitted; this is very liable to cause trouble, especially in stations like that at Dickinson-street situated in the centre of a city, whereas with batteries the load can be temporarily dealt with until the boilers are fully under steam. The modern battery is built on sound and much improved lines, and after considerable experience of three old-type batteries I am pleased to say that the fourth is now doing good work. With previous batteries the trouble was that they had always to be nursed until the time they were required, and then the battery would invariably fall short of the demand put upon it. When I installed my present battery the makers told me "to work it as hard as possible and not to nurse it, and the battery would work well." I have done this, and for the past four years the maintenance of the battery has been low, and the battery has carried out satisfactorily all the hardest loads put upon it.

Mr. S. L. Pearce: I want to deal with the question of the saving obtained from the battery. There has undoubtedly been a saving in the amount of coal consumed. The figure is just over £3,348. There is, in addition, a saving of £1,056 on capital account as between the steam scheme and the battery scheme. This gives a total saving of £4,404. It might be suggested that from this figure the capital cost incidental to the battery itself ought to have been deducted; if that is done there is still a saving of £2,000. That is bringing it down to the irreducible minimum. If that had been the only saving, however, it would still have justified the scheme in my opinion. Reference has been made to the fact that the losses incurred in the battery and the booster are more than covered by the coal saving. These figures I gave in the discussion in London on Mr. Taylor's paper about two years ago. The total units lost under the above headings are approximately 700,000, and the coal saving covers these losses more than four times over. There has also been some discussion on the capital estimates, and I quite agree that with present-day knowledge the figures for Stuart-street steam plant are possibly on the high side. These estimates were drawn up at the end of 1907 or the beginning of 1908, but in comparison with the figures given in Mr. Snell's book published last year, they are, I think, fairly reasonable. Taking the battery on a 2-hour rating, and reducing the estimates on the steam plant, one may say that there is practically no saving on capital, one scheme costing the same as the other.

I think that the design of the switchgear gave rise to more anxiety

than anything else in connection with the battery. I remember paying a visit to some of the larger battery stations on the Continent; whilst I was impressed by the lay-out of the battery stations I did not see much to admire in the design of the switchgear, and I came to the conclusion that very special treatment was necessary. I discussed the question with Mr. Field, and it was decided that we should treat this battery switchgear as if it were a high-tension board; we therefore adopted, as the author has pointed out in his paper, the cubicle construction, and the isolation of all leads where possible. Then came the question of circuit-breakers controlling the battery. One point I should like to mention, *i.e.* there should always be a separate means of isolating the battery other than by the circuit-breakers placed at the back of the battery board. I think a second set of circuit-breakers should be placed as near the battery terminals as possible. We have a system of circuit-breakers at Manchester which are fitted in the battery room alongside the battery, with remote control from two points in the engine room. A great deal may be said about the loan periods. The Local Government Board seem to be drawing the strings tighter and tighter. We have only had seven years for this storage battery, and as Alderman Walker pointed out, they are not only cutting down the period for batteries, but also for generating plant. To-day we only get 15 years for generating plant, and 15 years for sub-station plant, and the term for boilers has also been reduced from 17 to 15 years. There is one error in the paper in the treatment of the sinking fund. In connection with the battery scheme interest on capital is taken at 3½ per cent and sinking fund at 8¾ per cent on an equated basis for the several loans incidental to the complete scheme. This of course is not quite correct, but I quite agree that the author could not have expressed it in any other way. It simply means that the saving in the first seven years is less; and after the expiration of that loan period for the battery the saving is proportionately greater. However much attention may be given to the design of a battery scheme of this magnitude, the success of the scheme will after all very largely depend on those who have to operate the battery. In this connection the records which have been devised and kept by Mr. Whysall and his staff have been of inestimable value, and the success of this battery has undoubtedly been due in a very great measure to the care bestowed upon it by the author. Mr. Pearce.

Mr. C. L. E. STEWART: Mr. Wheelwright has said that he got very much better results when making considerable use of his battery. I have had exactly the same experience. We have a small battery which has a very busy time and which is doing remarkably well under it. It is used on traction load, and the efficiency is very good. It is charging and discharging up to its full 1-hour rating every two or three minutes, and it is in very good condition after being in use for four years. We find that the easiest way of judging overdischarge is for the switchboard attendant to watch the recording voltmeter chart. I am referring more particularly to when the battery is on traction. At first sight the care taken of his records by Mr. Whysall looks very elaborate, Mr. Stewart.

Mr. Stewart. but I think the elaboration is more apparent than real. When we bear in mind the amount of money spent on the battery, I do not think that the records kept are too elaborate; and they are well worth keeping. I do not like very long loan periods. If a profit is made it is taken in most cases to help the rates. If it can be managed, therefore, it is better to pay off the loans as early as possible so as to able to sell current at a lower rate than when the undertaking is burdened with capital charges. In view of the proportion that capital charges bear to the sale price of current, one wants to be finished with loans as quickly as possible; and as far as circumstances will permit it is best to pay out of profits, for plant on which the Local Government Board only allow a very short repayment period. To me it looks ridiculous to contribute, say, £5,000 to the rates and simultaneously to apply for sanction to a loan of £20,000.

Mr. Koppel. Mr. A. A. KOPPEL: Before making up our minds that the battery is the correct thing for peak loads and stand-by, I think the oil engine should be taken into consideration and the efficiency of both compared. I have looked through some figures Mr. Sowter gave in the *Journal* last year,* and I find that for a 400-kw. size the oil engine costs £13·3 per kilowatt, which compares favourably with steam plant, leaving larger units out of consideration. Again, in a paper read before the Northampton Institute Engineering Society on 3rd November, 1911, by Messrs. Houston and Parnel, figures are given for a load factor of 14 per cent. The Diesel plant gave 0·53d. per unit. According to the load curve in Mr. Whysall's paper, page 385, 14 per cent load factor shows 0·8d. per unit with a battery.

Mr. Cramp. Mr. W. CRAMP: At the top of page 393 it is mentioned that the makers have signed a contract to maintain the battery at its rated capacity for 15 years. In dealing with batteries I have found that makers have objected to maintaining a battery at its rated capacity throughout the maintenance period, but that they will guarantee 85 to 90 per cent of its rated capacity at the end of the maintenance period. Mr. Whysall's statement points to the fact that the Manchester battery is either capable of an output which is larger than its normal rating so that it can be maintained at that rating, or that this make of battery has an advantage over other makes. In regard to the saving obtained, the load-factor improvement was estimated to save £10,166, whereas the estimated saving from stand-by value and in cost of generation is given as £5,821. Is the difference between these two figures due to the "buffering" effect alone? If so, this is a far more important saving than is usually considered possible from this cause, and central station engineers would do well to note its magnitude.

Mr. Thomas. Mr. E. THOMAS: I have just completed the renewal of a battery which has been in regular service for 15 years in an electric light station. There are probably few generating station batteries with so long a life, and during the whole of it the battery has been maintained at a substantial percentage of its initial capacity, and has taken all the

* *Journal of the Institution of Electrical Engineers*, vol. 49, p. 611, 1912.

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night load and a considerable part of the day lighting load. It has also taken the peak load in the heavy season and has acted as a load equalizer all through. Altogether it has saved its cost probably several times over. It would even now be in satisfactory use but for serious neglect during the last few months of 1912. Its capacity is now very low, and the utility of the battery has been impressed on us by the great increase in the station costs during the last few months. I had particularly hoped for some estimates, based on experience, of the working expenses with batteries of different sizes, as some guide to the best size to install. The use of a battery in small generating stations and for country house lighting has been long acknowledged. It arouses rather cynical reflections on the conservatism of electrical engineers that it should be necessary in 1913 to bring forward this paper to show that batteries also give economies in large generating stations. The use in the large station with much day load is obviously different. In Manchester the battery might be made large enough to enable the steam plant to be shut down for, say, 7 or 8 hours and to work two shifts only. But it is doubtful if there would be any steam economy, although the load would then be almost dead level during the whole of the running time, and a larger proportion of the peak would be taken. If three shifts have to be worked, then there is actual loss in discharging the battery for the night load, and the battery is normally only of use to diminish the total number of boilers and engines in commission by taking the peak, and to get the full value out of engines and men working by equalizing the load. I am convinced that the main advantage is in the reduction of plant installed and in commission. Its further uses in abnormal conditions as a stand-by are sufficiently obvious. I do not accept with confidence the figures given either for the actual or the estimated saving. The records show remarkable savings from year to year without the battery. How, therefore, can we feel sure that the difference between 1909 and 1910 was all due to it? Indeed, it is acknowledged that some corrections were made. Further, with the great interest aroused in the power station by the use of battery and the attention that would be paid to everything affecting the costs, there would be improvements all round, which were not directly due to the battery itself. Hence it is not surprising to find that Mr. Pearce interprets the actual figures differently from Mr. Whysall, and considers the saving due to the battery less than that shown. Again in the estimated saving, to calculate merely by difference in load factor, using curves (Fig. 7) which had been collected in earlier years under quite different conditions, is surely not safe. Since, however, it is necessary to estimate, it would give me more confidence if Mr. Whysall, with his intimate experience of the detail working of the station, both with and without battery, would take, say, a dozen representative load curves, one for each month, and lay out his programme for working the year through as to boilers, engines, men, and hours of running of each. Past records and tests would enable him to calculate the coal required, also the wages, and make all other

Mr. Thomas. allowances. This having been done for the station without the battery, and then for the same loads with batteries of several different sizes, would give results of immense interest and use to other engineers who want to plan battery extensions for themselves. Mr. Whysall's short-cut method of estimating from load curves alone is surely not safe, and I cannot believe that the station cost of the peak load is 1·55d. The decision as to the size of battery to be installed seems to have been quite arbitrary. There is no magic in 8 per cent or in the peak load on a 2-hour basis. The decision was probably based, as in other cases, on a keen and practical judgment, or on the size of the space available for the battery, or on the expenditure to which money or courage would run. As a guide for future work it would be most useful to see the effect of other sizes. The Americans have adopted batteries relatively and actually much larger. How do these different practices affect working costs?

Mr. Whysall. Mr. F. H. WHYSALL (*in reply*): Mr. Highfield has referred to a battery discharged over the peak, and then of no use as a stand-by. Our battery at Manchester is used chiefly for a peak-load supply, but we also regard it in some measure as an emergency battery. Against Mr. Highfield's objection I might point out that the time when the battery is generally fully discharged is when the load is falling very rapidly. Mr. Highfield favours the hand booster, and says that a short-circuiting switch ought to be provided to cut out the booster in the case of emergency. We have such a switch at Manchester, and I think it is very necessary.

Mr. McKinnon drew attention to the amazing saving given in the paper for the first year when the battery was used. I sympathize with him to a certain extent, and confess that I should not like to be obliged to reproduce those figures year after year. It is quite natural to expect better results when a battery is new and everybody is striving to obtain for it ideal conditions. Aluminium is only used for the switchboard; not in the battery room. There were special reasons in our case why the question of weight had a different value. I favour stand-by batteries only when the supply authorities take financial responsibility for damages due to failure of supply. A peak-load battery is not a duplication of plant, but a stand-by battery admittedly is, and saves nothing in running expenses. Mr. McKinnon leaves me unconvinced regarding the size of the battery in New York. It has fewer cells and the plates are smaller than those of the Manchester battery; moreover, it is purely an emergency battery of the pasted type. His suggestion that maintenance charges should be based on the number of charges and discharges in a given time is an American innovation and unsuited to our requirements.

Mr. Taylor criticized some of the estimated savings. On page 393 of the paper I give a summary of several methods, and the estimate that he found fault with was merely one of two that I use as a sort of check against the actual results, with the idea of enabling engineers to estimate for themselves the possibilities of any proposed scheme. I

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have corrected the figures in the paper, and I have to thank him for drawing attention to the mistake. Mr. Taylor has referred to the saving effected by the battery apart from its own stand-by capacity. That is really a very important point, and I would again call attention to the fact that the value of the battery in the case illustrated by the storm-load diagram was not in any way proportional to the battery's capacity. The value of the battery was due to the elasticity that it gave to the system, enabling us to get more boilers away than we had at the commencement of the darkness. The chart shows that the battery is by no means efficient at the time of maximum demand. The hatched portion on the left-hand side of the diagram shows the duty the battery was doing on the rising load, holding the load until the steam plant could be got away. I calculated that on that particular occasion we made the battery equivalent to 44·5 per cent of stand-by boilers. Mr. Taylor also referred to the advantages that we derived from having our battery arranged for use on both traction and lighting, and he mentioned the consequent advantage of our booster arrangements. I am sorry to say that we do not operate in that way, and we have now entirely disconnected our battery from the traction system.

Mr. Jacob called attention to the term of years. That given in the comparison of capital costs is an equated period of 10 years. It will be seen on page 387 the loan periods are given for the various portions of the equipment—7 years, 15 years, and 20 years. In calculating the saving I took an equated period of 10 years, and of course what Mr. Jacob said is perfectly correct. The calculation is distinctly to the disadvantage of the battery. We have a maintenance contract for 15 years, and the life of the battery is really much longer than the term taken. With regard to the Local Government Board period of 7 years, another speaker said that this was too short, and that in effect we should have nothing to pay after that period; that we should have a battery with no interest and sinking fund charges on it. The point that Mr. Jacob made about the scrap value of the battery was very interesting; it had not occurred to me.

Mr. Fennell spoke about a 500-kw. battery. The principle applies to any battery no matter what size, and I tried to confine the range of operation to 8 per cent load factor in the paper. With regard to boosters and regulating switches, several speakers have made comparisons between the practice in this country and the practice on the Continent. To me it is only a question of what duty one expects to get from the battery. If the latter is to be an emergency battery, I think a regulating switch ought to be fitted; but for a peak-load battery a booster combination, with short-circuiting switches for emergency conditions, is the correct thing.

Mr. Sparks mentioned that a battery became too small for a particular scheme after a few years. The proper thing to do, of course, is to extend the battery and keep its capacity a constant ratio to that of the plant installed.

In reply to Mr. Roger Smith, we try with our battery to get 100 per

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cent plant load factor; that is to say, the whole time a machine is on load we endeavour to have it working at 100 per cent load factor. The number of units discharged by the Manchester battery is 5 per cent of the total number of units generated, calculated on one year's working.

In reply to Mr. H. E. Yerbury's communicated remarks, the estimated life of plates is for positive plates 4 to 6 years, and for negative plates 6 to 8 years. It is expected that the plates of the Manchester battery will be completely renewed twice, and perhaps three times, during the 15 years' maintenance. I would refer Mr. Yerbury to Mr. A. M. Taylor's remarks regarding battery floor space. The loan period of 7 years is not necessarily standard, and may be altered by the Local Government Board if sufficient reason be found.

Several speakers have referred to the portions of the curves representing the duty of motor converters; perhaps it will be well to say that there are three 1,800-kw. sets and two 500-kw. sets in the Dickinson-street station. Previous to the installation there were two more 500-kw. sets. This sub-station forms the connecting link between the city stations and the high-tension station at Stuart-street. It is necessary also to state that when Stuart-street station acts as stand-by to the city stations the stand-by fuel cost at Stuart-street is charged to and included in the works cost of the city stations. Reference should also be made to the explanation of the load-factor diagram in the paper which deals with that point.

Mr. Lackie confuses load levelling and buffering. The chief duty of the battery at Manchester is to take 3,000 kw. off the lighting peak. In the corrected estimates of possible saving, the saving due to buffering, as distinct from peak-load duty, is clearly defined. This is also considered below in my reply to Mr. Cramp. Mr. Lackie is in error regarding the load curve Fig. 4. The base of 16½ hours is the period the steam plant would be in use, but the load factor is worked out on 24 hours and is 43·5 per cent. The central station engineer does know very nearly what shape the load curve will be, and I think the storm load curve Fig. 8 is a complete answer to Mr. Lackie's criticism regarding fog or sudden darkness. The battery was certainly not sufficient in itself that day to supply the demand, but it held the load long enough to enable the staff to get up the extra boilers necessary. With regard to the 22 fewer men employed, there is no mistake; the particulars are as follows:—

	Before the installation of the Battery.	After Battery was installed.
Tradesmen and labourers... ...	44	29
Station hands	81	74

Reduction $15 + 7 = 22$.

Mr. Lackie deals with the estimated saving on peak-load units and thinks the reasoning defective. His values for load factor are incorrect, as they are daily load factors, and the curve from which the figures are

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taken is worked out on monthly observations. I can only repeat that I pin my faith to the actual saving of £7,800 shown, and only give the methods of estimating the possible saving in order that others may find out the possibilities in their own circumstances. Mr. Lackie states that 25 per cent of the units discharged from the battery would be required to meet the peak loads. The actual units discharged by the battery for the year mentioned were 1,673,600, of which some 780,000, or 46½ per cent, were used to meet the peak loads.

In reply to Mr. Rankin, the reduction in the number of staff was 15 per cent.

The first question that Mr. Watson raises is whether the battery should be installed at the generating station or the sub-station. This depends entirely on the circumstances and the system. In the case of a large continuous-current system the advantage is in favour of installing a large battery at the generating station. In the case of a three-phase system with sub-stations, the best place is the sub-station. If the system is such that continuous current is distributed from sub-stations, the battery is installed at the sub-station. There is then not only a saving on the generating plant, but on the cables and converting plant as well. In a case of this kind it would be best to install cables and sub-station plant for the immediate requirements, and to carry out battery extensions. This seems to me the best method. Mr. Watson also raises the question of the rating, whether it should be one hour, two hour, or three hour. He considers a two-hour rating should obtain, and in the case of Manchester thinks the battery would not be large enough for the duty required if the peak lasted over one hour. I disagree with Mr. Watson, and if he will draw a parallelogram round the peak in the curve on a base of two hours he will find that the area enclosed by the curve is exactly half that of the parallelogram. I purposely gave details in the paper of capital cost so that anybody could add or take away anything which he thought necessary. I will not attempt to justify the figures any further than to say that I fitted them for the purpose of the paper to our actual conditions, and took the actual costs as far as I was able to ascertain them. With regard to stand-by boilers, the saving represents stand-by boilers in the sense that the boilers are not under steam. As to the difficulty anticipated by Mr. Watson in paralleling boosters under breakdown conditions, there is a large margin, and failing this, we have arrangements for use at our discretion whereby we can put the battery on load without boosters. It is quite possible for the station staff to anticipate trouble and to get the boosters out of circuit under many conceivable conditions. I think we can take it as a general rule that the cost of cables for a battery is not any greater than it would be for machines. Mr. Watson suggests the use of an integrating wattmeter instead of pilot cells. We have integrating meters to register the input and output of the battery, and also a recording voltmeter and recording ammeter, as recommended by another speaker. We may wish to know perhaps at some critical time how much longer the battery can be relied upon, and there may have been such a varying

Mr. Whysall. load on the battery that it is almost impossible to make the necessary mental calculation to determine this. The pilot cell reading provides the information at once, and the state of the battery can be calculated exactly from the specific gravity of the cell. The size Mr. Watson recommends for a battery happens to be the size of the Dickinson-street battery. He suggests that the two-hour rating should be the capacity of the largest machine, which of course only refers to the station where the battery is installed. As to his comparison of capital costs, and his suggestion that we might have made a worse showing with the plant at Dickinson-street, this is a question of land values. Land costs more in the city area than it does outside. I think in fairness I should under certain circumstances question the moral stability which a battery gives.

Alderman Walker asks if arrangements are made to find out the specific gravity at the bottom of the cell as well as the top. The specific gravity is taken in the usual way at the surface, and we do not endeavour to differentiate between that and the specific gravity at the bottom of the cell, which of course will be greater.

Mr. Koppel mentions Diesel engines, gas engines, and batteries. Comparisons have been made at various times between these different schemes for peak-load duty, and I was very surprised to find that gas engines could not possibly compete in the matter of generation costs.

Mr. Wheelwright says that the battery prevents the making of smoke. I must say, however, that the only time we have been within an ace of the police court due to smoke nuisance is since the installation of the battery: I can explain it in this way. Owing to reliance on the battery we did not have so many stand-by boilers, and one day through a sudden load coming on we tried to do the load with the boilers in commission, consequently making more smoke than usual. I believe that the oftener a battery is charged and discharged the better it is for the battery, provided the operations are carried out with due care.

Mr. Pearce criticizes the capital cost, and makes out the worst case he possibly can in order to show that even putting the problem at its lowest level we could expect £2,000 annual saving. Perhaps I am too optimistic in crediting all the saving shown to the battery. I must admit that the circumstances at Dickinson-street are peculiarly favourable to a battery. We make an immense saving from the cutting off of all condensation losses at Dickinson-street works when we are able to shut down the steam plant there, and the battery has made this possible for certain months of the year. The points Mr. Pearce makes regarding the switchgear are very important, particularly that in regard to the circuit-breakers and double control. Mr. Pearce also explains that loan periods for generating plant are being cut down by the Local Government Board, and therefore this influences the figures shown in favour of the battery.

In reply to Mr. Cramp, the difference between £10,166 and £5,821 does represent the saving possible due to buffering. In reply to his

other question, the makers do guarantee to maintain 100 per cent capacity. Mr. Whysall.

Mr. Thomas asks if 8 per cent load factor is the only figure that can be given when using a battery. In the paper the load-factor chart and the figures given in connection therewith are intended to supply all the information that Mr. Thomas asks for. Referring to the saving in 1909 over 1908, Mr. Thomas asks if there would have been any saving if the battery had not been installed. In the year 1909 we were undergoing a good deal of reorganization, and were allowing men to leave without replacing them, and experiments were being made to produce the battery conditions, and of course we got the benefit of these. Otherwise, if it had not been for anticipating the battery scheme there would not have been any difference between 1909 and 1908.

Conclusions to be drawn from the discussion seem to be: That a battery with a one-hour rating equal to the largest generating set will effect all possible saving due to buffering. That a purely emergency battery effects no saving in running expenses. And finally that a peak-load battery effects a considerable saving in capital charges and running expenses, and has in addition the advantages of a buffering battery and an emergency battery to a large degree.

ANNUAL DINNER, 1913.

The annual dinner of the Institution was held in the Grand Hall of the Hotel Cecil on Thursday, 6th February, 1913. The President, Mr. W. Duddell, F.R.S., presided at a gathering numbering about 380 persons. Among those present were: The Right Hon. H. L. Samuel, P.C., M.P. (Postmaster-General), Lord Justice H. B. Buckley, P.C. (Lord of Appeal), The Hon. Sir C. A. Parsons, K.C.B., F.R.S., Sir Andrew Noble, Bart., K.C.B., F.R.S., General Sir R. Harrison, G.C.B., Lt.-Colonel Sir Matthew Nathan, G.C.M.G. (Chairman of the Board of Inland Revenue), Sir John Wolfe Barry, K.C.B., F.R.S. (Governor of the Imperial College of Science and Technology), Sir H. F. Donaldson, K.C.B. (Superintendent of Woolwich Arsenal), Sir A. Geikie, K.C.B., D.C.L., F.R.S. (President of the Royal Society), Sir R. Hunter, K.C.B. (Solicitor to the General Post Office), Sir A. Whitelegge, K.C.B. (Chief Inspector of Factories, Home Office), Sir John Bradford, K.C.M.G., F.R.S. (Secretary to the Royal Society), Sir Frederick Bridge, C.V.O., Sir J. Mackenzie Davidson, M.B. (President of the Röntgen Society), Sir John Gavey, C.B. (Past President), Sir Alfred Kempe, D.C.L., F.R.S. (Treasurer to the Royal Society), Sir Henry Norman, M.P., Colonel R. E. Crompton, C.B. (Past President), Dr. R. T. Glazebrook, C.B., F.R.S. (President of the Faraday Society), Mr. W. F. Marwood, C.B. (Assistant Secretary to the Board of Trade, Railway Department), Mr. R. A. S. Redmayne, C.B. (Chief Inspector of Mines, Home Office), Major W. A. J. O'Meara, C.M.G. (Vice-President), M. Grosselin (President of the Société Internationale des Electriciens, and President of the (6th Section) Société des Ingénieurs Civils de France), Herr Geheimrat Christiani (President of the Verband Deutscher Elektrotechniker, and Past-President of the Elektrotechnischer Verein), Mr. A. B. Anderson (Member of Council), Mr. J. J. Bisgood (Mayor of Richmond), Mr. R. Blomfield (President of the Royal Institute of British Architects), Mr. T. P. Brogan (Mayor of Battersea), Dr. S. Z. de Ferranti (Past President), Professor P. F. Frankland, F.R.S. (President of the Chemical Society), Mr. F. Gill (Member of Council), Mr. J. S. Highfield (Member of Council), Mr. W. Judd (Vice-President), Mr. J. E. Kingsbury (Member of Council), Mr. P. V. McMahon (Member of Council), Mr. W. McWhirter (Chairman of the Scottish Local Section), Professor T. Mather, F.R.S., Mr. R. C. Heron Maxwell (Assistant Secretary to the Board of Trade, Companies Department), Dr. R. Messel, F.R.S. (President of the Society of Chemical Industry), Mr. W. M. Mordey (Past President), Mr. W. H. Patchell, Mr. S. L. Pearce (Member of Council), Professor J. Perry, D.Sc., F.R.S. (Past President), Mr. W. Rutherford (Member of Council),

Mr. A. H. Seabrook (Member of Council), Mr. Roger T. Smith (Member of Council), Mr. C. E. Spagnoletti (Past President), Mr. C. P. Sparks (Member of Council), Mr. J. Swinburne, F.R.S. (Past President), Mr. A. M. Taylor (Chairman of the Birmingham Local Section), Professor S. P. Thompson, D.Sc., F.R.S. (Past President), Mr. H. L. Thomson (Mayor of Westminster), Mr. C. H. Wordingham, and Mr. P. F. Rowell (Secretary).

The PRESIDENT gave the toasts of "His Majesty the King," and of "Her Majesty the Queen, Queen Alexandra, His Royal Highness the Prince of Wales, and the other members of the Royal Family."

The Right Hon. HERBERT SAMUEL, P.C., M.P. (H.M. Postmaster-General), in proposing the toast of "The Institution of Electrical Engineers," said: Perhaps it is not unfitting that the member of the Government who is for the time being the head of what is probably the largest electrical enterprise in this country should have been invited to propose this toast. The Post Office, which is charged with the duty of conveying by telegraph and by telephone the communications of the whole of the population, owes very much to electrical engineers, and it is grateful for the assistance which is rendered by this Institution to the development of electrical science. I should particularly like to express the obligations of my Department to the Institution for the loan of its commodious and stately building for the recent meetings of the International Conference on Wireless Telegraphy. I know that the distinguished men who came as delegates from all over the world were impressed by the magnificent quarters which the Institution has provided upon the Embankment. The electrical engineers of the United Kingdom have by their labours built up for this country a great, prosperous, and rapidly growing industry. I cannot refrain from drawing your attention to some figures which were given in answer to a question in the House of Commons only a few days ago, and which illustrate the remarkable growth of the British electrical industry. Until a few years ago—the date given was 1903—the British exports of electrical machinery of all kinds were less than one-half of those of the United States of America. Thus, the United States had a considerable lead, for while British exports of electrical machinery were valued at £437,000, theirs were valued at over £1,000,000. The latest figures—those for 1911—show that while the exports of the United States have increased from £1,000,000 to £1,700,000, British exports have risen from £437,000 to £1,791,000; so that the British exports have multiplied more than fourfold in eight years, and are now slightly greater than those of the United States of America. I draw no moral from that with respect to differences of fiscal policy; but it shows a remarkable result of the well-directed use of capital and labour, and above all of professional skill, that so large an advance should have been made in so short a time. And, after all, if capital is necessary for the development of an industry, and if skilled labour is essential for its prosperity and progress, the intellectual effort of the men who direct that industry is most important of all. If the capitalist represents the pocket,

and the workman represents the hand, electrical engineers represent the brain, which stands, of course, on a far more august level. My toast is the health of the Institution of Electrical Engineers, and I ask you to couple with it the health of the President, Mr. Duddell. When the Government recently had to form an expert committee to inquire into the difficult subject of wireless telegraphy, the task of forming that committee was difficult. To secure both competence, independence, and impartiality, was not easy. The first name, combining all three of those qualities, that occurred to the Government as a suitable member of that committee, was that of the President of this Institution, Mr. Duddell; and for the other technical members of the committee it was felt to be impossible to do better than select two ex-presidents of the Institution and one member of the Institution who is an ex-president of two of the sister institutions. Although Mr. Duddell has the distinction of being the inventor of the singing arc, which is the foundation of one of the competing systems of wireless telegraphy, so great is the Government's confidence in his impartiality that the only doubt they felt in his appointment was lest he might be biased, owing to excessive conscientiousness, against the system which had made use of his invention. I have no doubt that there are many triumphs in store for Mr. Duddell, and that he will in the future win fresh laurels in that sphere which he has made his own, and on which he has cast so much distinction. He is, I believe, the youngest President in the history of this Institution, but certainly not the least distinguished of them. In the conviction that the future has in store for him many more victories in the field of electrical science to add to his reputation and to add to the distinction which his work has conferred upon electrical science in this country, I propose the health of the Institution of Electrical Engineers, coupled with the name of its President, Mr. Duddell.

The PRESIDENT (Mr. W. Duddell), in responding to the toast, said: I do not think, speaking for my fellow-members of the Institution, that I need say much about the Institution itself. We are all of us greatly interested in promoting its welfare and in extending its scope. The Postmaster-General has spoken of the great advances that have been made in electrical engineering. When this Institution was founded as the Society of Telegraph Engineers in 1871, the only real application of electricity of any importance was telegraphy. Electricity has gradually invaded since that day all the sister sciences; it has become more and more woven into the needs of our daily life. There is hardly anything we now do, there is hardly anything we now employ, which has not at one time or another been partly dealt with electrically. Our food is now cooked electrically; we heat our homes electrically; we light electrically; we supply power to our factories electrically; we now produce our metals from the ores electrically; we are raising them from the bowels of the earth electrically; in fact one really doubts whether there is any domain which electricity will not shortly invade. Electricity is now intimately mixed up with the arts of

metallurgy and with chemistry, and we are beginning to wonder where it will all end. We know certainly that electricity has something to do with the vital processes going on in the human body: it is part of our daily life. It is being applied to agriculture to improve our crops, and there are some people who go so far as to suggest—and not without reason—that mass itself, which we thought was the one thing that was constant in Nature, is after all only a manifestation of electricity in motion. Is it surprising, therefore, that electricity has advanced during the last forty-one years? During that time this Institution has advanced also. The first list of members contained 73 names, and to-day, with the applications for membership that are before the Council, the number is 7,300. We have increased a hundredfold in forty-one years. And I do not think that we have by any means reached the limit. In 1907, for which very convenient figures are available, there were about half a million people employed in electrical industries in this country, and no doubt the number has not diminished since then. Engineering was looked upon in those days as being one of our big industries, and the electrical branch of it was about 14 per cent of the total. What will be the position in two or three years' time when the census of last year is published by the Government? I venture to predict that that 14 per cent will be enormously increased, for there is no branch of mechanical and general engineering which we have not invaded since that time. There is one further point I should like to mention. The Postmaster-General has referred to the exports of this country. What are the imports into it of electrical apparatus? The figures are available in a very convenient form for 1907 as percentages of what we ourselves produced. I find that in general engineering, exclusive of electrical engineering, the imports were about 9 per cent of our total production, while the imports of electrical machinery at that date were about 14 per cent, and of electrical apparatus about 26 per cent. I prophesy that by the time the next census is published the position will have been reversed, and we shall find that electrical apparatus is now imported into this country to a smaller extent than is machinery in general. I do not think I need detain you longer in thanking Mr. Samuel for the kind way in which he has proposed my health and that of the Institution with which I am connected. We all appreciate his kindness in leaving his duties at the House of Commons to attend our dinner to-night, to give us a good send-off, and to wish God-speed to our Institution.

Professor J. PERRY, D.Sc., F.R.S., then proposed the toast of "Our Guests."

The Right Hon. Sir H. B. BUCKLEY (Lord Justice of Appeal), M. GROSSELIN, and Herr GEHEIMRAT CHRISTIANI responded. M. Grosselin said that this year the Société Internationale des Electriciens hoped to arrange in Paris, in May, a joint meeting with the Institution, including visits to works and conferences on various subjects.

A re-union was subsequently held in the Victoria Hall of the Hotel.

THE MECHANICS OF ELECTRIC TRAIN MOVEMENT.

By F. W. CARTER, Associate Member.

(*Paper first received 8th January, and in final form 14th February, 1912; read before the* MANCHESTER LOCAL SECTION *16th April*, 1912.)

A couple of years ago the author, having ventured to criticize certain deductions, made in the course of a paper read before the Institution of Civil Engineers, in that they were mechanically unsound, was answered by an exercise in the *argumentum ad hominem*, commencing with :—

> "The arguments put forward from time to time on this subject by a small group of engineers of American training or association unfortunately gave evidence of an assumption by these gentlemen that ability to deal with the technics of electric-railway engineering was a monopoly of their own. At least they had permitted themselves such an indulgence in mathematical manipulation as could be explained only by their feeling assured that, for some such reason, their figures would be immune from analysis and correction." *

The imputation is untrue, but without bearing on the technical matter then under consideration. The methods that the author used as the basis of his criticism in this case, although so far as he is concerned original with himself, are doubtless commonplace with others whose duty requires them to guarantee performance of electric trains. Whilst the author proposes here to give an account of some of the simpler methods he employs, the object of the present paper is rather to call attention to the reckless disregard of mechanical considerations that has characterized discussion of electric railway subjects in this country in recent years. It is not in recondite technical matters that this disregard is most manifest, but in matters of fundamental mechanics, having no exclusive relation to electric traction. For example, the kinematical principle that the distance traversed is the time integral of the velocity and is thus represented by the area of the speed-time curve, is not infrequently ignored, and the principle of conservation of energy, which requires the energy input to the train to be greater than the output, is not always observed. In fact, there appears to be a disposition in some quarters to treat train motion as *sui generis*

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, p. 224, 1909–10, and vol. 186, p. 164, 1910–11.

and as uninfluenced by the considerations which govern motion in general.

The methods that one might reasonably expect to find in general use for the calculation of train characteristics and energy consumption belong to the province of elementary mechanics. They are simple and straightforward, involving so little of the element of estimation that there is practically no room for bias in their employment. They apply equally well to all systems of operation, and are capable of taking account of all the essential features of the problem. They would naturally be looked for in the earlier chapters of any book of electric railway technology, as they constitute by far the commonest form of calculation that the electric railway engineer has occasion to make. If the author presents the arrangement of work that he himself employs, he must not be understood to be claiming special superiority for it, still less that he is the sole originator and user of such methods. He believes that any competent person having to compute the characteristics of electric train movement would instinctively employ similar or equivalent methods.

Whatever be the ultimate object of the calculation, the immediate problem is assumed to be that of determining whether a given train, driven by an electrical equipment whose characteristics are completely known, is capable of running to a certain schedule, and if so, of determining what the characteristics of the run are—the speed, energy consumption, temperature of motors, etc. The chief feature of the solution is the adoption of speed as independent variable, and the calculation of increments of time, distance, energy, etc., to correspond with given increments of speed. The smaller the increments are taken the more accurate will be the calculated curve, but for practical purposes half a dozen points between rest and free-running speed will usually be found sufficient.

Given the characteristic curves of the motor, for the particular voltage, gear ratio, and size of wheel, the tractive effort at any speed is known. If the opposing force due to train resistance be subtracted from this, the remainder is accelerating force, and knowing the weight of the train the rate of acceleration is known. This divided into the increment of speed gives the corresponding increment of time, which, multiplied by the mean speed, gives the increment of distance. The kinematics and dynamics of the subject are included in this statement.

The tractive effort of a motor is for this purpose naturally measured at the rims of the driving-wheels when running at constant speed. The resistance due to gear, motor brush, and bearing friction is usually, therefore, charged to the motor and does not appear as train resistance. When the speed is varying the tractive effort at the driving rims at a given input is different on account of the motor torque being in part used in accelerating revolving masses.

If F is the tractive effort of a motor measured in the ordinary way, r the radius of the driving-wheels, and g the gear reduction, the torque on each armature (over and above friction torque) is $F\,r/g$. If a is the

acceleration of the train, the angular acceleration of the armature is $g\,a/r$, and of the driving-wheels a/r. If T is the counter torque on the armature, and I′ its moment of inertia—

$$\frac{F\,r}{g} - T = I'\,g\,\frac{a}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If f_1 is the effective tractive effort, I the moment of inertia of a driving-wheel, and i that of a gear-wheel—

$$g\,T - f_1\,r = 2\,I\,\frac{a}{r} + i\,\frac{a}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Eliminating T from (1) and (2)—

$$F - f_1 = \frac{I'\,g^2 + 2\,I + i}{r^2}\,a \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Σf_1 is the total effective tractive effort at the rims of the driving-wheels. Part of this is used in overcoming train resistance (F′), part in accelerating the mass of the train ($M\,a$), and part in imparting angular acceleration to trailing-wheels ($\Sigma\,\frac{I}{r^2}\,a$).

Hence—

$$\Sigma f_1 - F' = \left\{ M + \Sigma\,\frac{I}{r^2} \right\} a \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Eliminating f_1 from (3) and (4)—

$$\Sigma\,F - F' = \left[M + \Sigma\,\frac{I'\,g^2}{r^2} + \Sigma\,\frac{I}{r^2} + \Sigma\,\frac{i}{r^2} \right] a \quad . \quad . \quad . \quad . \quad (5)$$

Thus, $\Sigma\,F$ being the sum of the tractive efforts of the motors, measured as specified above, the influence of rotary inertia is to increase the effective mass of the train by—

$$\Sigma\,\frac{I'\,g^2}{r^2} + \Sigma\,\frac{I}{r^2} + \Sigma\,\frac{i}{r^2},$$

where the first summation is taken for all armatures, the second for all wheels, driving and trailing, and the third for all gears.

The radius of gyration of an average steel railway wheel is about 0·77 of the radius of the wheel, so that approximately—

$$\frac{I}{r^2} = \frac{m\,k^2}{r^2} = 0{\cdot}6\,m. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The order of this quantity is usually about a quarter of a ton for each wheel, or a ton per 4-wheeled bogie truck.

The radius of gyration of a commutating railway motor armature

is usually about 0·7 of the radius of the armature, so that approximately—

$$\frac{I' g^2}{r^2} = \frac{m' k'^2 g^2}{r^2} = 0·5\, m' g^2 \frac{r'^2}{r^2} = 0·5\, m' g^2 \frac{d'^2}{d^2} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

d and d' being the diameters of driving-wheel and armature respectively. Thus if—

$$m' = 1{,}800 \text{ lb.}, \quad d' = 18\tfrac{1}{2} \text{ in.}, \quad d = 40 \text{ in.}, \quad g = \frac{65}{18},$$

$$\frac{I' g^2}{r^2} = 2{,}500 \text{ lb. approximately.}$$

The gear-wheels are of small consequence, adding perhaps a tenth of a ton each to the effective weight of the train.

For the three-coach train considered in the example below the addition for rotary inertia becomes:—

	Tons.
24 wheels	6·0
4 armatures at 2,500 lb. each	4·5
4 gear-wheels	0·4
	10·9

Thus the actual train weight being 118 tons, the effective weight is approximately 129 tons.

There is still much to be written on the subject of train resistance, for whilst long experience with steam trains has given fairly reliable figures for the resistance behind a locomotive, an equal degree of consistency has not been reached for electric trains. There is still doubt as to the amount to be added for head resistance, whilst the resistance of motor coaches seems to be much higher than would have been anticipated,* though whether particularly due to the blocking of the air-way under the train by the motors, to low centre of gravity, to large non-spring-borne weight, or to some other cause, is not clear. However, it is not the author's intention to discuss the subject here, but in the example worked out below the train resistance curve of Fig. 2, taken from an appendix to Mr. Dawson's recent paper,† will be assumed. In suburban service an exact knowledge of the train resistance is not of primary importance, as free-running speed is practically never reached, and the speeds actually attained are hardly affected within the limits of the practicable variation of train resistance. It should be noted that the train resistance when coasting is necessarily higher than when running with power on, since it then includes motor and gear friction. The author usually takes account of this by assuming

* J. A. F. Aspinall, Presidential Address, *Proceedings of the Institution of Mechanical Engineers*, 1909, Appendix iv., p. 473.

† P. Dawson, *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, p. 46, 1910–11.

the coasting retardation in miles per hour per second to be a hundredth of the train resistance in lb. per ton, thus increasing the coasting resistance in about 2 per cent greater ratio than the effective weight, bears to the actual weight (in the example below 11 lb. per ton train resistance with power on is taken as giving 0·11 mile per hour per second coasting retardation, which corresponds to actual resistance of about $12\frac{1}{4}$ lb. per ton). This is, of course, merely a council of laziness, and devoid of scientific justification other than that a little error here has but very little effect on final results.

The method of making a schedule calculation will be best understood from an example. Take, as a typical schedule, that of the South London

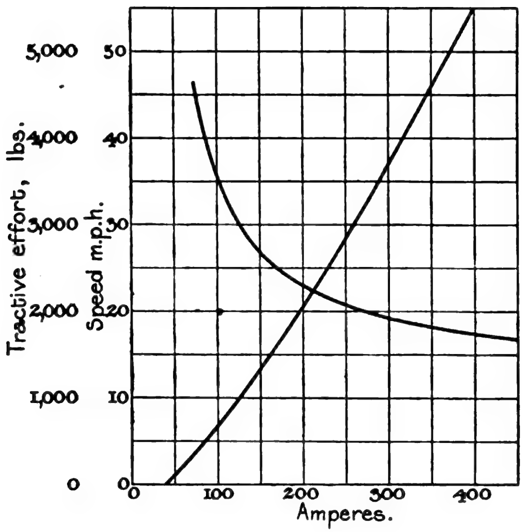


FIG. 1.

elevated trains. Here the total distance of 8·7 miles is timed to be run in 24 minutes, including 20-second stops at each of 9 intermediate stations. The average distance between stations is therefore 4,590 ft., and the running time for this distance may be taken as 124 seconds, thus leaving 20 seconds in hand at the end of the journey. It is proposed that the train consist of three coaches, two being trailer coaches weighing 30 tons each, and one a motor coach (of similar build, but having heavier trucks) weighing 34 tons exclusive of equipment. It is assumed that power is to be supplied to the train at an average pressure of 550 volts continuous current, and it is proposed to use a 4-motor equipment, the motors being of the same type as is used on the Metropolitan District Railway and elsewhere. The characteristic curves of the motor, with 65/18 gear and 40-in. driving-wheels, are given in Fig. 1. The weight of the 4-motor equipment,

with all accessories and structural material, and including air-compressor equipment, is 15½ tons. The total train weight is therefore 118 tons, made up as follows :—

	Tons.
1 motor coach complete	49·5
2 trailer coaches at 30 tons each ...	60·0
Average passenger load...	8·5
	118·0

The effective weight, as shown above, is about 129 tons.

The whole schedule calculation for this case is shown in Table I. In this table the first three columns (except as to the first line) are taken from Fig. 1. The fourth is from Fig. 2, being the ordinate

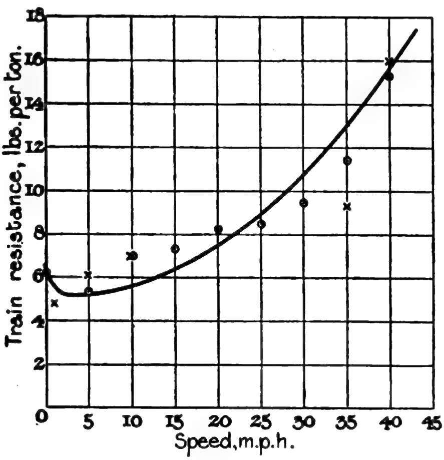


FIG. 2.

multiplied by 29½ (tons per motor). The first three lines of the table taken together cover the period of acceleration on resistance. The figures in the first three columns and the first line are special, being supplied to correspond with starting conditions. Series-parallel control ensures that the train takes 175 amperes per motor for half the acceleration period and 350 amperes per motor for the other half, thus leading to the correct mean in column 12. Whilst these special figures would be different in the case of a different system of electrification—the single-phase system for instance—the calculation of the acceleration section of the table follows along exactly the same lines as any normal section. In order to show more particularly what the process is, consider lines 3, 4, and 5 as embodying a typical section of the table. The figures in the first four columns having been taken from the curves, that in the fifth is the mean between 4,600–210 and 3,300–220.

The increment of the speed in the sixth column is, of course (20·0–18·2). The increment of time is obtained from a single setting of the slide rule by multiplying the increment of speed (1·8) by 3,290, and dividing by the mean accelerating tractive effort (3,735). The figure, 3,290, which applies to the whole table is, of course, 102 times * the effective weight of train per motor in tons. The time (column 8) on the fifth line is obtained to the nearest tenth of a second by adding the increment to the time given on the third line. The speed in column 9 is the mean between 18·2 miles per hour and 20 miles per hour, and the increment of distance (column 10) is this speed (19·1 miles per hour) multiplied by the increment of time (1·58 seconds) and by 1·467 (*i.e.* 5,280/3,600). The distance (column 11) follows by successive addition of increments. The figure in column 12 is the mean between

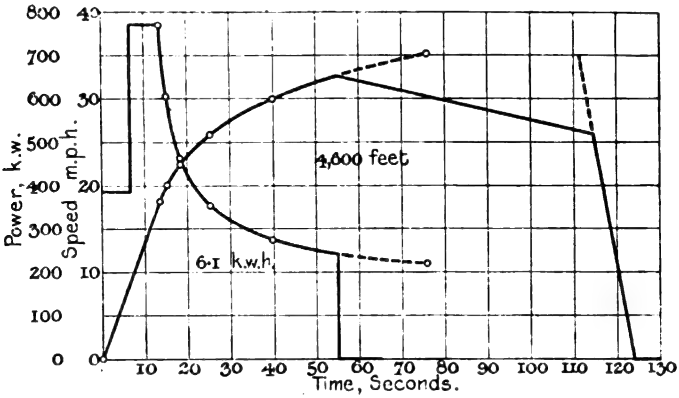


FIG. 3.

350 and 275 amperes, whilst that in column 13 is derived by multiplying this figure by the increment of time (1·58 seconds), again by 550 (volts), and dividing by 3,600. In the case of an alternating-current system one naturally includes columns for power and mean power instead of column 12 of the table, otherwise the calculation proceeds on exactly the same lines as for the continuous-current system.

In the above manner, then, the table is developed as far as line 13, at which point it is advisable to plot the curve between speed and time (columns 2 and 8), and also put in the braking curve to terminate at 124 seconds (Fig. 3). The latter curve has been taken as sloping at the rate of 2·75 miles per hour per second as in Fig. 8, which applies to the service in question. This, whilst a somewhat higher rate than usual, is quite suitable for a fast suburban schedule, as conducive to

* To produce an acceleration of 1 mile per hour per second in an effective mass of 1 ton requires a force of $\frac{2240}{32\cdot2} \times \frac{5280}{3600} = 102$ lb.

Weight
Equipm
Voltage

	1 Current per Motor.	2 Speed.	3 Tractive Effort.	4 Train Resistance.
	Amperes.	Miles per Hour.	Lb.	Lb.
1	175	0·0	4,600	210
2	—	—	—	—
3	350	18·2	4,600	210
4	—	—	—	—
5	275	20·0	3,300	220
6	—	—	—	—
7	210	22·4	2,250	240
8	—	—	—	—
9	160	25·8	1,490	270
10	—	—	—	—
11	125	30·0	990	320
12	—	—	—	—
13	*(100)*	*(35·2)*	*(670)*	*(390)*
14	—	—	—	—
15	111	32·6	—	—
16	—	—	—	—
17	—	26·0	—	—
18	—	—	—	—
19	—	0·0	—	—

Energy consum

low energy consumption. It is now necessary to find by trial the position of the coasting curve which makes the total area included equivalent to 4,590 ft. If power is cut off after 55 seconds the total area included works out at 4,602 ft., which is probably near enough to the 4,590 ft. required, allowing a little for contingencies. Lines 12 and 13 are now abandoned in favour of lines 14 and 15 and the table completed in accordance therewith. The energy consumption works out at 6,100 watt-hours for the 118-ton train per 4,600 ft., leading to 7 kw.-hours per train-mile, or 59·4 watt-hours per ton-mile.

Such being the straight dynamical solution of the problem of train movement, it is perhaps desirable to discuss its practical value, and to inquire in what measure it corresponds with the facts it is supposed to represent. There are few points in connection with the solution that have not been subjected to rigorous analysis and careful comparison with test. So far as concerns the principle of the method it is as little open to objection as the customary calculations on Atwood's machine or the ballistic pendulum, and must be accepted unless one is prepared to tilt against the laws of motion. The most obvious theoretical objection to the method is that the increments of speed are taken by no means small, whilst for accuracy they should be infinitesimal. It is open to any one to make the calculation with increments as small as he pleases, or by a simple and obvious use of curve paper to get the effect of infinitesimal increments. When he has done this, however, and plotted his train characteristics he will find that they differ inappreciably from the author's.

It will be seen that the increments of distance and energy in Table I are computed as if successive points on the speed-time and energy-time curves were joined by straight lines. Accordingly (from the direction of curvature), the distance, as measured by planimeter, should be, if anything, a little greater than calculated, and the energy a little less. The period of acceleration to the speed curve is treated in the calculation as if the number of controller points were infinite and the rheostats adjusted to give uniform accelerating current per motor, whereas actually a few controller points only are employed, resulting in as many peaks of current. With rheostats adjusted to give uniform peaks, however, this deviation from actual conditions is without appreciable effect on the speed, energy consumption, or loss in rheostats, so that there is no need to complicate the calculation with nearer approach to actual conditions in this respect.

Where such a schedule calculation is made to correspond with the conditions of a test, there is clearly no reason for anticipating sensible difference between the results of calculation and test, and as a matter of fact no such difference is discernible. As a published instance of this one may direct attention to the figure of 92·8 watt-hours per ton-mile given by Mr. Deeley in the communication quoted below,* as the input to the motors alone in a certain test—which happens to have been made on straight and level track—and the figure of 91·5 watt-hours per

* See Table I, p. 445.

ton-mile obtained by the author from a calculation of the same schedule *—in deducing which figure it may be remarked that the author had no exact knowledge of the train, nor was he interested in making the manner of running in his calculation exactly correspond with the test, as comparison between test and calculation was quite beside the purpose of his argument. Such differences as occur between the results of test and calculation are usually, in fact, traceable to imperfect knowledge of the circumstances of the test. Guarantees of performance, for instance, have often to be made before trains are built, so that only estimated train weights and other particulars are available. The guarantor then naturally keeps a little in hand, to cover possibly adverse conditions. The specified rate of braking, again, is often much below the realized rate, and the maker of the equipment gets quite undeserved credit for improving on his guarantees. But where the circumstances are known the performance of the train can be com-

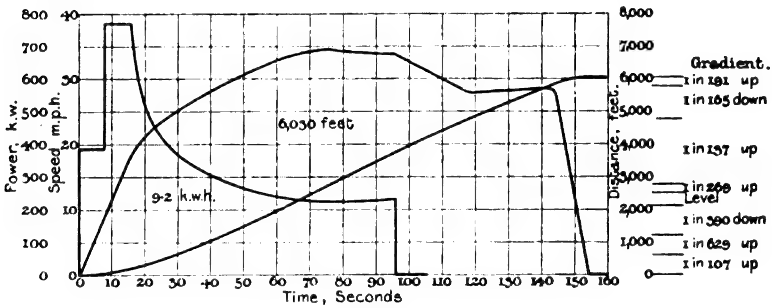


FIG. 4.

puted with as great a degree of precision as it is practicable to determine it by test.

The complete calculation for an actual run although introducing no new principle is usually, of course, much more elaborate than the above example. In particular the gradients are very troublesome, not as gradients—for their effect is accurately included in train resistance—but from the fact that the point in the calculation where a change of gradient occurs has to be determined by trial, which operation is apt to consume much time and patience. A far more expeditious method for runs extending over many gradients was developed by the author some nine years ago and published by the American Institute of Electrical Engineers.† The method in question is an analytical one, employing a system of universal speed-time and speed-distance curves from which those appropriate to the problem are selected. However, as these curves were not reproduced in sufficient detail or on large enough scale to be of value, it is unlikely that any one but the author makes use of

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, p. 199, 1909–10.
† *Transactions of the American Institute of Electrical Engineers*, vol. 22, p. 133, 1903.

them. As an example of the method the train characteristics have been computed for runs in the two directions over the route to which curves A and B of Fig. 8 correspond, using, however, the same train and equipment as was employed in calculating Table I. and Fig. 3. The results are represented in Figs. 4 and 5. It may be remarked that the method of the present paper was assumed as known in the American Institute paper, and was there referred to as the "point-to-point method."

Although a somewhat long and tedious calculation is usually necessary to furnish a complete representation of a particular service, it must not be thought that such average calculation as is exemplified in Table I, and Fig. 3, is without value. On the contrary, the results obtained from the average schedule run are in general very fair approximations to the average results corresponding to the actual schedule. Of course, if the line is, like the Central London Railway, graded so as to dip in each direction from every station, the schedule can be run with less

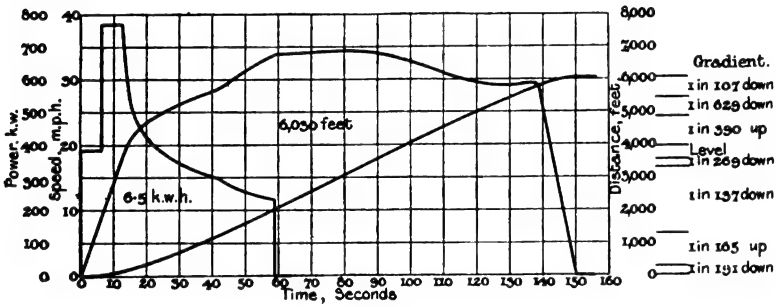


FIG. 5.

powerful equipments and with lower energy consumption than would otherwise be the case, but where the stations are promiscuously arranged with reference to gradients, the average energy consumption for runs which return to the starting-point is not usually more than 2 or 3 per cent greater than it would be if the track were level everywhere—the extra power required at one place being practically given back at another. In the worst case that the author has had occasion to investigate, viz. for the line between Finsbury Park and High Barnet, there is a continuous up-grade averaging 1 in 72 for the first $2\frac{1}{8}$ miles from Finsbury Park, after which the line undulates. In this case, with stations about a mile apart, the average energy consumption for runs in the two directions worked out about 6 per cent greater than if the track had been level. Small as is the usual effect of gradients on the average performance, the effect of variation in distance between stations is even smaller—the performance for the average distance agreeing very closely with the average performance for the actual distances. The effect of curves, junctions, etc., unless such as to impose restriction

of speed, is small and sufficiently well met by taking a somewhat high figure for train resistance.

Altogether the train characteristics of an average schedule run are of great value and significance, corresponding closely with the general performance of the train. How close the correspondence is depends on the particular features of the problem, but if a complete investigation of the South London elevated service—represented typically in Table I—were made, there is no doubt that the equipment would be found dynamically sufficient for the work specified, and it is unlikely after allowing for all possible contingencies that one would find it necessary to fix a higher figure than 7½ kw.-hours per train-mile for the guaranteed energy consumption of the 3-coach train in question under the conditions of actual service tests over the route.

Enough has perhaps been said to show that any one who possesses the requisite data and who cares to take the requisite trouble, can determine the effect on a train of a given equipment driven in a given manner over a given route. Much less elaborate methods, however, are often sufficient to determine whether a stated result is within the bounds of practicability or to show that some train test is valueless, either as being inaccurate or as being affected by some accidental circumstance which destroys its value as characteristic performance. It is proposed now to discuss a few published cases, typical of much that has been written on the subject in this country, in which claims are made whose untenability is obvious from the simplest mechanical analysis.

As a first example it might be advantageous to discuss that which led to the remarks quoted in the first paragraph, for, although some calculation is necessary to determine the full facts, elementary mechanical considerations indicate that something is wrong. The issue in this case was more consecutively stated in a communication * from the late locomotive superintendent of the Midland Railway than in the paper from which the above quotation was taken.

The letter in question was directed at Mr. Aspinall, and it is sufficient for the present purpose to quote a few paragraphs only :—

"The tests were made on the level lengths between Lancaster and Heysham. The train weighed 51¼ tons, and was worked over a distance of 1,760 yards, starting from rest. This was accomplished in 119 seconds, the mean speed being 30·25 miles per hour.

"Table 'A' gives the leading particular of the train used.

"In Table I. the mean results of 12 tests, 6 in each direction, taken on different days, are shown. On Diagram 1 † attached is plotted the most representative single curve. The total consumption was measured by meter on the high-tension circuit, and the figures agreed with the sum of the watt-hours registered by the low-tension meters.

* R. M. Deeley, in the *Railway Gazette*, vol. 47, p. 8c, 1909; see also *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, p. 88, 1909–10.
† See Fig. 6.

"TABLE 'A.'

Train for 26 Miles per Hour Schedule, including one 20-second Station Stop per Mile.

	Tons.	Passengers.
2 motor-cars (Siemens' revised weights) ...	72	144
1 trailer-car	18	54
	90	198
Weight of 198 passengers	12·5	—
	102·5 tons total.	

Total horse-power = 4 by 210 = 840.
Horse-power per ton of train = 9·33 (without passengers).
Horse-power per ton of train = 8·2 (with passengers).
Test train, 51½ tons, 420 H.P.
Horse-power per ton = 8·15.

"The diagrams were taken by properly calibrated testing instruments of the indicating type, the curves being obtained by photographic records from them on a mechanically driven drum. The variation during acceleration of the curves on the diagram shows the action of the steps of the controller as the speed increases, and

"TABLE I.

Driving-wheels	43·5 in. diameter.
Gear ratio...	88/30.
R.M.S. current ... ... =	445 amperes.
R.M.S. current ... ... =	11 per cent. over continuous capacity of motor.
Watt-hours per ton-mile (low-tension) =	92·80
Main transformer, C^2R loss ... ... =	1·20
Main transformer, iron loss ... ... =	2·35
Auxiliary transformer losses ... ... =	0·25
Vacuum pump ... ... =	1·59
Blower ... ... =	1·00
Control ... ... =	0·11
Watt-hours per ton-mile ... (Total) =	99·30

Sustained acceleration ... = 25 miles per hour in 22 seconds.
Maximum acceleration ... = 1·3 mile per hour per second.
Maximum acceleration ... = 1·91 ft. per second per second.

the effect of the voltage variation regulator at the station. It will be seen that, though the horse-power rating of the motors per ton of loaded train was only 8·15, the accelerations obtained and the energy consumption are both figures that compare well with the published results of the Lancashire and Yorkshire and other continuous lines.

"To obtain the best results possible with the given speeds and distance, which are regarded as typical of the requirements of urban and suburban services, somewhat altered gear ratios and wheel dimensions would have to be adopted.

"In Diagram II.* attached, and Table II. below, the curves and figures have been altered to show what the effect of such a change of motor purchase would be. The diagrams in this case are based on the curves representing the average of the test results :—

"TABLE II.

Driving wheels		40 in. diameter.
Gear ratio...		3·7/1.
R.M.S current	=	360 amperes.
R.M.S. current	=	10 per cent. under continuous capacity of motor.
Watts-hours per ton-mile (low-tension)	=	77·50
Main transformer, C^2R loss	=	1·03
Main transformer, iron loss	=	2·35
Auxiliary transformer losses	=	0·25
Vacuum pump	=	1·59
Blower	=	1·00
Control	=	0·11
Watt-hours per ton-mile ... (Total)	=	83·83

Sustained acceleration ...	=	19 miles per hour in 13 seconds.
Maximum acceleration ...	=	1·8 mile per hour per second.
Maximum acceleration ...	=	2·65 ft. per second per second."

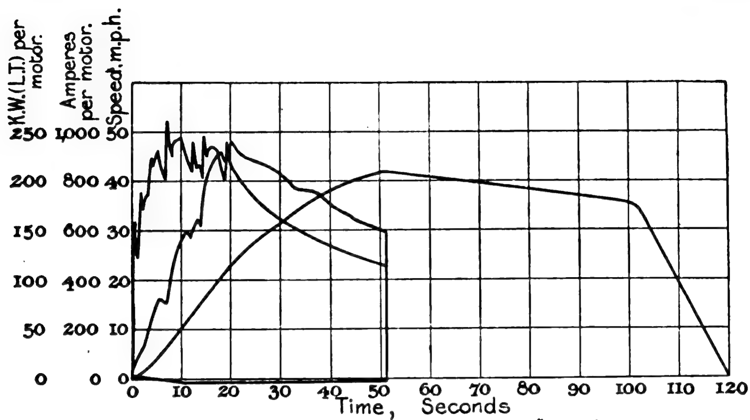
Here, then, the case is presented with clearness and simplicity. A test which bears evidence of care leads to an energy consumption at the train of 99·3 watt-hours per ton-mile. As a test result this is not remarkable in any way, and, in fact, indicates distinctly poor equipment efficiency. Mr. Aspinall claims a lower energy consumption at the train for a harder schedule.†

It is not likely therefore that publication would have been made on the strength of this result. However, from the point of view of low energy consumption, the gears are clearly not quite the most suitable, though experience would have raised no expectation of any great saving from their change. It is, of course, impracticable actually to change the gears, and our author, bereft of the wholesome guidance of electrical measuring instruments, appears to have been chiefly assisted by a strong bias in making his estimate. The figure of 83·8 watt-hours per ton-mile (or 83·83 to be exact) is admirably suited to bear its computor's conclusions, but hardly in accordance with expectation or consistent with the result obtained by test.

* See Fig. 7.

† *Proceedings of the Institution of Mechanical Engineers*, 1909, p. 432.

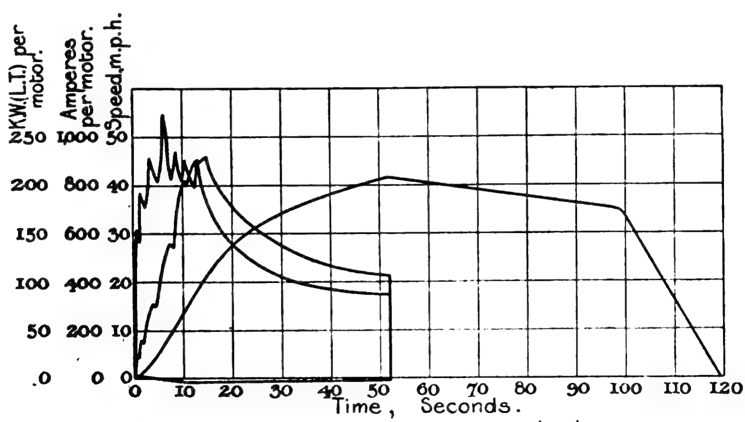
There are two ways in which change of gear can reduce the energy consumption—one by running the schedule in a somewhat different manner, so as to reduce the output required; and the other by doing



51½ ton, train; 88/30 gear; 43½" wheels.

FIG. 6.

more of the work when the equipment is running under conditions of higher efficiency and less under conditions of lower efficiency. Inspection of the two diagrams referred to, which have been reproduced in



51½ ton, train; 3·7 gear; 40" wheels.

FIG. 7.

Figs. 6 and 7, shows immediately that the output of the equipment is not sensibly reduced by changing the gears. For, the mean speed being the same, there can be no particular difference in the work done

against train resistance, and as the brakes are put on at about the same speed, much the same amount of kinetic energy is dissipated in them, the slightly higher speed in Fig. 6 being amply compensated by the greater effective train weight in Fig. 7, in which the rotary inertia of the armatures is almost doubled. The enormous saving of power would therefore have to be entirely made in the equipment—the diminution in the losses in each motor alone amounting to $15.3 \times 25.6 \times 26$ watts, or 10 kw. This is, of course, out of the question, and is sufficient proof that the results are untrustworthy. However, the characteristic curves of the motor have now been published,* and the comparative energy consumptions of the trains with the two gears, but under otherwise exactly similar conditions, can easily be computed. The author's calculations, made exactly as suggested in the present paper, show less than 3 watt-hours per ton-mile saving as resulting from the change of gear.† It should be remembered that the difference figure alone is pertinent to the issue, and this is substantially unaffected by any small error in fundamental assumption or indefiniteness in conditions. If Mr. Dalziel still considers his result justified, it is to be hoped that he will now disclose the course of reasoning by which he obtained it.

The second example is taken from a paper by Mr. P. Dawson,‡ and shows a disregard of elementary mechanical principles without parallel in the author's experience. Attention was directed to the matter in the course of the discussion on the paper,§ but it is here dealt with in greater detail :—

"The energy-consumption has been worked out for the South London train, for an average distance between stations of 6,535 ft. and an average speed, including stops, of 33·4 miles per hour ; with the present gear-ratio it would amount to 71 watt-hours per ton-mile at the train. The gear-ratio, however, was designed for the South London conditions, and by altering it a further economy on the figure given should result. A three-coach train on the Lancashire and Yorkshire line is stated to require under similar conditions 96 watt-hours per ton-mile on the train."

In order to appreciate this passage, it should be noticed that to run at a speed of 33·4 miles per hour, the distance of 6,535 ft. has to be run in 133½ seconds, which time includes the stop according to the text. To accomplish this feat a train is used, the speed-time curves of which in the two directions when running a distance of 6,030 ft. on a line of average gradients are as shown in Fig. 8 || (curves A and B). The time in one direction is stated to be 154·5 seconds, and in the other 150 seconds. This is actual performance, and consistent with the

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, Plate II, Fig. 18, 1909–10.

† Ibid., p. 200.

‡ P. Dawson, "The Electrification of the London. Brighton and South Coast Railway," *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, p. 36, 1910–11.

§ Ibid., p. 126. Since the above was written, Mr. Dawson's reply has appeared (Ibid., p. 162).

|| *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, 1910–11, Plate IV, Fig. 29, "East Brixton and Clapham Road."

whole recorded performance of the trains and the requirements of the service for which the trains were designed. But, of course, the running time could be reduced by keeping power on longer, though it does not follow that it would be advisable, or even practicable, so to reduce it in regular service.

It is apparent from the train resistance and motor curves that the free-running speed of the train on level track is below 45 miles per hour, for at this speed the total tractive effort of the motors at full voltage is about 2,000 lb., and the train resistance, according to the formula * given in the paper—which seems to agree with the tests—is above 2,500 lb. However, taking the free-running speed on level track as 45 miles per hour whilst continuing the acceleration curves to this speed in the most optimistic manner one conceives to be possible in order to obtain a more favourable result than one expects to realize, the very best time in which the distance of 6,535 ft. can be run seems to be a little over 135 seconds † (Fig. 8, curve C). But, of course, it would be quite impracticable to work to this in service, and the most favourably disposed person, however ignorant of the difficulties of railway operation, would hardly venture to suggest less than 140 seconds running time for the 6,535 ft. as a practicable everyday schedule. Thus it is kinematically impossible to run the given distance in the time specified, with the train whose acceleration curves are as given by Mr. Dawson. The result will perhaps be clearer if put in the form of a table :—

TABLE II.

Distance.	Time.	Remarks.
Feet.	Seconds.	
6,030	152 (excluding stop)	Actual performance.
6,535	140 (excluding stop)	Suggested dynamical limit of practicable performance.
6,535	135 (excluding stop)	Best theoretical performance estimated with optimism.
6,535	133½ (including stop)	"Worked out," method not divulged.

Energy consumption has no meaning as applied to a service beyond the dynamical capacity of the train, but it may be of interest to estimate the output of the equipment when the train is doing its best to run to the schedule given (see curve C, Fig. 8). Taking the average train

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, p. 34, 1910–11.

† That is to say, the area of the curve cannot be made to represent 6,535 ft. in other time than this.

resistance as 14 lb. per ton, the work done against this amounts to about 28 watt-hours per ton-mile.* The energy dissipated in braking from 45 miles per hour, allowing $12\frac{1}{2}$ per cent increase in weight for rotary inertia, amounts in this case to 52 watt-hours per ton-mile.† Thus the energy output of the equipment is of the order of 80 watt-hours per ton-mile. The energy input necessary just to run the distance of 6,535 ft. in 135 seconds without margin, using the trains in question, would be of the order of 110 watt-hours per ton-mile. The author of the passage quoted appears, however, to have been able to satisfy himself that his trains are able to run the distance of 6,535 ft. in $133\frac{1}{2}$ seconds, including the stop, and with such margin implied as is customary in railway service, with an energy input of only 71 watt-hours

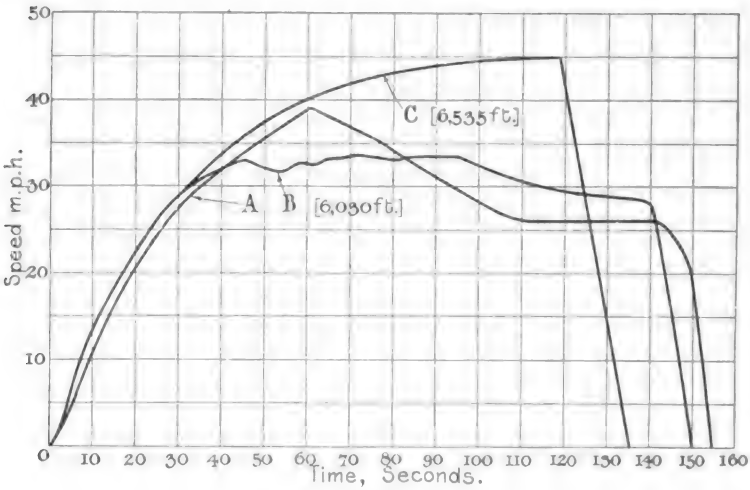


FIG. 8.

per ton-mile. It is, moreover, suggested that even this is not the best possible, for further economy is predicted from the use of more suitable gears. A lower speed gear would, however, render the equipment farther than ever from running to the schedule in question, whilst a higher speed gear would result in further increase of energy consumption. Other examples ‡ of the same kind are to be obtained from the same source.

* 1 lb. per ton train resistance implies 5,280 ft.-lb. of work per ton-mile or—

$$\frac{5280 \times 746}{33000 \times 60} = 1{\cdot}99 \text{ watt-hours per ton-mile.}$$

† 1 ton actual weight or $1{\cdot}125$ tons effective weight, moving at v miles per hour has kinetic energy $2240 \times 1{\cdot}125 \times \frac{v^2}{2g} \times \left(\frac{5280}{3600}\right)^2$ ft.-lb. $= 0{\cdot}0283 \times 1{\cdot}125\, v^2$ watt-hours.

‡ See for instance the paragraph following that quoted.

Sometimes misapprehension arises through mistaking some casual record, in which possibly conditions are unknown or sources of error overlooked, for a normal and characteristic one. This may be the explanation of a doubtful statement occurring in Mr. Aspinall's Presidential Address to the Institution of Mechanical Engineers *: "The consumption of current at the train is . . . 61 watt-hours per ton-mile for 6-car stopping trains weighing 196 tons, and 96 watt-hours per ton-mile for 3-car stopping trains weighing 117 tons."

We are not told that the 6-car and 3-car trains work to the same schedule and under the same conditions, but in the absence of any statement to the contrary it is a legitimate inference. We are therefore justified in questioning one of the figures for energy consumption. The author, having had occasion to study the Liverpool-Southport stopping service in detail, has no hesitation in stating that 96 watt-hours per ton-mile is a reasonable figure for the 3-coach train running to this schedule, being, if anything, on the low side. A slightly lower figure is to be anticipated for the 6-coach train, as the train resistance per ton is less the longer the train.

In Appendix IV to his Presidential Address Mr. Aspinall gives something on the resistance of his trains † and finds this to be 12 lb. per ton for the 3-coach train at 40 miles per hour and 10·5 lb. per ton for a 7-coach train at the same speed. The difference of 1·5 lb. per ton would lead to a diminution in output of 3 watt-hours per ton-mile or in input of perhaps 4 watt-hours per ton-mile. As the energy dissipated in the brakes is proportional to the weight of the train, there is no reason to anticipate further reduction in the energy consumption per ton-mile due to diminution in the portion of the output so dissipated. The idea that the energy consumption is greatly reduced in long trains seems somewhat widely prevalent, being doubtless derived from experience with long-distance steam trains, which are, of course, quite a different matter.

It is desirable that Mr. Aspinall should reconsider his figure of 61 watt-hours per ton-mile or give details of the service to which it refers, as it is certainly much below the mechanical output of the equipment in any practicable way of running to the accepted schedule of the Liverpool-Southport stopping trains. ‡

However, better examples of confusion between accidental observation and characteristic performance are to be found in the recent paper read before the Institution of Civil Engineers on the London, Brighton and South Coast Railway electrification. The following case § was referred to in the course of the discussion and may without impropriety be mentioned here.

* *Proceedings of the Institution of Mechanical Engineers*, 1909, p. 432; see also *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, p. 123, 1909–10.

† *Proceedings of the Institution of Mechanical Engineers*, 1909, p. 477.

‡ 18½ miles in 37 minutes, including 14 intermediate station stops. If, as seems probable, the 6-car train contains only two motor-cars, it is clear that it would be incapable of running to this schedule.

§ *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, p. 29, 1910–11.

"In order to show the influence of long runs on the energy consumption a non-stop run was made from Victoria to London Bridge, this being the only direction in which such a run was possible, owing to traffic conditions. This run was made on Sunday, the 13th November, 1910, and occupied 14 minutes 8 seconds for the complete distance of 8·7 miles, giving an average speed of 37 miles per hour. The consumption measured at the Denmark Hill switch-cabin was 42 units, and that given by integrating the chart of the recording wattmeter at Deptford was 40·7 units. Taking 41·4 as the mean value, this gives a consumption of 34·4 watt-hours per ton-mile. In addition, however, to several unfavourable gradients, there is a net rise of 25 ft. in level from Victoria to London Bridge, which represents an energy-consumption of 2·9 units for the 138-ton train of this test. Making this allowance, the figure for energy-consumption becomes 32 watt-hours per ton-mile.

"The track followed has many curves and gradients, and certain speed restrictions are thereby imposed, which should be taken into consideration in judging the merits of this performance. The scheduled speed would also have been increased had the run been on a level straight track of the same distance.

"The maximum speed attained during the run was 50 miles per hour, which is probably the highest practicable speed on this line, in consequence of the curves and other conditions which necessitate slowing down."

There is more of it later,* including the customary comparison with Mr. Aspinall's line; briefly, then, in a non-stop run of 8·7 miles, at a mean speed of 37 miles per hour and a maximum speed of 50 miles per hour, the energy input is estimated from the tests at 32 watt-hours per ton-mile. In judging the merits of this performance, the fact that the mean train resistance is higher than the train resistance at mean speed and is not likely to be less than that at 40 miles per hour, should be taken into consideration. The train resistance of the 3-coach train on straight and level track was made the subject of test, and the results are given in Appendix V. of Mr. Dawson's paper.† This at 40 miles per hour was found in one series of tests to be 16 lb. per ton, and in another series 15·3 lb. per ton. The track, however, as is duly and frequently pointed out, is not straight and level, but has many curves and gradients, which impose restrictions on the speed, and without vouching for the accuracy of the train-resistance tests one may safely assert that to assume a mean resistance of 16 lb. per ton does not err in the direction of excess. This requires an output from the equipment of 32 watt-hours per ton-mile. But this is not all, for the kinetic energy of the train which is ultimately dissipated in the brakes may amount to some 5 watt-hours per ton-mile. Thus the total output of the equipment is of the order of 37 watt-hours per ton-mile, and we are asked to accept as the appropriate input the figure of 32 watt-hours per

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, p. 36, 1910–11.
† Ibid., p. 46.

ton-mile. It appears more likely that a favouring wind was blowing or that alternating-current station instruments of great capacity are useless for registering energy at so small a rate, than that the system employed is unrestricted by the principle of conservation of energy.

The foregoing will suffice to show that there is neither mystery nor difficulty about the mechanics of electric train movement and no excuse for the nonsense which has in great measure marred discussion of electric railway matters in this country. The author offers this contribution to the subject by way of protest against the presentation of biased estimates as facts and of claims of superlative merit for performances that are in every way commonplace; also as a plea that members of this Institution insist that their discussions of the subject be made on the basis of reason and fact alone.

Discussion.

Mr. E. O'Brien: The question of train resistance is of considerable interest; though it is not of very great importance on suburban work, it is of some importance, and it is of very great importance in connection with the larger electrifications which are bound to take place in the future. In Mr. Aspinall's Presidential Address to the Institution of Mr. O'Brien.

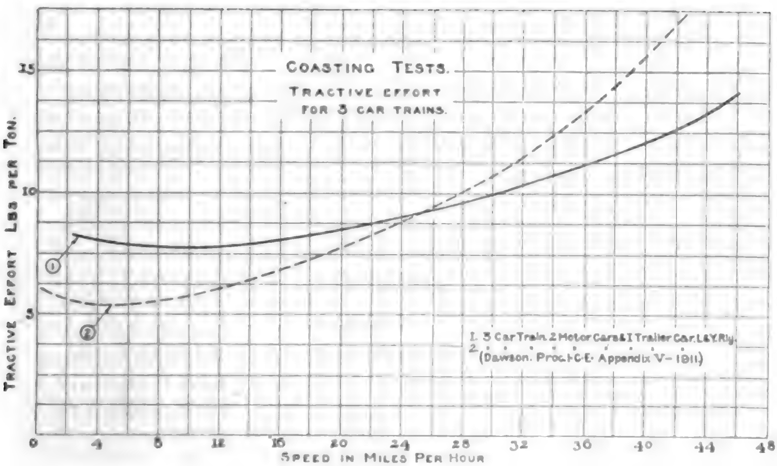


Fig. 9.

Mechanical Engineers, certain train resistance curves were given. It will be seen that these curves show rather lower train resistance at the higher speeds than those given in the author's paper. In making the experiments from which these curves were produced it was soon found that it was exceedingly difficult to determine the train resistance of an electric train, and that the determination of the train resistance of the coaches of a steam train was an easy matter in comparison. The only

Mr. O'Brien. satisfactory way, in my opinion, of obtaining train resistance is by coasting, and very delicate and accurate instruments are required in order to measure the retardation accurately. A very suitable instrument is one used in the dynamometer car on the Belgian State railways. This instrument, Doyen's Inertia Ergometer, is an application of the Amsler integrator to the Desdouit's inertia dynamometer. It integrates distance and inertia resistances alone, and records a curve whose ordinates represent the work done against these resistances. It is operated by a pendulum whose indications are unaffected by the action of gravity on gradients. In all other tests it is necessary to have a very exact survey of the line. This will be more fully realized when it is considered that a gradient of 1 in 2,000 makes a difference of about 1 lb. per ton in the train resistance, and as the train resistances being dealt with are of the order of 10 lb., an inappreciable error in the gradient will cause a 10 per cent error in the train resistance results. The author refers to a consumption of 61 watt-hours per ton-mile referred to in Mr. Aspinall's Presidential Address already mentioned. Mr. Carter's footnote to the effect that "If, as seems probable, the six-car train contains only two motor-cars, it is clear that it would be incapable of running to this schedule" is correct. Practically all the trains on the Liverpool and Southport line run with two motor-cars with a varying number of trailer-cars, and the schedule varies slightly with the number of trailers. It should also be pointed out that the motors in the six-car train are working at a considerably higher efficiency when running on the motor curve than in the case of the three-car trains. The combination of these facts explains the discrepancy. Comparisons have so frequently been made with the Liverpool and Southport line that I think it is here desirable to clear up a misunderstanding which continually occurs in making these comparisons. It is assumed that the average length of run on the Liverpool and Southport line is the distance between Liverpool and Southport divided by the number of stations in that length. As a matter of fact 70 per cent of the train mileage run on the Liverpool and Southport line is run between Liverpool, Hall Road and Aintree, and Southport, Meols Cop and Crossens. The average distance between stations on these lengths is approximately 1 mile and not 1·32 miles, which is the average distance between stations between Liverpool and Southport. The line between Liverpool, Hall Road and Aintree, and Southport, Meols Cop and Crossens, is neither flat nor straight; it has considerable gradients and abounds in curves. The flat, straight portion of the line is that between Hall Road and Southport on which comparatively a small portion of the mileage is run. The difference in vertical height between Liverpool and Southport is 50 ft., and the maximum difference on any part of the electrified line is 135 ft. (I give this for what it is worth.) This compares with a difference of 25 ft. between Victoria and London Bridge on the L.B. & S.C. line (see Fig. 10).

It is a pity that there has been so much comparing of tests that

Mr. O'Brien.

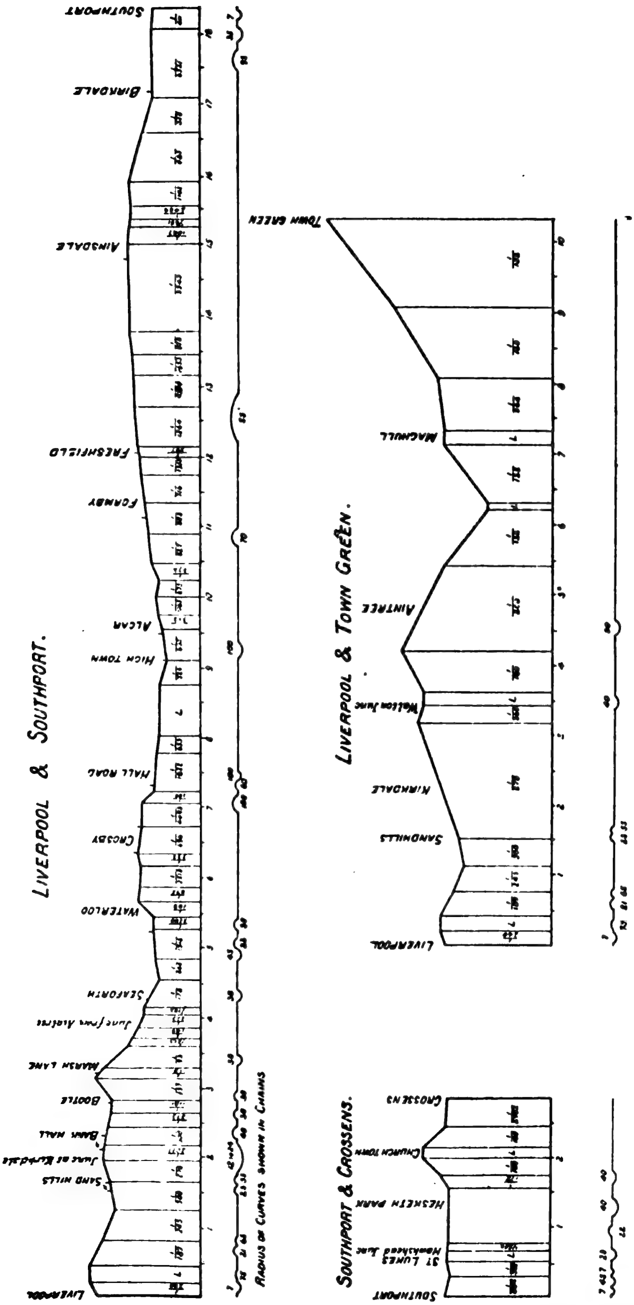


FIG. 10.

Mr. O'Brien. are not comparable. The only tests that we really can compare are actual tests made with actual equipments on level line over a given distance and at a given schedule, and owing to the very large increases of energy consumption that occur with comparatively slight increases in the schedule when running at high speeds with one stop or more per mile, it is of the highest importance that such comparative tests should be conducted with the greatest accuracy. The necessary accuracy for comparison is seldom attained, and the author is to be congratulated on drawing attention to the inaccurate comparisons of totally different services that have been made. The author indicates that varying gear ratio can have but little effect on the energy consumption, and I agree with this conclusion. If the gear ratio is increased, and if a higher acceleration is maintained while running on the resistances the resistance losses have a smaller value during this period, but the acceleration on the motor curve falls off proportionately more rapidly, and less coasting can be done with consequently greater brake losses; on the other hand, if the gear ratio is reduced the starting acceleration is reduced. The resistance losses are therefore greater and the notching period is longer and more coasting can be done. Thus the losses are approximately the same in both cases. I look forward to considerable developments from the high-tension continuous-current system. It is now well known that the Lancashire and Yorkshire Railway Company are going to electrify a short length of line between Bury and Holcombe Brook at 3,000 volts continuous current. It is not anticipated that the consumption of current at the train will be any way different from that of any other continuous-current train, but it is certain that the capital cost of rolling stock will not be appreciably greater than that of the low-tension continuous-current rolling stock, and that a very high efficiency of transmission will be obtained between the power station and train.

Mr. Cunliffe. Mr. R. G. CUNLIFFE: The considerations of mechanics dealt with by the author are obviously derived from the laws of motion and are beyond criticism. I have no doubt that all engineers studying the subject make use of methods similar to those used by the author. Carrying equation (5) somewhat further, it becomes possible to determine the fundamental relationship between the acceleration during a given time interval after starting a train and the b.h.p. required to sustain such acceleration:—

$$\Sigma F = F' + \left(M + \Sigma \frac{I' g^2}{r^2} + \Sigma \frac{I}{r^2} + \Sigma \frac{i}{r^2}\right) a \quad . \quad . \quad . \quad . \quad (5)$$

$$= F' + K a \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$K =$ constant for a given equipment.

Speed $= a\,t$ (at t seconds after start).

b.h.p. $= k\,a\,t\,\Sigma F$.

Substituting values of ΣF—

$$\text{b.h.p.} = k\,t\,(F' a + K a^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Mr. Cunliffe.

The b.h.p. is therefore proportional directly to the time element considered, to the first and second powers of the acceleration, to the train resistance, and to the physical characteristics of the equipment. Equation (7) is important from the point of view of maximum demand per equipment, and indicates that, with a given equipment, the acceleration can only be increased by increasing the maximum demand which in the train performance is represented by the peaks of energy or current curves. Equation (6) indicates the effect which a change of ΣF, such as could be brought about by alteration of gear ratio or wheel diameter, has upon the resulting acceleration of the train. F' is external to the train and not subject to any great modification by dynamic changes in the train equipment. K changes with any dynamic change made, but is a factor representing a fractional component only of the total tractive effort and does not change by any large amount. Large changes in ΣF must therefore produce still larger proportional changes in the acceleration. In considering the train performances represented by Figs. 6 and 7, as first put forward by Messrs. Dalziel and Sayers, we find that the speed graph does not coincide with any of the motor characteristics published by these writers,* and that it refers, in fact, to a higher voltage characteristic than any of those given. In their reply to the discussion on their paper † the voltage is given as 340 volts, and there is some ground for complaint in that such curve was not given with the remaining and less important characteristics. In the absence of the 340-volt characteristic it is not possible to check the whole of the train performances, but the second may be partially developed from the first, on the assumption that the first is correct, as follows: The result of the dynamic changes is to increase the tractive effort, as compared with the normal values, by 37 per cent and to decrease the speed by 27 per cent. New characteristic curves for tractive effort and speed may be prepared, whilst a portion of the missing characteristic at 340 volts may be obtained from the logarithmic portion of the current curve and its corresponding speed curve in Fig. 6 between limits of 800 and 460 amperes. By means of the above curves the first half of the second accelerating period when the motors are running on 340 volts may be checked in Fig. 7 as against Fig. 6, and the agreement is sufficiently close. The important point, however, in comparing the two performances is in the first portions of the curves, whilst the controller is in use. The times taken in operating the controller are respectively 20 and 13 seconds with similar energy and current peaks, and since the power input is similar in both cases the time saving represents a saving in energy. The acceleration shown in Fig. 7 is much lower than would be expected from the proposed dynamic changes, and the speed curve does not support the low notching time given. I cannot accept the starting peaks of the current curve with

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, Fig. 18, Plate 2, 1909.

† Ibid., vol. 179, p. 224, 1909.

Mr. Cunliffe. the rapid notching shown. Such peaks should be much higher than indicated or, alternatively, the time during which the controller remains on any notch should be longer. Fig. 6 shows clearly that a more rapid use of the controller would have produced results far more satisfactory and with lower energy consumption, whilst much of the saving indicated by Fig. 7 is due not to the dynamic changes made, but to the more efficient use of the controller. In my own practice I have found without exception that very large percentage variations exist between the results given by different motormen using the same equipment, to the same schedule, on the same track, merely by differences in method of control of their equipment. The results have been such as to prove that the maximum demand varies inversely as the rate of energy consumption during the accelerating period.

Mr. Moffet. Mr. J. L. MOFFET: On page 443 the author states that the energy consumption up and down a gradient of 1 in 72, with stops at an average distance apart of 1 mile, is only 6 per cent greater than on a level track with the same distance between stations. I have made some calculations for a run of 1 mile, at an average speed of 30 miles per hour, on a gradient of 1 in 100, with a train weighing 144 tons. The energy consumed worked out to 26 kw.-hours for two 1-mile runs on the level, 23 kw.-hours for a 1-mile run up the gradient, and 9 kw.-hours for a 1-mile run down the gradient. This shows an excess of 23 per cent for the run up and down the gradient as compared with two 1-mile runs on the level. This difference from the result quoted by the author is probably due to different conditions. In the case I worked out, the same average speed was taken for the run both up and down the gradient. The gear ratio was also altered to obtain the speed on the gradient, being 1·95 on the level and 1·25 on the gradient. It would be useful if the author could give the results of any calculations showing the difference in energy consumption, with a train running under its best conditions on the level, and its best conditions up a gradient and down again. It would probably be greater than 6 per cent. On page 450 the author states with reference to curve C (Fig. 8) that "a higher speed gear would result in further increase of energy consumption." By a higher speed gear I presume that a gear ratio nearer to unity is meant. A decrease in gear ratio, in all but very short runs, allows of longer coasting, and this is commonly supposed to reduce the energy consumption. It would be useful if the author could give some information as to the method he adopts in finding the most economical gear ratio for a given schedule. I have found that, within fairly wide limits, the gear ratio makes little difference to the energy consumption.

The following result is a helpful guide in settling the most economical gear ratio. The kinetic energy of a train $= M v^2/2g = E$. Rate of increase of kinetic energy $= dE/dv = M v/g$. Let $V_n =$ speed at which notching ends and $V_b =$ speed at which braking commences. Extra energy to be given to a train during notching, when the notching speed is increased by δV_n, is $\delta V_n \cdot M V_n/g$. If the percentage energy

lost during notching is taken at 50 per cent for direct-current control and $33\frac{1}{3}$ per cent for alternating-current control, which are average values, the extra energy lost during notching by increasing V_n to $V_n + \delta V_n$ is $\delta V_n . M V_n/g$ for direct current and $\delta V_n . MV_n/2g$ for alternating-current control. Decrease in energy lost in the brakes owing to decrease of braking speed from V_b to $V_b - \delta V_b = \delta V_b . M V_b/g$. Assuming that the motor efficiency and the train resistance are unaltered, the energy consumption will be decreased if decrease of energy lost in brakes > increase of energy lost in notching, *i.e.* for direct current if— Mr. Moffet.

$$\delta V_n \frac{M V_n}{g} > \delta V_b \frac{M V_b}{g};$$

that is, if—

$$\delta V_b > \delta V_n \times \frac{V_n}{V_b},$$

or for alternating current if—

$$\delta V_b > \delta V_n \times \frac{V_n}{2 V_b}.$$

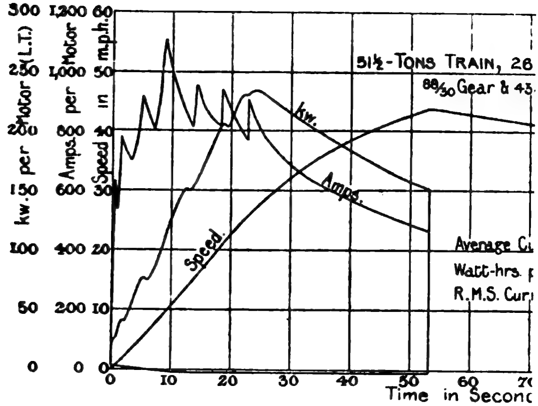
From this it can be seen that if the notching speed is 20 miles per hour and the braking speed 40 miles per hour, a saving in energy consumption will be obtained, if by decreasing the gear ratio so as to increase the notching speed 1 mile per hour, the braking speed is reduced by more than $\frac{1}{2}$ mile per hour with direct-current, or $\frac{1}{4}$ mile per hour for alternating-current control. The increased importance of working with a gear ratio so as to allow of some coasting in alternating-current working is also brought out.

Mr. F. LYDALL: I always find that the proper way to explain a discrepancy is to go right down into the causes of the same and see on what the various results are based. If the energy consumption in two different cases is analysed, it can be divided into several portions, such as losses in the brakes, in overcoming tractive resistance, motor losses, etc. If it can be shown that the various losses are different in the two cases due to different circumstances, then the comparison which is made between the two sets of results is quite useless. This paper seems more or less intended to be a sort of sequel to other papers dealing with the controversy between the alternating- and direct-current systems—at least that is the impression conveyed. It is, of course, quite impossible for a man who believes in the single-phase system to give up his convictions, and it seems to me the difference of opinion must continue. The decision will mainly rest with the authorities of the railways. How is the question to be attacked from their point of view? It appears to me that the only sound method of getting a perfectly satisfactory comparison between the two systems is to get alternative tenders from firms who are capable of carrying out both systems equally well, not only for the first cost of the installation, but also for the running cost, and for a considerable number of equipments or locomotives. I would suggest that railway companies who Mr. Lydall.

Mr. Lydall. have decided to go into the matter should send inquiries along to manufacturers, who would certainly be quite willing to deal with them.

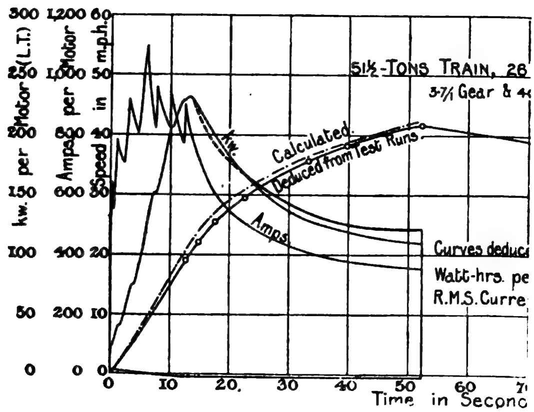
Mr. Cramp. Mr. W. CRAMP: On page 437 there is a statement which is of very great interest. The author says: "In suburban service an exact knowledge of the train resistance is not of primary importance, as free-running speed is practically never reached, and the speeds actually attained are hardly affected within the limits of the practicable variation of train resistance.". Does the author mean that the force required to accelerate the mass is always so much more important than that required to overcome train resistance that the latter may be neglected? The weights given on page 439 are also very interesting. It is there stated that 1 motor coach weighs about 49 tons, 2 trailer coaches at 30 tons each = 60 tons, total 109 tons, and that the average passenger load is only 8 tons. It was hoped that when electrification was accomplished the ratio of passenger load to train load might go up; as a matter of fact that figure does not show any improvement upon ordinary steam trains. 8·5 to 109 still seems an extraordinarily low ratio. Surely with distributed power this might be increased. The meaning of the figures italicized in Table I is not very clear. I refer now to lines 12 and 13. Perhaps the author will explain these. The marking of curves in Figs. 4 and 5 requires explanation. In Fig. 4 it is quite clear that the curve with square corners is a power curve; the arched curve is apparently a velocity curve, of which the third curve presumably is an integral, but there is nothing in the paper to say so.

Mr. Dalziel. Mr. J. DALZIEL: I disclaim any bias towards single-phase such as is alleged by the author to influence my calculated results. Neither on my part nor on that of the company with which I am connected is there any such bias, nor are there any interests to be served to account for such: the sole object of all tests made and calculations founded thereon has been to ascertain facts and obtain reliable information on which to base future action. So far as I am concerned, the author's criticism is concentrated on my calculated consumption of 83·8 high-tension watt-hours per ton-mile for the schedule runs of which particulars are given in the paper, and he disputes the correctness of my figure on the ground of incorrect handling on my part of the calculations involved in connection with the proposed more suitable gear ratio. He asserts that any saving to be thus effected is of an insignificant order, but he acknowledges that the establishment of the correctness of my figures would show the single-phase system to distinct advantage. To take up the author's points, my calculated curves and figures were obtained not from the makers' published characteristic of their motors—though they substantially agree with this—but they were more reliably deduced directly from the curves of the actual road tests with the existing gear ratio, the results of which latter precede the former in the paper. The curves were simply redrawn, due allowance being made for the increased motor purchase and the consequent higher acceleration, and for the increase of rota-



300 1,200 60
250 1,000 50
200 800 40
150 600 30
100 400 20
50 200 10
0 0 0
kw. per Motor (L.T.)
Amps. per Motor
Speed in m.p.h.
51½-Tons Train, 26
88/30 Gear & 43
kw.
Amps.
Speed.
Average Cu
Watt-hrs. p
R.M.S. Curr
0 10 20 30 40 50 60 70
Time in Second

FIG. 11.



300 1,200 60
250 1,000 50
200 800 40
150 600 30
100 400 20
50 200 10
0 0 0
kw. per Motor (L.T.)
Amps. per Motor
Speed in m.p.h.
51½-Tons Train, 26
5·7/1 Gear & 4
kw.
Calculated.
Deduced from Test Runs.
Amps.
Curves deduc
Watt-hrs. pe
R.M.S. Curre
0 10 20 30 40 50 60 70
Time in Second

FIG. 12.

tional inertia, due to higher armature speed. The actual and correct figures for rotational inertia are respectively 9·7 and 11·6 per cent of the train weight. Inspection of the curves in Diagram A will show that the current and watt curves in Fig. 12 are substantially reproductions of those in Fig. 11 to a time-scale compressed in accordance with the increased motor tractive effort per ampere with the resultant higher acceleration. The running on the motor characteristic is, of course, prolonged in the calculated case, but it is the simplest of matters to obtain this part of the diagram. Mr. Carter is no doubt aware of the method of obtaining the diagrams and figures required for a new gear ratio from a running diagram obtained with another ratio, and that though it involves several steps of calculation, it is the most accurate method possible, and absolutely reliable. Not only so, but the tabular data of analysis of the two sets of runs given on pages 188 and 189 of vol. 179 of the *Proceedings of the Institution of Civil Engineers*, and reproduced on the screen, suggest this by the coincidence of the figures for notching efficiency, and they also provide a complete answer to the author's inquiry as to the saving in consumption which is shown by the diagram as being 5·9 watt-hours per ton-mile in braking—the brakes being applied at 35 instead of 37·3 m.p.h.—and 9·6 watt-hours per ton-mile in increased efficiency. The source of this latter can easily be traced. The speed-time curve with the new gear ratio was originally got out not so much with a view to saving consumption as in an endeavour to reduce the root mean square current, the saving in consumption being more or less a by-product. The reduction in the root mean square current, of course, follows from the work being done at a higher mean voltage and mean motor speed, due to the notching periods being more quickly got over, and the run on the motor characteristic being proportionately longer; in other words, it might be said that the specific capacity of the motor is better utilized. When it is remembered that the efficiency of the machine rises practically inversely as the square of the current, considering that the root mean square current is decreased from 445 to 360 amperes the source of the efficiency gain need not be much further sought for. Mr. Dalziel.

It is not the case that with the second diagram there is no margin for making up time; this is practically identical on the two diagrams; neither is the saving only of the order of 3 watt-hours per ton-mile, and it is evident that the author has repeated his Inst.C.E. error of working my motors up to only 320 volts, whereas 340 volts is the correct maximum pressure. For a fuller reply on these points I refer members to the *Proceedings of the Institution of Civil Engineers* previously mentioned. To drive home the correctness of my contentions by actual road test, I will refer to page 184 (Inst.C.E. *Proc.*), giving particulars of a single-phase run of the L. & Y. 30 m.p.h. 1·32 mile 15-second stops schedule. This schedule both the author and Mr. H. M. Hobart have stated to be a more exacting one than the 26-m.p.h. schedule of my tests just referred to; it happens that by reason of the longer distance of run the existing Heysham gear ratio is well suited

Mr. Dalziel. to this schedule. The high-tension single-phase consumption amounts to 88 watt-hours per ton-mile on the average of a number of runs in both directions, reducible with a heavier train probably to 83 or 84 watt-hours, which compares with the L. & Y. consumption stated by Mr. Aspinall to be, on trains with a similar weight per installed horse-power, 96 watt-hours per ton-mile low-tension direct current. In comparing these consumptions it must be borne in mind that behind the direct-current figures up to the power station there lie losses in feeders, sub-stations, etc., of some 19 per cent, as given by Mr. Aspinall; behind the single-phase figure there lies a loss of only some 3 per cent. As a further and final comparative figure of alternating-current and direct-current consumptions, I will instance that of a test by Mr. O'Brien (p. 135, Inst.C.E. *Proceedings*). This was made on my 26-m.p.h. schedule, and specially for quotation against my tests; the direct-current figure was 77 watt-hours per ton-mile, only attained, however, by a braking deceleration of 2·7 m.p.h.p.s.; the consumption with a deceleration of 1·8 m.p.h.p.s. equal to that of the Heysham tests would have been 97 watt-hours per ton-mile. In my paper, and now, I would have instituted further direct-current *versus* alternating-current comparisons based on Mr. Aspinall's L. & Y. records in his 1909 address to the Institution of Mechanical Engineers, but for the fact that a deeper analysis of these records shows them to be unsuitable for comparative purposes without reconstruction to an extent that would have made coincidence with their published form not immediately recognizable. In fact, as suggested in the present paper (page 451), and as pointed out in my reply to the discussion on my paper, it is very easy to misread Mr. Aspinall's figures and curves. More particularly the consumption savings shown as due to increased weight of train in a great measure do not arise actually from this cause. None of Mr. Aspinall's trains above his 3-car train can, in fact, work to his time-table. His 4-car train, for example, takes 151 seconds exclusive of stop to cover his mean distance of 1·32 miles, and therefore fails to keep time to his 30-m.p.h. 15-second stop schedule by 7 seconds per station. The timing of his heavier trains is, of course, increasingly worse. Time can therefore only be kept to the L. & Y. published time-table by stations being missed, and a heavy, full-stopping train must either run late or be timed to an easier schedule than 18½ miles in 37 minutes with 15-second stops.

To speak generally, I do think it is time for this question of railway electrification to be put on a basis to inspire greater confidence among railway people. No one can object to criticism, but I am quite certain that the critics would be employed to better purpose in the interests of their profession in this country in directing themselves to the elimination of those weaknesses of the single-phase system which they are so quick to exaggerate for its condemnation, and to its development here, so as to put the British electrical trade in a better position to compete where single-phase apparatus is required, than in uselessly endeavouring to stem the tide of progress by indiscriminate fault-

finding, thereby assisting to undermine confidence. My view is that that is best which will be of the greatest future benefit to the railways of this country. Mr. Carter himself in a traction article * which I recently came across referred to the continuous-current third-rail system as having complicated sub-stations, large current and excessive loss in low-tension feeders and return, and as being limited in application to crowded districts in the neighbourhood of big towns, and he stated that there is absolutely no prospect of the electric operation of main line railways while such a system is the best that can be offered. In contradistinction to this are placed the advantages of alternate-current working with single-phase alternating motors, transformer starting, high-voltage overhead current collection, alternating transmission, and few sub-stations and feeders. I do not think there is now any engineer who will not admit the preponderating advantages of single-phase for main line working, but there are still some who consider and who state that it is not suitable for urban and suburban work, and who advocate proceeding with such work regardless of main line possibilities. This may be a correct policy in some instances. I have, however, given considerable time to the study of the subject, and I can say that I believe firmly that the coming of main line electric working, particularly on heavy graded lines with dense and particularly dense goods traffic is, provided the prosperity of the country be maintained, only a matter of a very few years. To point this, I have only to say that by means of a single-phase locomotive, not too heavy to pass the requirements of the Engineers' Department, trains of 40 wagons, as against the 29 that can now be taken by the heaviest permissible steam locomotives, could be run on the Midland Railway between Derby and Manchester Ancoats, and that with a reduction in running time from 3 hours 5 minutes to 2 hours 1 minute, a saving of 35 per cent in running time.

Mr. Dalziel.

Because I say this, I do not want any one to conclude that the immediate electrification of the Derby-Manchester Midland line is impending, for it is not. I merely use it as an example of what electrification could accomplish on a heavily graded stretch of railway. What I have endeavoured to do is to show in accordance with the teaching of my own investigations, that while the single-phase system has certain drawbacks in increased weight and cost of rolling stock equipment, on the one hand possible main line electrification cannot with prudence be overlooked, especially on some lines, and on the other hand that even in dense urban passenger traffic work its disadvantages are by no means so outstanding, and it, in fact, has advantages of comparative performance sufficiently great to make its rejection for any work that may be in contemplation a matter for the gravest consideration, and indeed of bad engineering where main line work is possible. While the use of both types of current on one line is possible, and in some cases no doubt may be inevitable, the resulting complications and additions to expense cannot but be most

* *Electrical Review*, vol. 54, p. 868, 1904.

Mr. Dalziel. serious, and have indeed been the cause of practically every set-back to which the use of single-phase apparatus has been subjected. The larger question which has been occasionally raised of late of rolling stock interchangeability between railways, I leave. Prophecy has been made of another "battle of the gauges" resulting over this in the future. I do not anticipate this. The interchange of locomotives between railways is not extensive, and in my opinion motor-cars are not required and not suitable for any but certain types of short distance traffic, where, except in London, interchange need not be practised; they are also expensive luxuries, as compared with locomotives, both to install and to maintain.

Communicated: As arranged in the course of the discussion on Mr. Carter's paper, I beg to submit the following supplementary remarks and diagrams. Diagram A exhibits the speed-time, current, and watt curves of my trial test, and deduced 51½ tons 26 m.p.h. 20-second stop per mile train performances, characteristic speed, tractive effort and current curves of the Siemens motors on the new gear ratio and 340 volts as based on the results obtained during actual trial runs, and notes and calculations *re* the deduced curve and the method of obtaining the same; diagram B is a copy of our diagram of the motor characteristics on the various voltages as already published, but with two curves for 340 volts added. I wish to point out in the first place that the speed curves of A and B, which should correspond, do not exactly do so for two reasons: (1) while B shows the motor performances with steady given pressure applied to their terminals, A shows the actual resultant effect obtained in practice with a power station limited in capacity and rather poor in voltage regulation: inductive effects of power station, line, and transformers are also taken into account; and (2), the commutating transformer tappings made use of at Heysham, as found in actual road running to be the best, were and are not the same as those used during the shop tests on which the characteristics as published were obtained; this alters the power factor and consequently the speeds. As regards (1), reference to some of the other Inst.C.E. curves will show that the overhead line and motor pressures are considerably lower than standard during the periods of heavy draught of current at starting, and rise gradually; and as regards (2), on diagram B, an additional set of curves is plotted in red showing characteristics on 340 volts as obtained from tests made at Heysham with the motors in road condition; these are in accordance with and based on the curve given in the paper by myself and Mr. J. Sayers.*

Owing to the deduced curve being based on the test curve as explained in my spoken remarks, the whole of these variables are taken into account in it, including every limitation of power station, line, high-tension equipment, and the motors themselves; it is, therefore, strictly such as can be with certainty fully attained in practice,

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, plate 4, Fig. 31, 1909–10.

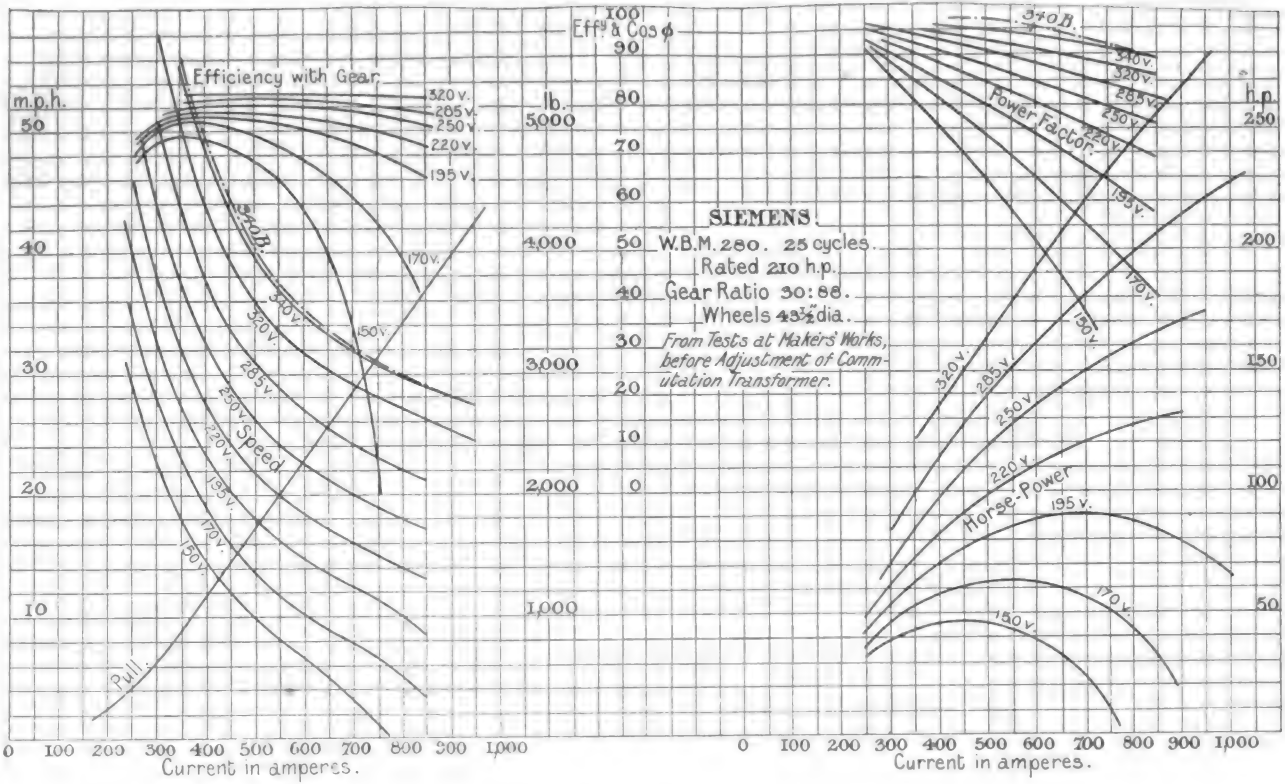


DIAGRAM B.

Curves 340 B were deduced from road tests after adjustment of commutation transformer to most suitable tapping for running conditions (see *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, Plate 4, Fig. 31, 1909–10).

Mr. Dalziel. and I know of no other way by which similar certainty of accuracy could be ensured; the foregoing in fact confirms in every way my contention that my results are much more accurate and reliable than would have been obtained by the point-to-point method using the motor characteristics without any thought of the actual line conditions; such a result as the latter could in fact have been put forward at any time. There is no doubt that with a power station of much greater capacity and better regulation, better results would have been obtained on both curves, but in deducing the second curve special care was taken to avoid any consideration of this, of the probable favourable effects of heavier train weight, or of any factor other than that of better gear ratio.* From the calculated curve which I have dotted in, which has been worked by the point-to-point method from the characteristics and mean currents, it will be seen that in the deduced curve as published ample contingency allowance has been made. Furthermore, this curve would obviously show a better consumption than mine.

The notes in diagram A show fully, I think, how the deduced diagram was obtained; they do not add much to my remarks during the discussion and the cross conversation between Mr. Carter and myself during his reply, and I think after consideration on his part they will probably have made matters clear to him. The first part of the process of deduction was to plot the current-tractive-effort curve on diagram A from the test curve, thereafter the new speed-time, current, and watt curves were obtained as follows: It is obvious that the new speed at the end of notching can be obtained at once from the variation of motor purchase, motor speeds and currents being as before relatively to each other; this fixes the required point as to speed, *i.e.* vertically; then with notching done as before, so exactly repeating the

* It should be clearly understood, however, that the difference between the two sets of curves has never been put forward as typical of the improvement to be effected by change of gear under normal conditions. The power station regulation at Heysham was not intended for a service such as these tests are typical of, and is not good enough for them; nor is the capacity of the station sufficient. The result of this is that on the heavy low power-factor demands during starting, and just after reaching the full speed notch, the voltage is low, rising gradually and considerably to a maximum of somewhat over 350 volts. On the lower voltages the motor efficiency falls off very considerably, more particularly at the higher currents, and it is self-evident that the power-station voltage on a demand such as that of the calculated curve, owing to the shorter duration of persistence of these heavy currents and the generally much lower average current, would be much better maintained and would enable the input to the motors to take place on higher and more efficient voltages than in the case of the road tests. The high average current demands with the existing gear ratio in the case of the latter is in fact one of the special reasons making the difference in conditions abnormal.

It has been made perfectly clear in the Inst.C.E. *Proceedings* and in these remarks that it was with the intention of eliminating these abnormalities and so indicating a figure more really representative of single-phase performance under normal conditions that the calculated curve was got out, and with no intention whatever of making a comparison pure and simple between the effect of two different gear ratios. The correctness of this curve is all that I am concerned with.

Better power station conditions are allowed for only to the above extent in working out the calculated curve; and obviously could similar maintenance of voltage with heavy currents have been ensured during the road tests the results in these cases would have been much better, and less difference in consumption would have been shown between the two gear ratios.

amplitude of the current variations and keeping the mean currents the same, the new acceleration obtained is exactly in accordance with the new gear ratio less additional inertia. The position of the point is then also fixed as to time, *i.e.* horizontally, and so definitely, by the seconds taken to reach the new speed with the new acceleration. The speed of notching up will obviously be faster, and the total time taken to put the motors on full voltage less, in proportion to the higher acceleration. This, I think, clears up Mr. Cunliffe's difficulty. If it is desired to check this by the point-to-point method, this can be done by finding the new accelerations from the same mean currents as before; this will be facilitated by the knowledge from the test curve of the train speeds at the beginning and end of working on each notch, which as already shown must bear to the test curve speeds the relationship of the old motor purchase to the new. No substantial difference will be found, and such as there is will be in the direction of improved working and lower consumption than shown on my diagram, which with a margin on the conservative side corresponds both to the test curve and to physical considerations. The new curve follows the characteristic on the end portion where the current and watt curves fall below, and the motor speed is above that of the test curve, this portion of the characteristic having been checked by special test runs. It may be noted in passing that the voltages in actual use are not those given by the curves in diagram B. Mr. Dalziel.

I cannot in any way agree with Mr. Carter's figures for my curves with the amended gear ratio. A general inspection of his diagram and figures suggests that his error lies in only working the motors up to 320 volts, but as I specifically stated during the discussion, I have never attempted to verify either these or his consumption figures, and I refuse to associate myself in any way with his table. With regard to Mr. Carter's contention that my two curves involved are inconsistent with one another, I have to reply that, as Mr. Carter himself now points out, the Inst.C.E. and the *Railway Gazette* curves are different. The Inst.C.E. curves were later, and were perfectly consistent with each other. It is an elementary fact that the power and armature speed are fixed for a given current and voltage, but this is exactly what I pointed out, both in my spoken reply to Mr. Carter and in the cross discussion between us during his reply, as being the basis for my assertion of the correctness of my deduced curve, though I could not get him to admit it. The reasons why the *Railway Gazette* and the Inst.C.E. curves differ are set out in the letterpress respectively accompanying each curve. It is stated in the *Railway Gazette* that the first or test curve is a diagram of a single run, while the deduced curve is based on the average of a number of runs. The Inst.C.E. curves are in both cases curves representing a large number of runs on different days and in both directions. In the case of the *Railway Gazette* curves a cursory examination will show that the first diagram has a steeper brake line and takes one second longer to complete the journey than standard. As for the difference in speed by which Mr. Carter endeavours to make a point,

Mr. Dalziel. this difference represents only some 5 per cent, and on a single run such as this is easily accounted for by the voltage of the power station being low. Reference to the characteristic will show that the speeds are lower than in accordance with it. There is therefore no peculiarity either in changing the curves or in insisting that they are correct. The *Railway Gazette* figures are correct, and they are worked on the averages, but as already stated the diagram given was absolutely that of a single run exactly as carried out.

Finally, in respect to Mr. Carter's reference to the remarks of two other speakers, Mr. Cunliffe rather supported the amended curves than otherwise, and I have pointed out already, and do so more fully below, where both he and Mr. Carter were under a misapprehension with respect to the speed of notching, while in respect to suitability of gear ratio Mr. Carter rightly points out that the most economical reduction is that giving the lowest speed capable of performing the service with sufficient margin, which is exactly my own view and that which I followed out in making my amended diagram, the Heysham gear ratio being as it existed suitable for long runs as proved by its satisfactory results on the Lancashire and Yorkshire 1·32-mile stops schedule. As I pointed out in the reply which the Chairman kindly permitted me to make to Mr. Cunliffe, in the calculations which the latter put forward the effect of alteration in gear is not taken full account of. It is perfectly true that higher acceleration entails a greater maximum demand; it is also true that the maximum demand on my two diagrams is the same. Mr. Cunliffe will be correct if he adds the words "at any given speed" to his remarks; analysis of my diagrams will show that the maximum demand at any given speed is higher in the second case; this of course only holds during the notching period, after which acceleration falls off.

With respect to the paragraph at the bottom of page 436, this should be made clearer, though I have no doubt Mr. Cunliffe is quite clear on the matter himself. As it stands, the statements that the power inputs are similar and that the time saving represents a saving in energy are contradictory. The times taken to operate the controller up to putting the motor on 340 volts are respectively 23 and 12½ seconds; while it is true that the average rate of power input is the same in both cases, there is a saving in total power input proportional to the saving in time. The actual main power input, viz. that for acceleration, varies as the square of the speeds at the end of notching. The motor armature speed is, of course, the same at any particular current, so the speed at the end of notching must be exactly in proportion to the new purchase; the increased acceleration is also in proportion to this, and the latter divided into the former, neglecting rotary inertia, gives the new time on notching, *i.e.* the old time of notching is to the new as the square of the alteration in purchase. The accelerations in the second diagram are not quite up to what might be expected, but as I explained this is due partly to increased rotary inertia due to higher armature speeds and partly to my having dealt conservatively with the deduced curve. The remarks as to

the peaks on the current curve are quite wrong. The notching during the test runs was such as to supply the motors on the peaks with as much current as they could handle; the peaks at the higher voltages were intentionally somewhat reduced as compared with the lower voltages, partly to keep down the maximum demand on the small power station, and partly to keep down the feed to the motors at the higher notches when the commutating voltage is less. The amplitude of such peaks is not merely a function of the rapidity of notching, but is a question of the particular speed of the armature, and therefore of the current passing to it at the time of notching up, of the voltage increment on the particular notch, and of the ohmic resistance and induction of the circuits concerned. There is an ammeter in the driving compartment of all the Heysham coaches. The rapidity with which current falls off and with which notching can be done depends entirely on the acceleration and the speeds to be attained on the respective notches; the remarks already made as to the difference in total period of notching and generally, clearly explain both why the current comes down so rapidly in the second diagram, and also why no improvement due to more rapid notching could be effected in the first. Furthermore, it will also show that the controller was designed to be used in exactly the same way in the second figure as in the first, *i.e.* to the safe limit of the capacity of the motors in ordinary work, and that the work the motors were called upon to handle, so far as maximums were concerned, was exactly the same in both cases, the improvement in the second case being one of total and not of maximum. I agree with Mr. Cunliffe as to the advantage in consumptions to be gained by high accelerations, though I think it would require a very large system or a very sluggish demand indicator to establish as a law the equation put forward in his concluding paragraph. He will, however, agree, I think, that the tramway conditions are rather different from mine at Heysham, and will see that I took out of my motors the full possible acceleration they could be expected to give in continuous service. This, in fact, was the only way in which results could be given on a comparable basis. Mr. Dalziel.

Mr. J. SAYERS (*communicated*): Personally I do not think either the extension of electrification of railways or their profitableness when electrified will rest very much with the so-called efficiency of the system. At the time of writing the joint paper by Mr. Dalziel and myself my own position was that for an experiment which was made simply to test the conditions for main-line work high collection pressure was essential, and at that time there was only one system available which could use a high pressure delivered to the car; but I pointed out even then that, owing to other troubles, if a continuous-current 3,000-volt motor came along it would, in my opinion, sweep the field. The recent successes of the American 3,000-volt continuous-current system are, I think, the most important new facts in electric traction. It seems impossible in this country to get any one to consider seriously the enormous importance of the telegraph and telephone interference question. I suppose it is because so very few people at present have Mr. Sayers.

Mr. Sayers. had any practical experience with a single-phase railway in this country; the adoption of continuous current as against alternating would very much simplify this important part of the question, especially so long as the Postmaster-General is allowed to insert in every Act mentioning electrification his present type of protective clauses. Between rival systems 2 or 3 per cent in power consumption is, in my opinion, nothing compared with difference in reliability, elasticity in output, and adaptability to the conditions of the older roads.

Mr. Livingstone. Mr. R. LIVINGSTONE (*communicated*): The author's paper deals with the subject in such a clear way that only very minor points are left for discussion. Problems such as the power required for the driving of trains or the operation of coal and ore winders lend themselves to treatment by simple mechanical methods so readily that no disagreement is found between the calculated results and the actual results, provided that the calculations take account of all forces which would influence the results to any appreciable extent. In all such problems the acceleration of mass is of such great importance compared with the frictional resistance that a small error in the estimation of friction and windage is of no great moment. This is especially the case where the time for a cycle of operations is short. In long-distance runs, however, the friction and windage become of greater relative importance, and one would expect in such cases the calculated results to be further out than in cases where the runs are shorter. In all cases, however, the accurate calculation of all resistances to be overcome should be made in the same way as the author has done, and I wish to support his contention that for short runs the actual results should be the same as the calculated. This I have found to be the case in such equipments as electric winders.

Mr. Dawson. Mr. PHILIP DAWSON (*communicated*): The author has made a comparison with the South London single-phase service by imagining a direct-current equipment placed on the actual trains, and calculating the consumption for a run on the level for the average distance between the South London stations. We are asked to accept a figure of 59·4 watt-hours per ton-mile for his train of 118 tons. The overall efficiency of the equipment for the specified run, although entirely omitted from the paper, can be deduced from columns 3, 10, and 13, as given in Table I., and on the same basis of calculation as adopted in the other columns, I find this to be 77 per cent. Now, as a result of actual tests on direct-current equipments, it has been admitted (see Hobart, "Electric Trains," page 75) that where trains operate at high schedule speeds with frequent stops, the overall efficiency of the electrical equipment on the train should in actual practice, and for correctly driven and correctly designed trains and equipments, be of the order of 70 per cent. For a distance apart of stations of 1 mile, and at a schedule speed of 25 miles per hour, which fairly corresponds to the author's run, the efficiency is specifically stated by Mr. Hobart to be 70 per cent. For a 3-coach train on the Lancashire and Yorkshire line the overall efficiency is said by Mr. Aspinall to be 72·6 per

cent. Mr. Hobart has published the results of tests made on the Great Northern, Piccadilly, and Brompton Railway, where the equipment taken by Mr. Carter is in use; the overall efficiency is stated to be 69 per cent for schedule speeds of 17·5 miles per hour, the average distance between stations being about half a mile. Mr. R. T. Smith gives the overall efficiency for direct-current equipments on the Great Western and Metropolitan as 68 per cent. The efficiency will doubtless vary according to the conditions of the service, but judging from the many results of actual test runs, I am of opinion that Mr. Carter's calculated figure of 59·4 watt-hours per ton-mile as the consumption, with an overall efficiency of 77 per cent for the electrical equipment, is totally incomparable with all other published results of tests on direct-current equipments, and does not represent any practical condition of working, even on a level line, and if the equipments, as proposed by Mr. Carter, were put to an actual test on the South London line, the efficiency of the equipment could not exceed 70 per cent; in fact, owing to the large number of gradients on which acceleration has to take place, I think that it is more probable that this figure would not be reached, having regard to the considerable losses in the rheostats with the direct-current equipment. The author gives the following figures as representing the calculated performances of the direct-current equipment chosen by him on the South London line, between Clapham Road and East Brixton :— Mr. Dawson.

Total Consumption for Single-run Direct Current—

Clapham Road to East Brixton	...	9,200 watt-hours.
East Brixton to Clapham Road	...	6,500 „

Now, the original South London motor was admittedly heavy for its output, and since its introduction, a motor has been used on the London, Brighton and South Coast Railway service, whose weight per horse-power has been so reduced, that the total electrical equipment is now 20 per cent lighter than the original South London equipment. With this later equipment the consumption for alternating-current working on the previous mentioned section is :—

Clapham Road to East Brixton	...	9,400 watt-hours.
East Brixton to Clapham Road	...	7,560 „
Alternating-current total	...	16,960 „
Direct-current total ...	...	15,700 „

The great gain with single-phase operation is, however, clearly defined if the total consumption at the generating station is calculated, and it can be shown that the direct-current system takes about 10 per cent more energy as units consumed from generators than with the alternating-current system. This is due to the fact that the losses which occur in the conversion from alternating current to direct current are entirely absent in the single-phase case, and that the loss in transmitting

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the power to the trains at the converted direct-current pressure is so much greater in the direct-current case than in the extra-high-tension alternating-current system. So far as published results show, the highest efficiency attainable from the alternating-current busbars at the power station to the curcuit-breakers on the train is 81 per cent for normal direct-current working. About 2½ per cent of this loss occurs in E.H.T. transmission to sub-stations, and 8 per cent from sub-stations to trains. The overall efficiency in the alternating-current case for the South London service is not less than 95 per cent. If the figures for consumption on the train are converted to power station consumption, using the above efficiencies to perform the double run between East Brixton and Clapham Road, the consumption at the generator is :—

Alternating-current working	17,850 watt-hours.
Direct-current working	19,400 "
Difference	1,550 "
Saving in energy consumption with alternating-current working ...	8·7 per cent.

Thus a considerable economy in alternating-current working is shown, even accepting the author's figures for consumption for the direct-current equipment, which, if the calculations are comparable with his previously calculated figure, cannot be considered as over-estimated, and are in all probability the reverse.

Dealing now with the performance of a South London train on the Liverpool–Southport service, I must thank the author for again drawing special attention to some figures which I gave in this connection, as I find on checking through the calculations again and re-drawing the curves, that although it is not dynamically impossible to run the journey in the time stated, an error was made originally in the measurements of area by planimeter; this necessitates a corresponding adjustment of my figures for energy consumption, and the latter should now be re-stated as 87 watt-hours per ton-mile, employing the braking retardation in regular use on the South London service. If, however, the exact schedule of the Liverpool–Southport service is repeated, the consumption of the train is 100 watt-hours per ton-mile with the original South-London equipment (*i.e.* not with the lighter equipments as now in use), as compared with 96 watt-hours given by Mr. Aspinall, and 110 watt-hours per ton-mile as calculated by the author for the best performance of the single-phase equipment. With regard to the latter estimate the author's analysis of this run actually credits the output of the motors with 2 watt-hours for every pound of train friction over the *whole* run, whereas the train friction portion of the output should only be taken into account to the point of cut-off; the kinetic energy of the train at cut-off is included separately in the output, and part of this energy is used in overcoming train friction for the remainder of the run. By the author's method this energy is therefore credited to the output in two places. In the curve shown in Fig. 8 of the author's

paper it is true that the error under this head only amounts to about 2 to 3 watt-hours, but the error is nevertheless there. Again, the author has swollen the figure for alternating-current consumption by taking an average of 14 lb. per ton for train resistance. Now, Mr. Aspinall has stated that the resistance of a 3-coach train on the Liverpool–Southport service was 12 lb. per ton at 40 miles per hour; the average speed of the run considered is 33 miles per hour, and therefore the average resistance of 12 lb. per ton which I took for this service to allow for the proportionately greater resistance at the maximum speed (45 miles per hour) should be ample. The author also attacks the records obtained in a non-stop run from London Bridge to Victoria, published in a paper which I read before the Institution of Civil Engineers last year. The result given was not deduced theoretically, but was the result of a test made under ordinary conditions of working, and without any specially favourable circumstances. For my own part I should decidedly prefer to base calculations on actual records of electrical energy consumed, as measured by instruments at the point of supply and checked again by further instruments at the power station from which my figures were taken, than to base them on a pure assumption as regards train resistance, and this to be estimated as an average to the point of cut-off of power, a factor admittedly most difficult to estimate, and susceptible to a large percentage of error. As Mr. Aspinall has already pointed out, very few reliable data are available as regards train resistance with electric multiple-unit trains. With regard to the instruments used on this run, the one at the point of supply was especially calibrated and installed by the British Thomson-Houston Company for the purpose of the test, and with a range chosen for the work in hand. The recording instruments at Deptford were only used as a check, so that the author's assertion that I have relied on instruments which were useless for the purpose has no foundation in fact. Mr. Dawson.

It is true that the author refers to some tests of mine and selects from these for his purpose the highest resistances recorded, after assuming an equivalent average speed which I may say, after examination of the speed-time records, is decidedly on the high side. He has, however, overlooked the fact that the tests which I made to determine the train resistance on the South London line consisted of very short coasting runs at varying speeds. In this connection I should like to point out that Mr. Hobart, who has devoted a good deal of attention to this subject, and may be considered an authority, makes the following statement in his book on electric trains: "It appears, however, conclusive that when over a level and well-built permanent way a train is run at a constant speed the train friction is very much less than when the train is driven at varying speeds, the mean value of which is numerically equal to this constant speed." The speed during the test run, which Mr. Carter criticizes, was fairly uniform from the end of the period of acceleration to the commencement of the period of braking, and if there is any truth in Mr. Hobart's remarks it

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would certainly apply to this particular case. Mr. Hobart also presents curves of tractive resistance, and according to them the train resistance at a speed of 40 miles per hour as taken from these is about 10½ lb., and 14 lb. at 50 miles per hour. Mr. Aspinall gives a higher resistance for his Liverpool and Southport service, viz. 12 lb. per ton at 40 miles per hour. Mr. Carter chooses the abnormal figure of 16 lb. per ton, and on the pure assumption of train resistance, which has every evidence of being considerably over-estimated, proves his case.

Another instance of Mr. Carter's methods in analysing my figures is his allowance of 5 watt-hours per ton-mile as equivalent output due to energy dissipated in the brakes representing momentum given to train up to point of braking. This allowance assumes no coasting, but it is entirely contrary to the actual record of the run in which actual braking commenced at a speed of 20 miles per hour, which reduces the watt-hours per mile for this purpose to 1·45. The total work done against resistance amounts to 24 watt-hours per ton-mile. Hence the dynamic output of motors = 25·45 watt-hours per ton-mile. The input was given in my paper before the Institution of Civil Engineers as = 32 watt-hours per ton-mile, which represents an efficiency for the run of 79 per cent. For the various pre-mentioned reasons I must therefore entirely disagree with his suggestion that the results I published represent a violation of the principles of conservation of energy. In the author's endeavour to elucidate the difference between alternating-current and direct-current working by calculating the consumption for the South London service with direct-current equipments, one half of the problem has been left in complete obscurity. If this be brought to account as shown in the above remarks, in many cases the advantage of single-phase working even for suburban services is conclusively demonstrated. This advantage was hardly expected in the early stages, as the system was mainly intended for long-distance lines to which the suburban lines were more or less incidental. The recent decision of the Prussian Government to electrify the whole of the Berlin Stadtbahn with a train frequency of forty trains per hour as a purely suburban line on the single-phase system shows that their railway authorities do not share the author's opinions.

Mr. Carter.

Mr. F. W. Carter (*in reply*): In the first place I wish to point out that this paper has no direct connection with the controversy concerning the merits of different systems of electrification. The comparisons suggested in the paper will be found in every case to be between trains operated on the same system, and the various arguments to have no reference to system at all. I have in fact, both here and elsewhere,* endeavoured particularly to avoid raising the question of which is the most satisfactory system, realizing that the elementary mechanics of train movement were but imperfectly apprehended, and that no discussion comparing the various systems could be of any value unless grounded on sound mechanics. It appears necessary to mention this, as some

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, p. 126, 1910–11.

contributors to the discussion have introduced extraneous matters, which in the interest of clearness I shall leave without comment. Mr. Carter.

I can quite endorse all Mr. O'Brien says concerning the difficulty of getting reliable train-resistance results for electric trains. The coasting method gives results which require correction for motor and gear friction as well as for rotational inertia, and this somewhat militates against its accuracy, as the correction is in some respects indeterminable. Another method, which is sometimes practicable and gives the train resistance directly, is to run the train with very few motors, and geared so that the motors operate at a fair load when the train is running at constant speed. There are two advantages in this: in the first place, steady running is very quickly reached with well-loaded motors; and secondly, the tractive effort exerted can be determined with considerable accuracy from the current taken under these conditions. Mr. O'Brien has explained the discrepancy that I pointed out in Mr. Aspinall's presidential address, by stating that the 3-coach and 6-coach trains run to different schedules. I had suspected this, and am glad to have it confirmed. The matter is useful as showing the danger of such figures unless employed with full knowledge of the circumstances. Mr. O'Brien's statement that 70 per cent of the electric train mileage is run over lines where the average distance between stations is only about one mile, is another instance, and a very important one, where fuller knowledge clears up a seeming anomaly. Many comparisons have been made with figures given by Mr. Aspinall and Mr. O'Brien, based on the assumption that the average distance between train stops is 1·32 miles.

Before dealing with Mr. Dalziel's remarks, it may be advisable to discuss briefly the general effect of change of gear. Both Mr. O'Brien and Mr. Moffet have mentioned that such change has in practice but little effect on energy consumption for a given schedule, and in fact Mr. Dalziel in claiming an enormous saving is going contrary to a well-established tradition, which happens also to be well founded on experience. In all systems of operation the period of initial acceleration is comparatively inefficient, and a certain saving is to be expected from shortening this period by the use of lower-speed gears. On the other hand, with such gears power must usually be kept on for a longer time in order to run to the schedule; and in extreme cases coasting will be so cut down that brakes have to be applied at a higher speed than when the higher-speed gear is employed. Moreover, the rotational inertia of the armature being increased by the use of lower-speed gears, the kinetic energy of the train at a given speed will also be increased, and with it the input. The foregoing points are generally understood; but there is a further cause, which is not generally apprehended, tending to increase the input when the gear reduction is excessive. This arises from the fact that the high armature speeds incidental to the use of low-speed gears and wheels lead to excessive friction losses and correspondingly low efficiency between terminals and driving wheels. Careful tests have shown that the gear,

Mr. Carter. brush, and bearing friction, and the windage and vibration losses, increase rapidly at high speeds; and as high speed means low power in these motors, the friction losses as a percentage of the input become very great.

Fig. 15, which gives actual results of tests of gear and friction losses

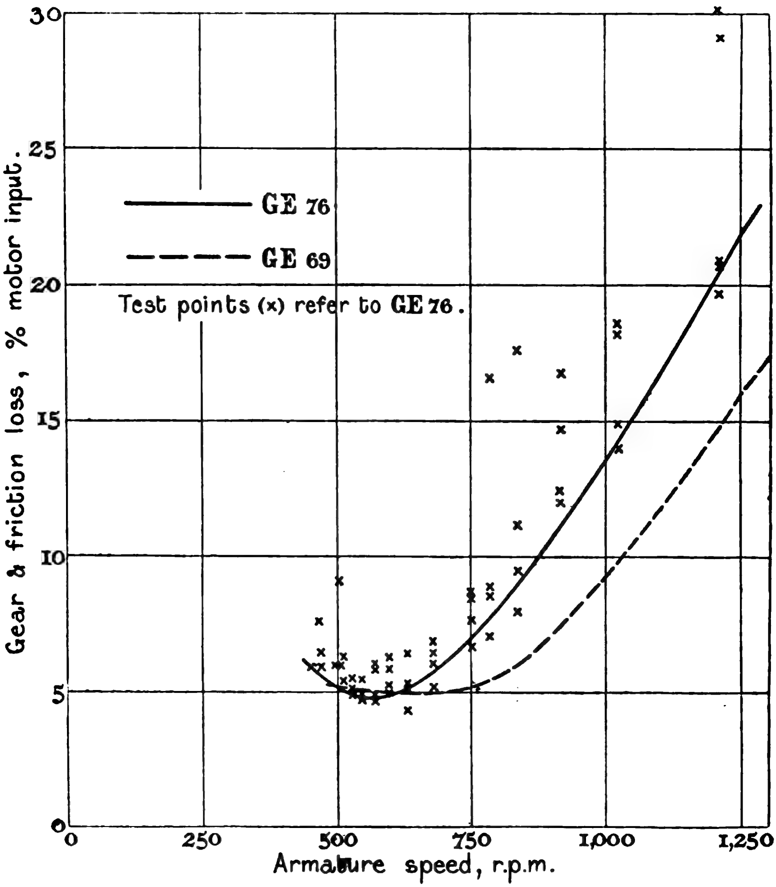

FIG. 15.

on 150-h.p. railway motors—results that in fact were used in deducing the characteristic curves of the motor in question—will serve to show this. On the same figure is shown dotted the corresponding curve for the 200-h.p. motor, whose speed and tractive-effort curves are given in Fig. 1, and whose efficiency can be easily deduced from the input and output there shown. I have given the test points of an actual case in

Mr. Carter.

order to draw attention to the indefiniteness of the quantity with which we are concerned. It will be seen from these tests, however, that the gear and friction losses, as a percentage of the input, are comparatively constant from low speeds to 700 or 800 revs. per minute, after which they increase rapidly.

There is no standard way of presenting the characteristic curves of railway motors, and the more usual practice is not to take the gear and friction loss from actual test, but to assume it to be a constant percentage of the input. This convention has the very desirable quality of definiteness, whilst being sufficiently correct over the useful portion of the curve, and thus leading to no appreciable error in normal practice. It will now be seen, however, that in finding the most suitable gears the use of such a convention may, if high armature speeds are attained, lead on paper to a saving which has no counterpart in reality. There is usually no difficulty in determining whether the friction losses have been correctly included, for the rapid drop in efficiency at the high-speed end of the efficiency curve is a clear indication. I have little hesitation in stating that the efficiency curves shown in Mr. Dalziel's Diagram B do not include the correct friction, but are based on an assumed friction taken as a constant percentage of the input. They should be dropped considerably at the high-speed end to be in accordance with reality. I do not suggest this as the main cause of the difference between our results; it is merely an important secondary cause, which is without bearing on my general contention, and which I shall not take into account in the first instance.

Returning now to Mr. Dalziel's remarks, I would point out first that the issue between us is not as to the correctness of a particular figure, but as to whether certain two figures are comparable, for the absolute values of the figures obtained from any calculation depend on the assumptions made. In the following I intend to adopt all Mr. Dalziel's assumptions; and though I do not happen to have checked his calculated result, if I had done so it would not have proved his case, unless the application of precisely the same calculation with the same assumptions to the run with high-speed gears had led to his test result, for only then could the assumptions be regarded as justified for the purpose of calculation. My sole contention is that the difference between the figures given by Mr. Dalziel for energy consumption with the two gears is excessive,* and not that a particular figure is incorrect.

Mr. Dalziel's contention that our disagreement is due to an error in my assumed voltage will not bear investigation, since the voltage affects

* Mr. Dalziel seems to suggest in a footnote that his calculated curve was intended not only to allow for change of gear but also to eliminate abnormal features of his system, and particularly the vagaries of an inadequate power station. The footnote refers to a paragraph which states that he has excluded from his comparison all considerations other than those of gear ratio. This is precisely the basis on which my comparison has been made, and it is clearly also the basis of Mr. Dalziel's computed curves. I do not consider the matter of any importance from a technical point of view, however, as I fail to see how the power station can have much effect on the matter at issue, the variation of efficiency with voltage being small.

Mr. Carter. both runs alike. I may point out that there was never any misapprehension on my part as to the voltage used in his tests, which is shown on a number of his own curves,* and mentioned by me in a footnote ; † and furthermore that his contention amounts to a claim of consistency for the following table :—

Watt-hours per ton mile (L.T.).	Dalziel (at 340 volts).	Carter (at 320 volts).
With 88/30 gear, 43½-in. wheels... ...	92·8	91·5
With 3·7 gear, 40-in. wheels	77·5	88·8
Difference	15·3	2·7

I am not acquainted with any reliable method for deducing one train characteristic from another. It is true that if the train resistance were zero, or would conveniently vary in a way that it certainly does not, it would be easy to deduce a train characteristic for a different gear by reducing the speed in the ratio of gear reduction (1·37 to 1 in the present instance), and reducing the time in the square of this ratio, suitably corrected for change in effective weight (1·85 to 1 in present instance), but I do not think this method will assist us here, and it anyhow could not have led to Mr. Dalziel's result, as it would have given instead too high a figure for energy consumption.

The information, however, that Mr. Dalziel has kindly furnished, and particularly that given in Diagrams A and B, provides all the material necessary to explain our difference. In Fig. 13 he has given a table showing how his computed curve was arrived at. I have nothing whatever to say against this calculation, which I shall adopt in its entirety as the basis of my own. There is, however, one particular in which the information supplied by Mr. Dalziel is deficient. He gives everything required to determine the output of his motor, but no proper connection between output and input, for he gives no efficiency curve corresponding to running conditions. However, since the motor losses are not sensibly affected by changes in auxiliary apparatus it is possible to determine an inferior limit of input. The characteristic curves of the motor show a maximum efficiency at 285 volts of 81½ per cent, and at 320 volts of 82½ per cent (Diagram B). If, therefore, I assume a maximum efficiency of 84 per cent under conditions of actual running, I may justly claim to have adopted a higher figure than is likely to be realized in service. If, further, I assume the same efficiency of 84 per cent, not only as a maximum, but throughout the whole range of

* *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, Plate 4, Figs. 21 to 30, 1909–10.

† Ibid., vol. 179, p 200, 1909–10.

Mr. Carter.

TABLE A.

25¾ Tons per Motor. Addition for Rotational Inertia, 11·6 per cent; 3·7 to 1 Gear Ratio; 40-in. Wheels.

Amperes.	Speed, m.p.h.	Mean Tractive Effort.	Train Resistance.	Mean Accelerated Tractive Effort.	Increment of Speed.	Increment of Time.	Time in Seconds.	Mean Speed.	Increment of Distance, feet.	Power (L.T.), kw.	Increment of Energy, watt-hours.
	0						0				
900	19·0					12·5	12·50	9·50	174	130·0	452
700	22·4	4,650	230	4,420	3·4	2·26	14·76	20·70	69	228·0	143
600	25·5	3,500	244	3,256	3·1	2·80	17·56	23·95	98	198·5	154
500	29·5	2,750	287	2,463	4·0	4·77	22·33	27·50	192	179·0	237
400	35·5	2,000	360	1,640	6·0	10·75	33·08	32·50	512	154·0	460
375	38·0	1,500	417	1,083	2·5	6·77	39·85	36·75	365	130·5	246
350	41·5	1,300	467	833	3·5	12·35	52·20	39·75	720	122·5	421
	41·6					0·30	52·50	41·55	18	121·0	10
	Coasting 34·75	retardation, 0·145 m.p.h. per second.				47·20	99·7	38·17	2,642		
	Braking 0	retardation, 1·8 m.p.h. per second.				19·30	119·0	17·38	492		
									5,282		2,123

2,123 watt-hours per motor-mile; 82·5 watt-hours per ton-mile.

Mr. Carter. speed-curve running, I may claim to have favoured the calculation very considerably in the matter of input, particularly as the armature speed reaches 1,280 revs. per minute with the low-speed gear; and, as I shall show, I should be quite justified in dropping the efficiency to the neighbourhood of 75 per cent at this speed. Concerning the input during acceleration, I find the mean power during the first 23 seconds of Fig. 11, or the first 12·5 seconds of Fig. 12, to be 130 kw., and I am willing to adopt this figure from Mr. Dalziel in order to reduce the points at issue. I shall, of course, adopt the same rates of coasting and braking retardation as shown in Mr. Dalziel's curves. Table A then merely completes Mr. Dalziel's calculation, the mean power being derived by converting the product of the mean speed and mean tractive effort into kilowatt input at 84 per cent efficiency.

Fig. 16, curves A and C, shows these results. It will be seen that whilst my speed curve A agrees with the corresponding curve of

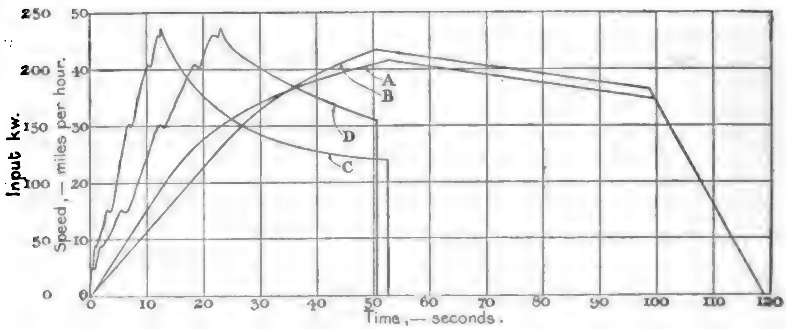

FIG. 16.

Curve A, Speed with 3·7 gear and 40-in. wheels.
" C, Power with 3·7 gear and 40-in. wheels.
" B, Speed with 88·30 gear and 43½-in. wheels.
" D, Power with 88·30 gear and 43½-in. wheels.

Mr. Dalziel's Fig. 12, my power curve C, although purposely calculated too low, is generally higher than Mr. Dalziel's. The energy input for the run is certainly greater than 2·12 kilowatt-hours, or 82·5 watt-hours per ton-mile (L.T.), although the corresponding figure given by Mr. Dalziel is only 77·5 watt-hours per ton-mile. Since Mr. Dalziel and I are quite agreed as to the speed-time curve it should not be difficult to discover the source of our difference in energy consumption. The following Table (B) shows that part at least is due to Mr. Dalziel not having allowed enough energy for the work that has to be done, and in fact that his motor efficiency actually rises greatly at the high-speed end of the curve.

It should be particularly noted that I have not supplied a single figure of the data which leads to the efficiency given on the sixth line

Mr. Carter.

TABLE B.

Mean current, Fig. 13...	800·0	650·00	550·0	450·0	387·50	362·50
Mean speed, Fig. 13	20·7	23·95	27·5	32·5	36·75	39·75
Mean tractive effort, Fig. 13	4,650·0	3,500·00	2,750·0	2,000·0	1,500·00	1,300·00
Power output, in kw....	191·0	167·00	150·0	130·0	110·00	103·00
Power input, Fig. 12, in kw. ...	230·0	209·00	185·0	146·0	123·00	113·00
Mr. Dalziel's motor efficiency from input and output	83·1	80·00	81·2	88·5	89·50	91·20
Efficiency assumed in Table A ...	84·0	84·00	84·0	84·0	84·00	84·00
Suggested reasonable efficiency ...	83·0	83·00	82·5	80·5	77·00	75·50

of the above Table—all are Mr. Dalziel's. It will be seen, however, that as soon as the point is passed where Mr. Dalziel can compare the computed curve of Fig. 12 with the test curve of Fig. 11, the implied motor efficiency immediately rises unreasonably; and, as will be seen from Table A, more than one-half of the energy is put into the motors in this region, whilst nearly one-half of the remainder is put in during the acceleration period, and is not in dispute. It is not a mere mistake in plotting the curves that is responsible for this result, for the error is in the wrong direction. The area of Mr. Dalziel's calculated power curve of Fig. 12, excluding commutating transformers, represents 2,045 watt-hours, or 79·5 watt-hours per ton-mile: this cannot be brought to 77·5 watt-hours per ton-mile by increasing the ordinates.*

* Mr. Dalziel, whose attention has been drawn to the matter, makes the following comments, which he wishes to have recorded :—

"I agree that my efficiencies per Table B are too high, and that the watt curve of my calculated diagram is too low. For this the explanation is that while in the Inst.C.E. curves for the work the watt curve there shown is about right, the train resistance work was only approximated, and is shown too low; while in Diagram A the proper train resistance work or rather more is shown, but, due to an error in checking, the watt curve practically follows the Inst.C.E. one. In the latter, train resistance was averaged as 12 lb. per ton throughout characteristic running, a figure too low for the total train resistance work in view of the long-distance run at high speed. While in Fig. 13 this was corrected, an error in the kilowatt curve was made, in that in reading off the kilowatts from the current inputs the fact that the voltage rose over 340 at the high loads was not allowed for."

"As regards correct efficiency figures, the test-bed results of the motors showed the efficiency of the machines to be well over the calculated figures in the diagrams; without gear on 340 volts an efficiency of 88 to 90 per cent is shown over a wide range. The gear in use is particularly good, showing practically no wear to this day, and the loss with it certainly does not exceed 2 to 3 per cent. It should also be noted that, compared with Diagram B, the power factor actually obtained on low currents is high, this partly accounting for increased efficiency; also, as already mentioned, the effect of voltage variation during running is to make the efficiency on the heavier currents low, and on the smaller currents high."

"A figure for efficiency on 340 to 355 volts that is reasonable and correct, and in accordance with the result of the various tests on which the redrawn characteristics are based, is 86 per cent, including gear losses, at the highest point of the efficiency curve; and this is well maintained. Redrawing the watt curve on this basis a consumption of 80 watt-hours per ton-mile low tension, and 86½ watt-hours high tension, is shown; figures not materially different from those originally put forward."

"The efficiency I have used at the higher speeds is less than the figure quoted above; but on account of the maintenance of input I cannot agree that the rise in the percentage of these losses is so considerable as Mr. Carter suggests."

Mr. Carter. The efficiency figures I should be disposed to adopt for this motor, on the evidence presented and my own experience of these matters, are somewhat as shown in the last line of Table B. These, which are offered as representative figures for average service, and not as the artificially favourable figures of the test bed, were obtained in the following manner:—

Starting with the efficiency curves of Mr. Dalziel's Diagram B, which are doubtless derived from tests by Messrs. Siemens, and are quite as high as they would be prepared to endorse assuming the motors hot, I have taken the slightly higher figure of 83 per cent (to make some allowance for the higher voltage) over the portion of the curve that comes into normal use, and I have lowered the high-speed end, which Mr. Dalziel purposes to use, in general accordance with Fig. 15. This does not mean that the same allowance is made for gear and friction loss as is shown on that figure, for the variation of power is much smaller in the single-phase than in the continuous-current motor.* Consider, for instance, the highest speed shown on Fig. 15: the GE 69 motor has about 18 per cent loss, and the GE 76 motor about 24 per cent. I have chosen the higher figure, since the motor to which our calculation is applied has greater commutator and armature diameters, and much greater brush surface than either of those of Fig. 15; moreover, the figure of 24 per cent is taken from a somewhat optimistically drawn curve, and my intention is here to obtain a representative figure to correspond with service conditions. Now of the 24 per cent something like 6 per cent is gear loss at this speed; and this remains 6 per cent if more power is transmitted through the gear. As, however, the single-phase motor has nearly three times the output of the others at this speed, the remaining 18 per cent has been divided by 3, giving a total gear and friction loss of about 12 per cent at this speed, and similarly with other speeds.† Using this efficiency curve instead of a uniform 84 per cent in Table A, the energy consumption becomes 2·23 kilowatt-hours, or 86·6 watt-hours per ton-mile (L.T.).

We have, however, still to determine the figure with which it is appropriate to compare the result of our calculations. In order to do so we should compute the test run, using exactly the same assumptions as were adopted in our calculated run, except in so far as change of

* Mr. Dalziel in the reply to his Inst.C.E. paper (*Minutes of Proceedings of the Institution of Civil Engineers*, vol. 179, p. 226, 1909–10) attributed to me the opinion that the single-phase motor lacked overload capacity. As I never held any such view I surmise that he merely misread my remark (*Journal of the Institution of Electrical Engineers*, vol. 36, p. 253, 1906) that the range of power is smaller in the single-phase than in the continuous-current motor; a simple statement of a general characteristic.

† It would seem probable that my efficiency curve is even now too high to represent the conditions under which Mr. Dalziel's test runs were made. For in the first place the figures given as Mr. Dalziel's efficiencies in the first three columns of Table B, which should correspond with these service tests, are generally lower than I have assumed. And secondly, the use of these efficiencies in Table C below gives the result 89 watt-hours per ton-mile against 92·8 obtained by test. This, however, does not affect the comparison between the two runs with which alone we are concerned.

TABLE C.

25¾ Tons per Motor. Addition for Rotational Inertia, 9·7 per cent ; 88/30 Gear Ratio ; 43½-in. Wheels.

Amperes.	Speed, m.p.h.	Mean Tractive Effort.	Train Resistance.	Mean Acceleration Tractive Effort.	Increment of Speed.	Increment of Time.	Time in Seconds.	Mean Speed.	Increment of Distance, feet.	Power (L.T.), kw.	Increment of Energy, watt-hours.
	0						0				
						23·00		13·00	439	130·0	832
900	26·0						23·0				
		3,665	278	3,387	1·5	1·28		26·75	50	232·0	83
800	27·5						24·28				
		3,110	307	2,803	3·5	3·60		29·25	154	215·0	215
700	31·0						27·88				
		2,555	363	2,192	4·4	5·78		33·20	281	200·5	322
600	35·4						33·66				
		2,150	418	1,732	2·6	4·32		36·70	233	187·0	224
550	38·0						37·98				
		1,875	464	1,411	2·8	5·71		39·40	330	175·0	277
500	40·8						43·69				
		1,620	507	1,113	2·6	6·72		42·10	415	161·0	301
460	43·4						50·41				
						0·29		43·45	18	155·0	12
	43·5						50·70				
	Coasting	retardation, 0·145 m.p.h. per second.				48·00		40·03	2,818		
	36·55						98·70				
	Braking	retardation, 1·8 m.p.h. per second.				20·30		18·27	544		
	0						119·00				
									5,282		2,266

2,266 watt-hours per motor-m e ; 88 watt-hours per ton-mile.

NOTE.—The train resistance was obtained by plotting the values given on Mr. Dalziel's Fig. 13, and employing the curve so derived. The speed and tractive effort columns were taken from Fig. 14, which is stated by Mr. Dalziel to correspond with the conditions of actual running on the road.

Mr. Carter. gear affects them. This has been done in Table C, the curves being plotted in Fig. 16, curves B and D.

The energy consumption per motor is now 2·26 kilowatt-hours, or 88 watt-hours per ton-mile (L.T.). Assuming, therefore, that it were practicable to give energy to the train at 84 per cent efficiency between the motor terminals and the driving wheels at all speeds and loads, the comparable figures for the high- and low-speed gears would be 88 and 82·5 watt-hours per ton-mile (L.T.) respectively, a difference of 5·5 watt-hours pei ton-mile instead of 15·3 claimed by Mr. Dalziel. If the more representative motor efficiencies shown at the foot of Table B be assumed, the comparable figures become 89 and 86·6 watt-hours per ton-mile (L.T.) respectively, a difference of only 2·4 watt-hours per ton-mile instead of 15·3. But even this figure is excessive, for the following reasons: The relation between current and speed is the result of direct test, and can be relied upon. The tractive-effort curve is derived in accordance with the efficiency. If, in order to take account of new factors, it becomes necessary to lower the efficiency curve, the tractive-effort curve must be lowered in the same proportion, the speed being unaffected. The tractive efforts given towards the end of Mr. Dalziel's Fig. 13 and my Table A are too large, so that the increments of time there given are too small. The correct speed-time curve would drop below that shown in Fig. 12, and it would be necessary to keep power on for a longer time in order to run to the schedule. This would materially increase the energy consumption with low-speed gears. Moreover, since the rotational inertia of the armature is increased by 88 per cent by the change of gear, I would estimate the increase to be nearer 3 per cent of the train weight than the 1·9 per cent allowed by Mr. Dalziel, which would still further increase the computed energy consumption of Table A. Altogether this closer analysis indicates that my original estimate of 2·7 watt-hours per ton-mile as the saving resulting from the change of gear is excessive. If we were possessed of full information as to the service characteristics of the motor, I think we should find no appreciable saving in energy consumption resulting from the change of gear. I am, in fact, disposed to think that the low-speed gears would be found, if anything, a little the less efficient, as they would certainly be the less desirable from the point of view of up-keep of motors. I have gone into the matter at some length in order to place it entirely beyond dispute, and I hope it is now clear to Mr. Dalziel that I have said nothing in this connection that is not fully justified.

Mr. Cunliffe makes a contribution to the subject of mechanics which is worthy of note, but there is one statement which I fear is not universally true. This is that "large changes in ΣF (due to changes in gear) must produce still larger proportional changes in acceleration." In railway work (as distinguished from tramway) F' is small compared with ΣF during the acceleration period, and the change in K, due to change of gear, which tends to reduce the change in acceleration, may easily preponderate and cause the opposite to

Mr. Cunliffe's statement to be true during the initial acceleration. Mr. Carter. Mr. Cunliffe will readily see that the acceleration will vary with change of gear faster or slower than ΣF, according as—

$$\Sigma \frac{I'g^2}{r^2} \lessgtr \frac{K}{2} \cdot \frac{F'}{\Sigma F - F'}.$$

Mr. Moffet has pointed out one or two things in the paper which I am bound to admit were not as clearly put as they might have been. With regard to the question of the effect of grades, I may mention that only 2½ miles out of 9 miles in the example dealt with is on a continuous gradient, whilst the figure of 6 per cent applies to the whole 9 miles. I have considered the whole distance involved because all trains were scheduled to run the whole distance, but of course a service involving the first 2½ miles of the line only is quite conceivable; and in this case the effect of the gradient would doubtless be to increase the energy consumption by 15 or 20 per cent over that corresponding to level track. My statement that the average effect of gradients is generally small should therefore be taken as applying to normal lines, but not necessarily to abnormal lines. The scheduled speed on the down-gradient would be greater than that on the level, which would again be greater than on the up-gradient. Such differences due to gradients would be revealed by a study of most suburban time-tables. It is, however, a matter of judgment how much these speeds shall be allowed to differ, and to this extent the effect of gradients is indefinite. With regard to my statement on page 450, to which Mr. Moffet is inclined to take exception, I am doubtful whether he quite grasps all the circumstances of the problem. It is quite true that if we consider a certain train running a certain distance we do introduce more coasting and reduce the energy consumption by cutting off power earlier, but we also take longer time for the given distance, and therefore run at lower speed. This, I believe, is all that is to be understood from the statement that introducing more coasting reduces the energy consumption. If, however, the time of the run is specified, and we change the gear so as to introduce more coasting without change of average speed, it is not at all necessary that the energy consumption should be reduced, for in most cases it would be slightly increased. In the particular case of curve C, Fig. 8, there is no coasting whatever, and the time is already in excess of the allowable time. Until we come down to the proper time, therefore, we do not begin to introduce coasting, and the higher speed gear in this case certainly implies greater energy consumption. I cannot give any rule of universal application for finding the most economical gear. In general, it is the lowest speed gear capable of performing the service with sufficient margin, but if Mr. Moffet will refer to the general remarks on the effect of change of gear in my reply to Mr. Dalziel he will see that any rule must be applied with caution, and I would generally recommend keeping a little on the high-speed side of what seems to be

Mr Carter. the most economical gear. However, it is perfectly true that within appropriate and fairly wide limits the gear ratio makes little difference to the energy consumption, and other considerations, particularly those of mechanical suitability, come into the determination of the most satisfactory gear.

Mr. Lydall appears to have misapprehended the object of the paper; he has, however, indicated the proper method for a railway company to pursue in order to arrive at a satisfactory comparison between the systems, *i.e.* to obtain from firms of repute tenders and guarantees of running costs for a definite service of considerable magnitude for electrification on the various systems. This has recently been done by the Commissioners of the Victorian Railways, who have in contemplation a very large electrification scheme in the neighbourhood of Melbourne. In this case there is no existing electrification or other circumstance to give initial advantage to either system; and as full tenders and guarantees have been invited from all the chief manufacturers of the world, the conclusions arrived at cannot fail to carry weight with other railway authorities. Mention may also be made of the Pennsylvania Railroad, the management of which undertook an elaborate and expensive series of comparative tests of the two systems before deciding which to adopt for their New York terminal service. As far as I am aware these are the only two cases of railway electrification in which a proper comparison between the two systems has been undertaken.

The Chairman has raised several matters of considerable interest and importance. By the statement on page 437, to which he draws attention, is implied that in general for most of the time that power is on the accelerating force is considerably greater than that required to overcome train resistance; so much so that the uncertainty in the latter is almost negligible. This is sufficiently true for suburban service, but becomes less and less true as the distance between stops becomes greater. The passenger load of 8·5 tons mentioned on page 439 is an average load; the full-seated load would be about 12 tons. The coaches in this case are somewhat unnecessarily heavy, for after the needs of strength and stiffness have been met the most desirable quality in coaches for suburban service is lightness. The figures italicized in Table I are so distinguished as being discarded in the final calculation, leading in fact to the points shown on the dotted extensions of the curves of Fig. 3. I should probably have done well to have marked my various curves for the benefit of those less familiar with the subject, but I would say that Mr. Cramp's interpretation of Figs. 4 and 5 is quite correct. The cross-line is a distance curve to be read in connection with the scale on the right-hand side; it is introduced here particularly to enable the times to be found at which gradient changes take place, so that their effect on the speed and power can be seen.

I quite agree with what Mr. Sayers says; there have been far too many comparisons made between things that are in no way compar-

able. Probably the reason that so few realize the enormous importance of the telegraph and telephone interference question is that given by Mr. Sayers—that few have had practical experience with single-phase railways. Mr. Carter.

Mr. Livingstone's contribution deals from a slightly different point of view with a matter touched upon by Mr. Cramp, and calls attention to a limitation in the way of accurate calculation. He remarks, very truly, that it is in the case of the comparatively short runs of suburban service that the calculated results are reliable; for in the case of long runs the inherent uncertainty of train resistance introduces a corresponding uncertainty in the calculated result. In this connection it may be of interest to point out a difference between calculations for the continuous-current and single-phase systems of operation, due to the difference between the characteristics of the motors involved. As I have pointed out in the footnote on page 482, the single-phase motor has a smaller practicable range of power on the speed curve than the continuous-current motor, as can be seen from a casual inspection of train characteristics. Figs. 4 and 5, for instance, show more than 3 to 1 range in the power curve, while Fig. 7, which it may be noted is not a practicable case, and which is abnormal in this respect, shows only about 2 to 1 range of power. If there is uncertainty in train resistance, the calculation will be affected by uncertainty in speed and power. If we were considering a constant-speed motor, like the induction motor, the whole uncertainty would be in the power. In considering the continuous-current and single-phase systems, however, the latter is affected more in speed and less in power than the former, due to this difference in motor characteristics. For instance, if the assumed train resistance is 10 per cent out, the effect may be to throw the free running speed out by 4 per cent, and the corresponding power out by 6 per cent in the continuous-current system, whilst in the single-phase system the speed may be thrown out by 6 per cent and the power by 4 per cent. These figures are chosen merely to illustrate a tendency, and there is no suggestion that they correspond with the facts of any particular case.

Mr. Dawson in the first paragraph of his communication confuses two uses of the word "efficiency." He finds the energy output in my Table I to be 77 per cent of the input. This is the true efficiency of the equipment, and there is nothing abnormal about it. Approximately one-third of the energy is supplied during the acceleration period, at an average efficiency of about 57 per cent, and two-thirds during speed-curve running at an average efficiency of about 88 per cent. If it were practicable to measure the output of the equipments in the instances given by Mr. Dawson, efficiencies between 75 per cent and 80 per cent would be found in every case. But it is *not* practicable, and it is therefore impracticable to determine the efficiency in actual service. The so-called efficiencies cited by Mr. Dawson are merely ratios of computed outputs to observed inputs. The output is usually computed by the method mentioned on page 450 of the paper. The input is usually

Mr. Carter. obtained from readings of sub-station instruments on the outgoing lines, divided by the ton-mileage scheduled—the tonnage being that of empty trains, as it is impracticable to determine the load. The result is a false efficiency, having no particular relation to the figure with which Mr. Dawson has identified it, though from some points of view of more value than the true efficiency. In my case, for instance, the output is 45·5 watt-hours per ton-mile, and the input being 59·4 watt-hours per ton-mile, the efficiency is 76·7 per cent. If, however, I had reckoned my input on the tonnage of empty trains it would have appeared as 64 watt-hours per ton-mile, and the efficiency would have seemed 71 per cent; whilst if allowance were made for grades, curves, adventitious stoppages, unscheduled operation, and other circumstances of actual service, the efficiency would have appeared yet lower, though still of the order of 77 per cent in reality. Mr. Dawson's difficulty is therefore easily explained, and the deductions he has drawn from his supposed discovery are clearly based on a fallacy.

Dealing now with the extract from Mr. Dawson's recent paper given on page 448, I see nothing in his comment to alter my view that this is a particularly good example of unsound mechanics. He remarks casually that it is not dynamically impossible to run the journey in the time stated. On this point I would say that the treatment given in the paper, rough as it is, amounts to a proof of the dynamical impossibility alleged, based entirely on Mr. Dawson's record of the performance of his trains, a record too which is consistent with every other record that he has presented. He can therefore hardly maintain his present contention without destroying the credit of the whole of his data. He, however, now admits errors in his curves and calculations, and as the result of his reconsideration of the matter states that the figure to be compared with Mr. Aspinall's 96 watt-hours per ton-mile—the very figure, that is to say, that we are discussing—should have been 100 watt-hours per ton-mile, an advance of 40 per cent on his original estimate of 71 watt-hours per ton-mile. The error that Mr. Dawson alleges in my calculation is no error, but an intentional omission of a complication whose effect is inconsiderable compared with the unavoidable uncertainties of the method employed.

With regard to the non-stop run between Victoria and London Bridge Stations, mentioned on p. 452, Mr. Dawson's explanations and amendments do not even render his test result physically possible, as I shall show after dealing briefly with some of the arguments by which he seeks to justify it. The quotation he gives from Mr. Hobart's "Electric Trains" indicates that he agrees with Mr. Hobart and myself that the mean train resistance is greater than the train resistance at mean speed. As this is capable of very simple but rigid mathematical proof,* however, it is hardly a matter of opinion. In the

* Starting with the curve between train resistance and speed, let the tangent be drawn at the point corresponding to the mean speed V, the train resistance at this point being F. Let the ordinate at the point on the tangent corresponding to train speed $V + v$ be $F + av$, a being the slope of the tangent, which is essentially positive since train resistance increases with speed. Let the ordinate of the point on the

Mr. Carter.

present instance the mean speed is 37 miles per hour, whilst the variation is between 0 and 50 miles per hour. Accordingly the mean train resistance is demonstrably greater than that at 37 miles per hour. But Aspinall's, and Hobart's, as well as his own train resistance results, are for straight and level track, whilst he particularly insists upon the exceptional difficulties of his route, due to curves and gradients, which, as is pointed out, impose speed restrictions. Difficulty is represented by energy consumption; if he reduces speed once only in the course of the run from 40 miles per hour to 30 miles per hour he throws away energy representing more than 2 watt-hours per ton-mile, or adds effectively more than 1 lb. per ton to the train resistance, besides necessitating increase of speed, and therefore increase of train resistance elsewhere. Curves also add to the train resistance even when they impose no speed restriction. Mr. Dawson therefore implicitly bids me reject his repeated asseverations concerning the exceptional difficulty of his route, and to treat it as straight and level track. I do not consider 16 lb. per ton too high for the average effective train resistance in this run.

With regard to the energy dissipated in braking, if Mr. Dawson will think this matter over he will see that it has but small effect on such an estimate of energy consumption as I have made. It is true that if I apply brakes at 40 miles per hour I dissipate 6 watt-hours per ton-mile in them, whilst if I coast to 20 miles per hour before applying brakes I dissipate only 1·5 watt-hours per ton-mile; but in the latter case I am introducing two or three miles of running at an average speed of about 30 miles per hour, necessitating greater speed elsewhere, and the increase in effective train resistance due to this variation practically compensates for the saving in braking energy. Any difference between us on this point is allowed for in my assumed train resistance. It will be noted that the prevailing gradient on which coasting takes place is downward, so that Mr. Dawson cannot have kept power on much beyond Peckham Rye if he braked from 20 miles per hour. It is true that Mr. Dawson claims †

train resistance curve corresponding to speed $V + v$ be $F + a v + f$; then f, which is zero when $v = 0$, is elsewhere always positive, since the curve lies above the tangent whether the speed be above or below the mean speed. The work done against train resistance in the total time T to which the mean applies is—

$$\int_0^T (F + av + f)(V + v)\,dt = FVT + a\int_0^T v^2\,dt + \int_0^T f(V + v)\,dt,$$

since—

$$\int_0^T v\,dt = 0.$$

Here the several integrals are taken over the speed-time curve of the train for the interval of time T. The quantities a, v^2, f and $V + v$ each being positive at all times, it follows that the work done against train resistance exceeds that which would have been done had the speed remained at its mean value V, by the positive amount—

$$a\int_0^T v^2\,dt + \int_0^T f(V + v)\,dt.$$

and thus the mean train resistance is necessarily greater than the train resistance at mean speed V.

† Page 473.

Mr. Carter. that his speed was fairly uniform from the end of the period of acceleration to the commencement of the period of braking; but as the average speed between these periods exceeds 37 miles per hour, and according to Mr. Dawson reached a maximum of 50 miles per hour, whilst the speed at the commencement of braking is alleged to have been 20 miles per hour, it is difficult to see how he managed to effect it. I am afraid Mr. Dawson wants the advantage of uniform speed in estimating the mean train resistance, and of low braking speed in estimating the energy dissipated in braking. I considered all these matters fully before raising the issue, and am satisfied that the estimate of energy output given in the paper is as near correct as the occasion requires; for it would not affect my argument if my estimate were 10 per cent or so in error. I now purpose examining the result of admitting all Mr. Dawson's amendments to my figures, and to show that his alleged performance is still impossible. Mr. Dawson wishes the mean effective train resistance for his run on this particularly difficult route to be taken at 12 lb. per ton, and brakes to be applied at 20 miles per hour. This gives him an output of 25·45 watt-hours per ton-mile, and (the corresponding input being 32 watt-hours per ton-mile) an average efficiency * between distributing cabin and driving wheel of 79·5 per cent.

The efficiency in question is a combination of trolley line, transformer, and motor efficiency.

The following Table gives a few efficiencies, and the times for which they are effective, taken entirely from Mr. Dawson's records :—

Time.	Speed.	Efficiency.	Remarks.
20 seconds †	0–21 m.p.h.†	68%‡	Acceleration period
10 seconds †	21–29 m.p.h.†	82% to 78%§	Motors only‖
30 seconds †	22–40 m.p.h.†	78% to 68%§	Motors only
Say 5 minutes	Say 35–50 m.p.h.	73% to 65%§	Motors only

The main transformers are said to have an efficiency of 97 per cent at full load.¶ It is not necessary for me to suggest a figure for the

* As the figure of 32 watt-hours per ton-mile is based on the weight of the empty train (138 tons), whilst the output is based on the train as actually loaded in the run, the actual efficiency claimed is really somewhat higher than 79·5 per cent. See above, page 488.

† *Minutes of Proceedings of the Institution of Civil Engineers*, vol. 186, Plate 4, Fig. 28, 1910–11

‡ Ibid., vol. 186, p. 34, 1910–11.

§ Ibid., vol. 186, Plate 4, Fig. 27, 1910–11.

‖ That the transformer efficiency is not allowed for in the motor curves is clear from Mr. Dawson's statement concerning these curves, that a motor current of 210 amperes at 450 volts is equivalent to 15·8 amperes at 6,000 volts, the power factor being 29 per cent (Ibid., vol. 186, pp. 18 and 27, 1910–11).

¶ Ibid., vol. 186, p. 28, 1910–11.

mean efficiency between distributing cabin and driving wheel, but seeing that the efficiency of the motor alone does not exceed 79·5 per cent for more than 10 seconds out of the whole 8 or 10 minutes that power is on, and that the combined efficiency of motor and main transformer hardly reaches the figure of 79·5 per cent at any time, whilst for the remainder of the time that power is on the motor efficiency alone is of the order shown in the above Table, I can say definitely that the average combined efficiency of motor, transformer, and trolley line is very far below 79·5 per cent. Mr. Dawson will have to modify his estimate of output a great deal before he can claim that his result is other than a violation of the principle of conservation of energy. Mr. Carter.

THE CHANGE OF ENERGY LOSS WITH SPEED IN CONTINUOUS-CURRENT MACHINES.

By Professor W. M. THORNTON, D.Sc., D.Eng., Member.

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The efficiency of the large turbo-generators now coming into use is difficult to determine experimentally by direct measurement, and, except in split-unit sets, the Hopkinson method cannot well be applied. It is therefore necessary in developing a new design to have some rule, based on previous tests, which can be extended to the new case. Certain losses, from copper heating, or windage and friction taken together, can be readily found, and manufacturers have each their own special formula dealing with iron loss as a whole. All losses, even to some extent copper heating, vary with speed, and it is one of the objects of the constructor to deduce from experimental tests an expression for loss covering the whole range of size and speed. This is at present difficult on account of the rapid increase in dimensions; but to establish any general rule it is first necessary to have accurate measurements at certain typical speeds and sizes. During the last few years a number of very careful measurements, especially on iron loss, have been made on small machines, but there are not yet sufficient to establish without question the power index of iron loss with regard to speed, or the coefficients for any given type.

The present paper deals with measurements for the purpose of examining the dependence of loss on speed, made upon a 7½-h.p. Westinghouse continuous-current 4-pole motor, running at a normal speed of 1,500 revs. per minute. The windage tests are novel in that they were made by running the machine in air and *in vacuo*.

A large cast-iron tank, specially made for the tests and capable of exhaustion to a pressure of a few millimetres of mercury, was fitted to contain the tested machine. The latter was driven from an external motor by a ½-in. steel shaft passing through a stuffing-box. The efficiency of the motor at the power and speed used was determined with great accuracy, and the power supplied to it during the tests measured by standard instruments. After each change in speed four to five hours were allowed to elapse before taking a reading, in order that bearing temperatures might become steady and that the temperature of the driving motor might also adjust itself to the change of load upon it. It was found that the power taken rose and then fell slowly after every

increase of speed, and, whether the speed was raised or lowered, did not in any case become steady under three hours.

The windage measurements were made by Mr. G. T. Williams, M.Sc., and those following by Mr. B. J. M. Lane, B.Sc., two skilled observers, to whom the writer is indebted for analyses of their observations, which are embodied in the following remarks. It was originally intended to find the windage loss in different vacua in order to bring out more clearly the cause of the various terms in the expression for windage, and to repeat the iron loss tests using a shaft dynamometer method; but other interests have arisen and the present results can only be regarded as preliminary, and as giving suggestions for further work.

The loss caused by bearing friction and windage together is most usually taken to obey a law of the form, watts $= a\,n^x$, where x has been found to be nearly 1·5, n being the rate of revolution. Neither of the

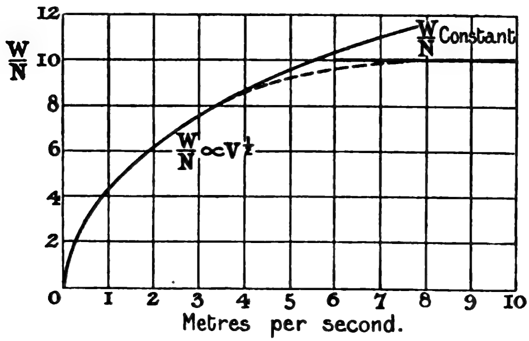

FIG. 1.—Change of Law of Bearing Friction with Speed.

components, however, obeys a simple law. At high velocities Arnold, summarizing the researches of Beauchamp Tower, Dettmar, Stribeck, and Heimann, gives for bearing friction at temperatures between 40° and 100° C. the relation*—

$$W = 9{\cdot}8\,\frac{K}{T}\,l\,d\,v,$$

in which v is the velocity of rubbing in metres per second, l the length and d the diameter of the bearing in centimetres, T the temperature in degrees Centigrade, and K a coefficient, numerically about 2, depending on the oil. Values for K are given by Dettmar, but they include windage loss with bearing friction, and therefore vary with the type of motor as well as with the bearing conditions. At lower velocities, from 0·5 to 4·0 m. a second, and over the same range of temperature—

$$W = 9{\cdot}8\,\frac{K}{T}\,l\,d\,v^{1\cdot5}.$$

There is, therefore, a period of transition, as in Fig. 1, from one law to the other, possibly depending on a change from the predominance

* *Die Gleichstrommaschine*, vol. 1, p. 676.

of rolling to that of sliding friction in the oil. If the temperature remains constant during a trial there must still be a point of flexure in any curve of loss and speed corresponding to entrance on this region.

The power required to drive air through a large machine is not negligible. All that design can do is to remove the loss by wind eddies, where these have no cooling influence, and to direct the air to where it can be most usefully applied as a cooling agent.

The laws of gaseous eddies would lead to an expression in which the resistance to movement varies as the square of the velocity, or the power as its cube, and all windage loss is generally taken to follow this law. There is, however, in the present experiments evidence of loss corresponding to a lower index.

THE WINDAGE INDEX.

To run the motor enclosed in the vacuum chamber electrically would have had the following objections. There would necessarily have been

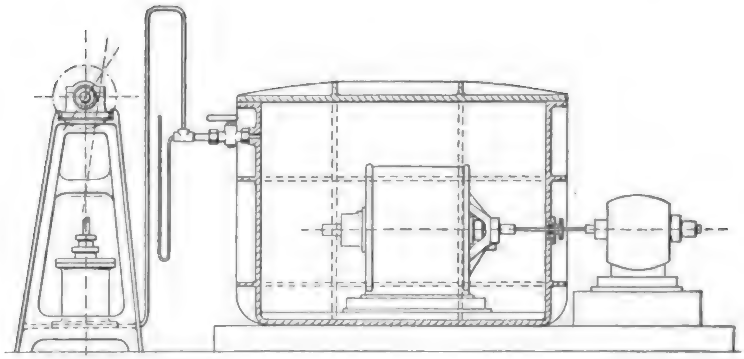

FIG. 2.—Arrangement for running *in vacuo*.

heating of the field coils, and without any convective influence of the air the temperature of these would certainly be higher than under normal conditions in air. The influence of removing the air on field coil temperature was examined by Mr. G. T. Williams,* and his interesting result, that exactly one-half the field heat is removed by the direct action of the air, seems to have escaped general notice. There would also be the iron loss to be removed when running *in vacuo*, and since all the heat from field or armature could only be conducted away through the solid metal of the machine, the bearing temperature would not be the same for the same speed as when air was admitted. In the present method it is the same at the same speed in the two cases, for there is no energy given to the machine other than through the shaft mechanically.

The machines were arranged, as in Fig. 2, with the brushes removed

* *Electrician*, vol. 63, p. 707, 1909.

from the driven machine. The air pump was in connection with a surface condenser supplied with a trickle of steam and circulating water to keep the pump wet. No trouble was experienced in maintaining a high vacuum for days. The driving motor was run, separately excited, from large storage cells, and the power measured by Weston laboratory standard instruments. The following results were obtained, each reading corresponding to four hours' interval. The speed is in revolutions per minute, and the power is that supplied to the driving motor.

TABLE I.

Speed and Power in Air.

Speed.	Watts.	Speed.	Watts.	Speed.	Watts.
475	63·0	1,100	163	1,595	290
590	83·0	1,181	185	1,660	303
720	102·0	1,250	196	1,705	309
815	117·5	1,310	215	1,725	316
876	126·0	1,345	225	1,860	360
953	138·0	1,425	229	2,000	409
1,018	150·0	1,490	252	2,145	475

Table II is an average of the readings in an absolute pressure of 0·25 in. of mercury; and these readings, with the results of Table I, are shown graphically in the curves of Fig. 3.

TABLE II.

Speed and Power in Vacuo.

Speed.	Watts.	Speed.	Watts.	Speed.	Watts.
600	76	1,300	185·0	1,810	302·0
790	100	1,485	225·0	1,970	345·0
1,055	138	1,630	257·5	2,145	377·5
1,140	162	—	—	—	—

The difference between the readings at any speed is the apparent loss of power due to wind resistance, in terms of the input of the

driving machine. The actual loss is clearly the product of this and the efficiency of the external motor when under the same mechanical conditions. This machine was calibrated at the different speeds under electrical conditions and with a mechanical torque identical with those used in the tests, and the relation between speed and efficiency found to be a straight line, as shown in Fig. 4. The higher speeds were obtained by weakening the field of the motor, with the effect that the efficiency rises slightly.

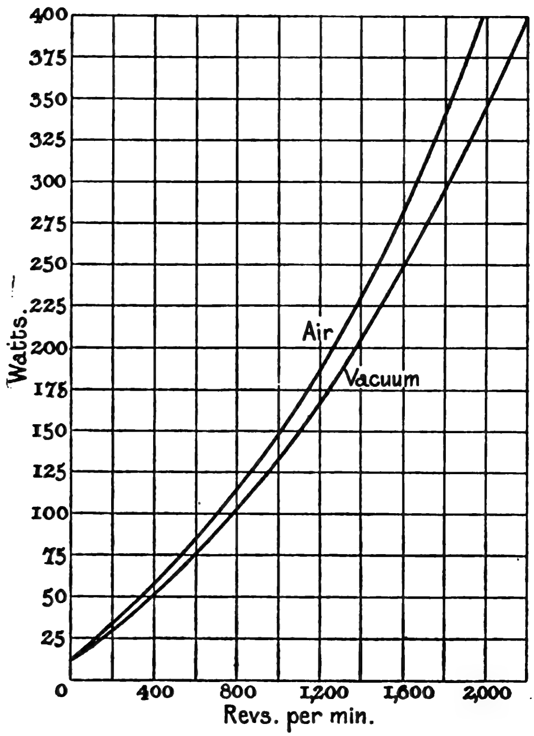

FIG. 3.—Power and Speed in Air and in Vacuum.

We have then the final values as given in Table III (p. 498).

The variation of loss with speed derived from these figures cannot be made to fit exactly any expression of the form $W = a\,n^x$. It will first be observed that the curve of loss in Fig. 4 approaches zero at a definite angle to the horizontal axis. The magnitude of this is too great for experimental error of observation and, whatever may be its cause, it points to a form $W = a\,n + b\,n^x$.

Following the usage of the Kapp-Hausmann diagram and plotting W/n against speed, the curve of Fig. 5 is obtained, which, intercepting

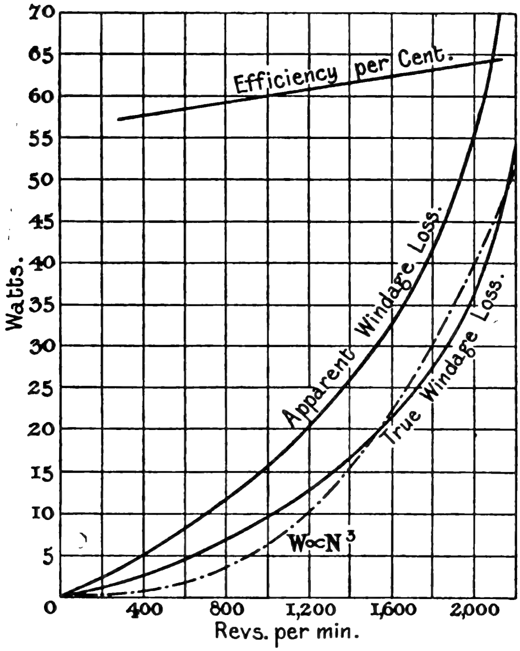

FIG. 4.—Apparent and True Windage Loss.

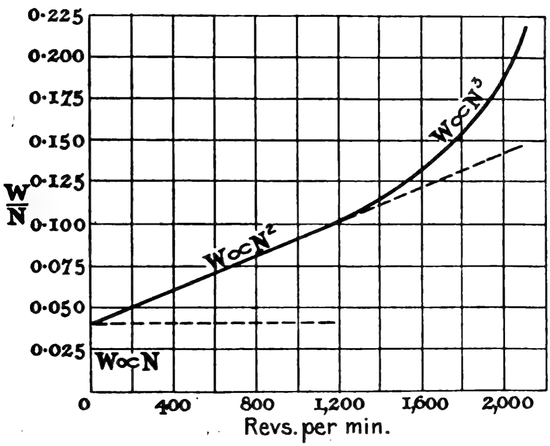

FIG. 5.—Wind Resistance Index and Speed.

the vertical axis above zero, confirms the existence in these experiments of a term proportional to the first power of the speed. Perhaps the most interesting feature of this curve is that it is straight over a long range of speed, and that there is a remarkable increase in the value of W/n, beginning in this case at about 1,100 revs. per minute. This long straight portion, which corresponds to a term in the power of the form $b\,n^2$, may explain the straightness of the line usually found in the running-light method of testing losses. Windage would then fall in with eddy-current loss over this range of speed. The fact that this line is sometimes curved from the base is explained later in the consideration of eddy-current loss, and that it is more often bent upwards at high speeds is to be expected from the prominence of the higher power windage loss, only developing at some critical speed, peculiar no doubt to each machine or type of design.

TABLE III.

Speed and Windage Loss in Watts.

Speed.	Apparent Windage Loss.	True Windage Loss.	Speed.	Apparent Windage Loss.	True Windage Loss.
400	5·5	2·6	1,400	26·5	16·5
600	8·5	4·5	1,600	33·5	21·2
800	11·6	6·9	1,800	42·0	27·0
1,000	15·6	9·4	2,000	57·0	34·2
1,200	20·6	12·6	2,200	85·0	55·0

The complete expression derived from the curve of Fig. 5 for the power absorbed would then appear to be here of the form $W = a\,n + b\,n^2 + c\,n^3$, in which the first term corresponds to a resistance of constant amount, the second to one proportional to the speed, the third to the square of the speed. As a first and rough approximation the curve can be represented by $W = 5{\cdot}5 \times 10^{-9}\,n^3$, though with this the values are at first low, and at intermediate speeds high, as shown in Fig. 4.

This is the law generally taken to represent windage loss and is always characteristic of loss of power by turbulent motion, in which the resistance is proportional to the square of the speed. Any resistance proportional to the speed can only be caused by opposing forces of the nature of *skin friction*, in which the energy is communicated to the surrounding medium through disturbances of the nature of collisions rather than eddies. The existence of a constant resistance is, so far as I know, a new feature in wind resistance experiments, and should, therefore,

only be received after further experimental evidence. This term alone would require that the same body of air should be moved every revolution against a constant resistance. It is possible that the condition of the bearing friction might have been changed by vacuum at its ends, or that the state of contact of the stuffing-box packing around the ½-in. steel shaft was altered by the air pressure along the shaft from outside. The intercept on the vertical axis has a value of $a = 0{\cdot}0042$ watt per revolution, at zero speed, and a slight change in the conditions of mechanical friction might account for the effect. It cannot, however, be omitted here as unimportant relatively to the other terms, and it is hoped to make further experiments on the point.

Assuming that at moderate velocities the law $W = a\,n^{1\cdot5}$ holds for windage and bearing friction, and that windage alone follows the law $W = b\,n^2 + c\,n^3$, it does not seem possible to reconcile the transition to the law of Fig. 1, that the resistance is constant at high speeds, without a definite change in the nature of bearing friction at some critical speed. It may be remarked that trials on centrifugal pumps or blowers cannot discriminate between square and cube indices, for by the nature of the case translational and whirling energy entirely masks gliding friction. Only whirling table experiments on gliding planes can determine the value of the skin resistance, and to represent the present case fully it would be necessary to drive smooth cylinders with rounded ends in air and *in vacuo*.

The conclusion from the present experiments is that there are at least two terms in the wind resistance of electrical machines, one corresponding to the formation of eddies, the other to a skin friction. For modern types of covered armature the former term only becomes important at high speeds, whilst for smooth covered surfaces the latter term is not negligible at low speeds. The two machines were not moved between the air and vacuum trials, and the mechanical condition of the bearings was the same in both cases.

Second Series. Bearing and Windage Index.

The same machine removed from the vacuum tank and driven by the same motor was tested by the measured power and running-down methods under three conditions: with solid poles, with solid poles and laminated pole-shoes, and with poles laminated throughout. In each case the friction plus windage loss was separately measured, and since there was no change other than the removal of end bearings to permit the armature to be drawn and the poles interchanged, the mechanical conditions should have been the same in each case. It will, however, be seen from Fig. 6 that the three windage and friction lines, curves 1, 2, and 3, do not coincide. This illustrates the effect on bearing friction which may arise in practice from exceedingly small changes in alignment.

From the curves of Fig. 6, which show the power delivered to the shaft of the machine under test, we have the following any three readings :—

TABLE IV.

Speed and Power in Air.

	Speed.	Watts.			
		Curve 1.	Curve 2.	Curve 3.	
n	1,200	112·2	94·8	106·0	W
n_1	1,300	125·0	106·0	118·3	W_1
n_2	1,408	139·5	118·8	132·3	W_2

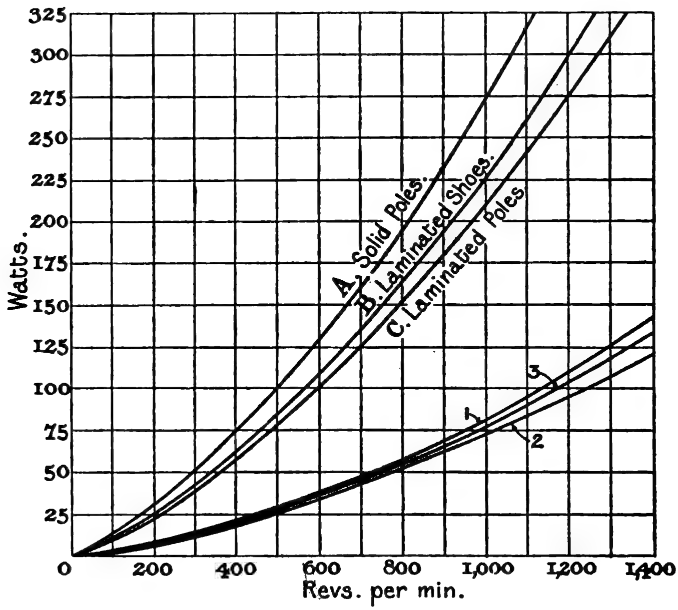

FIG. 6.—Power and Speed with Solid and Laminated Poles.

Writing at these speeds the combined friction and windage loss in the form $W = a n^3 + b n^x$, where a is the windage loss coefficient and b that for friction, x being the mechanical friction index, three readings are required to find a, for from the above we have—

$$a = \frac{W_1 W_2 - W_1^2}{W_2 n^3 + W n_2^3 - 2 W_1 n_1^3}.$$

The values derived from corresponding quantities in Table IV are $5{\cdot}4 \times 10^{-9}$, $6{\cdot}3 \times 10^{-9}$, $5{\cdot}95 \times 10^{-9}$, for curves 1, 2, and 3 respectively—giving an average value of $5{\cdot}88 \times 10^{-9}$.

The approximate loss due to windage alone at a speed of, say, 1,300 would then be $5{\cdot}88 \times 10^{-9} \times 1300^3 = 12{\cdot}9$ watts. The value obtained from the vacuum curves is 12·5; the cube law would, therefore, at this point seem approximately to satisfy both sets of measurements.

Having found the coefficient a the bearing friction index x can be determined, thus:—

$$x = \frac{\log \dfrac{W - a\,n^3}{W_1 - a\,n_1^3}}{\log n/n_1}.$$

This is given in Table V.

TABLE V.

Index of Bearing Friction Loss.

Speed Range.	x.			Mean.
	Curve 1.	Curve 2.	Curve 3.	
1,200–1,300	1·18	1·20	1·18	1·19
1,000–1,050	1·21	1·25	1·21	1·23
550–600	1·44	1·55	1·60	1·53
200–400	1·59	1·75	1·67	1·67

The last row was calculated on the assumption that the windage loss was negligible, and though it is exceedingly small this may not be quite the case on account of the influence of the constant term previously discussed. The above figures, as far as they go, would show that the shaft friction index does perceptibly change with speed, and in a manner not unlike that required to fill the gap in Fig. 1, between $W = b\,n^{1{\cdot}5}$ at low speeds and $W = b\,n$ at high.

IRON LOSS INDEX.

Repeating these tests with the fields magnetized to give a gap flux density of 5,800 lines per square centimetre, the curves A, B, and C of Fig. 6 were obtained, and, as before, four hours were allowed between readings. The intercepts between these curves and the corresponding lower set are equal to the combined loss by eddy currents and hysteresis. The evident effect of partial or complete lamination is, as anticipated, the more than proportionate increase when solid poles were used, being no doubt due to eddies in the pole-face.

Taking, as before, the ratio W/n as ordinate, a further set of curves, Fig. 7, was obtained, which are of interest. Each consists of two distinct parts, and the point of flexure is in each case at a speed between 750 and 800 revs. per minute. Below this the curves are parallel; above it they diverge in order of lamination. The intercepts on the vertical axis correspond to the hysteresis loss in each case, or one proportional to the speed; being $W/n = 0{\cdot}115, 0{\cdot}074$, and $0{\cdot}062$ for solid, partial, and complete lamination respectively. Denoting these

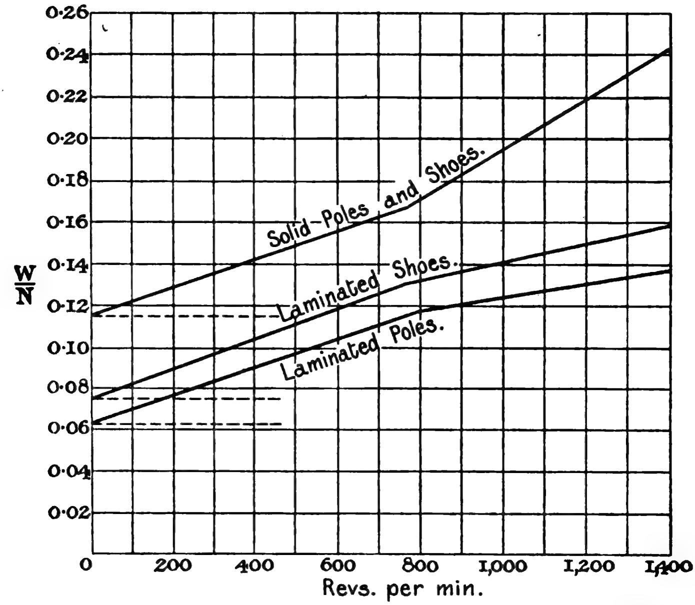

FIG. 7.—Change of Coefficient of Eddy-current Loss with Speed.

intercepts a, b, c, we have the corresponding ratios in the three cases—

$$\frac{W_a}{W_c} = 1{\cdot}87, \quad \frac{W_a}{W_b} = 1{\cdot}55, \quad \frac{W_b}{W_c} = 1{\cdot}20,$$

expressing the influence of lamination on hysteresis loss in the poles. If the hysteresis loss in laminated poles can be neglected in comparison with that in the armature, so that the whole of the ratio 0·062 is derived from the latter, we have the result that even with laminated shoes and solid poles there is an increase of 20 per cent in the total first power loss, and with solid poles and shoes 87 per cent increase, as distinct from second power loss. The ratio of slot opening to air-gap is 3·5, the large value of which might be expected to give rise to larger than usual pulsations in the pole flux.

Since the lines of Fig. 7 are straight, the eddy-current index

derived from them is in each case 2, both for speeds above and below the point of flexure. Apart from eddy-current loss in the armature, which is known to follow the square law, this shows that there is "turbulent" magnetic movement in the poles rather than symmetrical pulsation in the solid yoke, for the former obeys a square law everywhere, the latter giving rise to a loss proportional to the square root of the speed.

But though the speed index is the same for each curve the coefficients are not. Assuming a general expression—

$$W_h + W_e = c\,n + d\,n^2,$$

c has the values 0·115, 0·074, 0·062 previously given. Below a speed of 800, d is the same for each curve and is equal to $6{\cdot}7 \times 10^{-5}$; the values above this are $1{\cdot}2 \times 10^{-4}$, $4{\cdot}7 \times 10^{-5}$, $3{\cdot}2 \times 10^{-5}$ respectively, n being the number of revolutions per minute.

The cause of the change in the eddy-current coefficient at, in this case, 800 revs. per minute, is to be sought either in the amplitude or in the type of the magnetic oscillation in the poles. The frequency in the armature is then 27 periods, and the tooth frequency 676 per second. From the fact that the solid pole curve bends upwards and the others downwards, it follows that with a certain degree of lamination the eddy-current coefficient would be the same at all speeds. The change cannot be a magnetic resonance effect, though it may possibly be the result of mechanical resonance, and it would appear to depend more upon type of magnetic pulsation than upon amplitude. The "hog-backed" curves occasionally found in the running-light tests may, perhaps, be due to a change similar to that of the lower lines.

The conclusion from these tests is that they agree very fairly well with those *in vacuo*, as regards windage, and that the bearing friction loss is relatively higher at low than at high speeds, the index decreasing with rise of speed. Pole lamination has apparently an effect upon hysteresis as well as upon eddy-current loss. The index of the latter is in all cases 2, but the coefficients change at some critical speed, being the same in all cases at low speeds.

No attempt is made to derive a working formula from these preliminary trials, the first object of which was to observe, as accurately as possible, the change with speed of each of the causes of loss of energy, other than copper loss, in machines of a simple type.

Discussion before the Newcastle Local Section on 11th November, 1912.

Mr. A. H. Law: I think all designers agree that, at least in high-speed machinery, the question of windage is one of great importance; and although the author's experiments have been made on a plant of small size, the data appear to be largely applicable to larger machines. The windage of a turbo-alternator has a very important bearing on its efficiency; and in some cases where the windage has been excessive, Mr. Law.

Mr. Law. it has been possible to reduce the ventilation considerably without increasing the temperature rise of the plant, owing to a reduction of the losses caused by air. In alternators designed for very low temperature rises it has been found an advantage to put the ventilating fan on the outlet from the machine, so that the heating up of the air due to its passing through the fan occurs after the air has done its work in cooling the alternator. The question of surface speed has an important bearing on the windage of a plant, and in some cases where a designer is tempted to take a large diameter carcase for a moderate output by simply shortening its length the windage has been found to be very much more than was anticipated.

Mr. Davidson. Mr. C. H. Davidson: The machine Professor Thornton has experimented upon is rather a small one; in practice, of course, we have to find these losses on machines of very much greater output. In ordinary commercial testing we cannot keep a machine for a long enough time to make complete experiments, and we also cannot put 6,000-kw. plants in a vacuum box. I have been able to find some windage-loss curves for plants of various sizes, and in no case does a simple expression agree with a curve. Some curves vary approximately as the square of the speed, and some approximately as the cube. If a formula were worked out for one curve it could not be applied to other machines having different proportions of length to diameter, different surface speeds, and of different construction. The matter is further complicated by such items as the different shapes of slots and air-passages, and by the presence of various obstructions. In the case of a 250-kw. continuous-current armature, a curve giving the windage losses plus bearing losses was obtained. On assuming that the bearing losses varied directly as the speed, it was found that the windage loss varied very nearly as the square of the speed. This was a smooth armature with binding wire, and centrifugal fan action would be almost entirely absent. Of course, it is not correct to assume that the bearing loss is proportional to the speed; it goes up with increasing speed, and so the true windage curve will show the existence of the "$a\ n$" as well as the "$b\ n^2$" term. Always on these high-speed armatures there is a skin of air being carried round and round by the armature, to which it seems to stick, and Professor Thornton will, perhaps, be able to trace in this the origin of the "$a\ n$" term. In some cases this air can be got rid of by fitting scrapers, when the pole-pieces do not have this effect themselves. In the case of a 250-kw. alternator of an old type, specially built for single-phase work and having an "H" type revolving field, we found that the windage curve varied very nearly as the cube of the speed. In this case the rotor had an enormous centrifugal action, which would account for the size of the third power term in the equation.

There are one or two other points in Professor Thornton's paper to which I should like to refer. He mentions in one place a critical speed. I should like to suggest that this speed is the one where sufficient head is obtained to start a flow of air against the friction,

etc.; this would account for the third power term appearing at this speed. Also, in Fig. 6, no attempt has been made to separate the bearing losses; I think if these were determined accurately at different speeds the results might influence greatly the Kapp-Hausmann diagram Professor Thornton has drawn, because the bearing losses may be found to vary inversely as the speed at low speeds. The determination of windage losses is very important, as they are a large proportion of the total losses in turbine-driven plants. In some cases the windage loss of continuous-current machines, where the speed was high and in which resort was had to powerful ventilation for carrying away the heat, has amounted to as much as 40 per cent of the total losses. In the case of alternators the percentage is considerably lower; but where the speed is high it is often found that the windage loss amounts to as much as 25 or 30 per cent of the total losses of the machine. The loss is, of course, greater where the machine is ventilated by means of fans mounted on the rotor; it is notorious that it is difficult to design such fans to have anything approaching a really high efficiency. **Mr. Davidson.**

One of the 5,000-kw. alternators at Carville station has a windage loss as low as 26 kw. The alternator in question at 5,000 kw. has an overall efficiency, which has been carefully measured and which is known to include all losses, of 96·8 per cent at full load, so that the above-mentioned loss is only 16 per cent of the total losses; but in the case of this alternator the ventilation is produced by means of externally driven fans which consume approximately 17 kw., so that the total ventilation and windage loss amounts to about 27 per cent of the total losses of the alternator. In another case a 500-kw. single-phase alternator was found to have a windage loss amounting to almost 50 per cent of the total losses; but at the same time it was found that the temperature rises were quite moderate. Steps were taken to reduce the quantity of air passing through the rotor, with the result that the windage was halved; and although as a result of this alteration a much smaller quantity of air was passed through the machine, the temperature rises were practically the same as before the alteration, owing to the reduction of windage.

Mr. Turnbull. Mr. C. TURNBULL: The question of small losses in central-station plants is of considerable interest. Thus, a 20-kw. loss in windage may not seem much in a large generator, but it is equal to 160,000 units in a year's running of, say, 8,000 hours. If the small losses were calculated in some large power stations the units lost per annum would be tremendous. Any reduction in these losses may not show up much in the increased efficiency of the generator, but it may still result in an appreciable reduction in the yearly expenses of the station. Has Dr. Thornton tried ball-bearings on his motor? This might throw some light on bearing losses. In America recently a turbine of, I think, 5,000 kw. was scrapped, and instead of breaking it up the Americans set it up in a park as a monument. Now, it has occurred to me that it might be a good thing if we in Britain were to present some of our old

Mr. Turnbull. plant to people like Dr. Thornton for carrying out experiments. Windage and iron losses, evaluated, say, on an old 500-kw. turbo-generator, would be of the greatest interest to electrical engineers; and such experiments might lead to improvements worth many thousands of pounds per year to the nation. A London station recently disposed of old plant at a price which did little more than pay for the cost of advertising it, an incident which is likely to happen many times over in the future. Why not give such plant to our colleges instead of to the scrap-metal merchant?

Mr. Carter. Mr. T. CARTER: Previous speakers have referred more particularly to the application of the author's results to high-speed machines. What I have to say has reference to low-speed machines. One sentence on the first page of the paper may be mentioned as indicating the difference between the application of the principles to high-speed machines and to low-speed machines. In regard to the latter, it really does not matter particularly what the windage coefficient is, provided it is known with some approximation to accuracy. The windage and friction loss in low-speed machines may be only 2 or 3 per cent of the output of the machine, and an error of, say, 10 per cent in estimating this is, therefore, quite negligible. On the other hand, in high-speed machinery, where this loss is a much greater proportion of the total, a similar error would not be by any means negligible. This point may be taken as representing generally some of the important differences between the losses in high-speed machines and those in low-speed machines, and it is quite clear that it is not possible to argue from one to the other. In obtaining the efficiencies of ordinary motors in practice, when these have to be calculated at short notice and must, therefore, be worked out from very simple constants and expressions for the various losses, it seems to be quite correct to take the friction and windage losses as proportional to the speed within the ordinary range of moderate-speed motors. I should like to mention one loss which comes under the heading of those that change with speed, and this loss is not mentioned at all in the paper. I came across its effect in a marked degree first of all in the case of a motor-generator which had to stand considerable overloads. The efficiencies that were guaranteed at the overloads were not obtained, and it appeared that there was a loss which had not been allowed for and which depended on the load on the machine. Nothing could be found in text-books excepting one case where a formula was given which was so complicated that it could not be used. Fortunately I came across the result of an actual series of experiments, and was able to deduce from this a formula which was quite simple in its shape and really seemed to give an indication of the amount of the extra loss that was occurring. By allowing for this in the case of the next similar motor-generator built, the efficiencies came out quite correctly. This loss may be called "incremental loss," but the name does not matter particularly. The principal point is that something occurs which may be due either to eddy currents in the copper of the armature winding, or to extra core

losses arising from the change in distribution of the flux, and the formula gives in a practical form the effect of whatever does happen. That, after all, is the important thing to have in guaranteeing efficiencies. It would be very interesting, however, from the theoretical point of view, and might lead to a proper understanding of the meaning of the formula, if some experiments were conducted in order to find out what the loss really is. This might form at some time or another a useful continuation of the experiments detailed in this paper. The formula for this loss shows that it varies as the square of the speed, and, therefore, it clearly comes under the subject of this paper. Generally speaking, what seems to me to be specially valuable in a paper of this sort is that it is a record of an attempt to separate out the various losses, thereby enabling us to arrive at an intelligent conception of what is going on in the various parts of the machine. Mr. Carter

Mr. J. SCHUIL: I have seen in the *Elektrotechnische Zeitschrift* a report of some tests in which the author had tried to eliminate the air losses by enclosing the rotor in a smooth box, which again was made to run inside another box. The effect was the same as in a centrifugal fan with the air-gates shut. For large machines this could be used without much inconvenience. Mr. Schuil.

Mr. F. O. HUNT: I have been much interested in the application of what Dr. Thornton so neatly describes as the "usages of the Kapp-Hausmann diagram." There have been suggestions put forward to account for the upward curve sometimes found at the top end of the Kapp-Hausmann line, but until now I have never felt that we had arrived at anything like the truth. It has even been laid down in a well-known book that the curvature arises from eddy currents causing excessive armature reaction. As regards the question of skin friction, if we are considering end connectors cutting a column of rapidly moving air, would it not be natural to expect something of the nature of solid friction, seeing that a rapidly moving gas always shows a "quasi-rigidity"? Referring to the section on "Iron Loss Index," may I ask with what degree of accuracy the various kinds of poles were set. A very small difference in the diameter of the bore might make a great change in the performance of the machine. This effect would probably be very important where the ratio of slot width to air-gap is as high as 3 : 5. I should also like to ask whether 800 revs. per minute (page 502) gives the correct position for the bend in the curve. Mr. Hunt.

Mr. W. BAXTER: I should be glad to know what precautions were taken to eliminate any residual field, as otherwise the results could not be accurate. With regard to trapped air, I do not think that any reasonably good designer would be likely to leave pockets in which air could be imprisoned, since one of the chief points in design is to ensure that air circulates through every part of the windings. Curves A, B, and C in Fig. 6 are interesting as showing that there is not a great deal to be gained by carrying the laminations beyond the pole-faces, at least with a ratio of slot opening to air-gap of 3·5 : 1. Incidentally, however, I would mention that a ratio of 3·5 : 1 is Mr. Baxter.

Mr. Baxter. nothing very excessive, as a ratio of 5 : 1 is often found in small crane motors. I feel certain that the "incremental loss" mentioned by Mr. Carter is due to brush losses. As an example of the magnitude to which these losses—due to commutation currents in the brushes—might attain, I would point to the great variations which are observed in the no-load current of a continuous-current motor when the brushes are moved away from the neutral position. Some technical details of the motor which Dr. Thornton has tested would enable designers to compare their results with those given so fully in the paper.

Discussion at Middlesbrough, 8th November, 1912.

Mr. Longman. Mr. R. M. Longman: I should like to know if the machine was run in free air or in the tank, and whether the tank would make any difference. Also, would the vacuum affect the oil in the bearings, and so make a slight change in the bearing friction? I think the article by G. T. Williams in the *Electrician*,* on the dissipation of heat in magnet coils, deserved more attention. In regard to windage loss, a point is soon reached where it becomes of secondary importance to heat dissipation. Machines have to be so designed that while getting the air required through them, care must be taken to avoid eddies or stagnant pockets. The power taken to force air through high-speed machines is considerable—often reaching 100 h.p. in a 5,000-kw. machine—and it is, of course, a windage loss. Such a loss, and the difficulty of making it effective, makes one wonder whether some other method should not be adopted to dissipate the heat, such as water- or oil-cooling the stator, especially in the very large units now being built. In analysing the windage losses of the machine under consideration, has the weight of air actually contained in the revolving portion been considered, and the power required to rotate this at the various speeds? Further, is there also a skin effect where the armature moves through the standing air? As to bearing friction, the machine in the first case was running unexcited and there was no pull due to magnetic unbalancing. There are with many machines, especially high-speed ones, additional bearing troubles, due to extra friction caused by circulating currents in the shaft and bearings. These troubles must be carefully guarded against by insulating a bearing pedestal, or by fixing brushes on the shaft, or in other ways.

Mr. Christianson. Mr. W. A. Christianson: With regard to the speed of flexure for the eddy-current loss, does this usually take place below the normal full speed of a machine or sometimes above it? I take it that it will vary with different machines. I should also like to ask what percentage the windage loss usually represents. No doubt this will vary with the type and size of machine. So long as iron and copper losses take place in machines, the windage loss may be looked upon as a useful loss, with the exception of the wind eddy component. Referring to Fig. 7, the curves are rather instructive. I notice that all three curves are

* Vol. 63, p. 706, 1909.

practically parallel up to the point of flexure. This is not what might have been expected, but, instead, a gradual steepening of the curves as the degree of pole lamination was reduced. This, however, occurs after the point of flexure. From these curves it appears that up to this point the eddy-current loss is the same for all three cases, but that the hysteresis loss varies inversely as the degree of lamination in the poles. The question might then be asked: If there is additional hysteresis loss in the higher curves, why not a corresponding eddy-current loss? The saving in these losses by lamination in the example given would probably not be realized to the same extent in a larger machine, where the ratio of the slot opening to the air-gap was less. It is surprising to note the extent of the hysteresis loss in the poles. The separation of this and the armature hysteresis loss by analysis of the top and bottom curves is interesting. **Mr. Christianson.**

Mr. A. H. W. MARSHALL: I am rather surprised to find that the windage loss is so small a proportion of the total loss. This seems, however, to be borne out by a case I have heard of recently, where the overall efficiency of a medium-sized turbo-alternator was improved some 3 per cent by reducing the diameter of the journals. This goes to prove that friction accounted for a large proportion of the total loss. I notice that the increase in resistance as the speed increases is different from that which is given in most train resistance formulæ. The two cases seem much alike, and I should be glad if Dr. Thornton would explain the difference. **Mr. Marshall.**

Professor THORNTON (*in reply*): I was very much interested to hear what Mr. Law and Mr. Davidson had to say about the great absorption of energy by windage in large high-speed machines; and I note that there is clear evidence of windage loss being proportional to the square of the speed in certain cases. As confirming the result given in the paper this is very satisfactory; but it is more so as showing the possible real gain by shrouding the end connections wherever possible. It would seem that the occurrence of a square or cube index of windage loss could almost be predicted from the type of rotor. Although it is impossible, apart from being undesirable, to run a large machine *in vacuo*, there can be no doubt, as mentioned by Mr. Schuil, that windage loss might occasionally be reduced by entirely enclosing the machine in a tight iron case with cooling fans. There would then be a ventilating current of air absorbing little power, like a centrifugal pump with the discharge valve closed; but whether the cooling would be sufficient could only be found by trial. Mr. Turnbull's figures as to the cumulative cost of small long-continued loss are instructive. The designing of bearings seems to have followed old lines for a long time; the modern tendency is to reduce the diameter and the mechanical losses. Ball bearings in small machines appear to give very excellent results. Mr. Baxter suggests that Mr. Carter's interesting "incremental loss" may be a commutation effect. With brushes in the wrong position an active movement of the magnetism, especially the stray field between the poles, can be observed by means of the **Professor Thornton.**

Professor Thornton.

oscillograph. In alternators there is, as shown some years ago by Professor Threlfall, an increase in iron loss amounting to 7 per cent of the total losses, which appears as the load comes on. Possibly this might be examined by finding all the copper and other eddy losses first, though it must be mentioned that any iron loss in the stator is transmitted through the rotor magnets and may easily be taken as occurring in the rotor unless otherwise examined. The residual magnetism was made exceedingly small, as low as it is possible to get it, before running, and the long-continued vibration of the machine in the earlier experiments would shake out all but the last traces of the permanent flux. Any hysteresis and eddy-current components, if present in the windage experiments, must have been much less than the bearing friction, and would be the same at any speed whether in air or *in vacuo*. The results are based on the difference between the curves of Fig. 3, and the effects of possible residual flux are thereby eliminated.

Mr. Hunt's suggestion, that the loss found in the experiments to vary as the first power of the speed might be a consequence of the dynamic quasi-rigidity of a blast of air, should, I think, be further considered. If the effect be a real one, and not derived from some undetected change of the mechanical friction, it is difficult to see another explanation. In reply to Mr. Longman, there is no measurable difference caused in the power taken by the driving motor by removing the lid of the tank, which would from this appear to have been suitably large for the experiments. I do not quite see how an external vacuum the same at both ends of the bearing could affect the state of the lubrication; the thickness of the film is too small for end effects. In reply to Mr. Christianson, the flexure of the eddy-loss curves occurs in the case of this machine below the normal speed. With regard to the relative values of hysteresis and eddy-current losses in solid and laminated poles, what is put down as hysteresis loss in Fig. 7 should be more properly described as first power loss. Also, since the eddy loss in solid masses varies as the square root of the speed, the only explanation that appears reasonable is that the magnetic movement only becomes turbulent at a critical speed, and below this is lower than might be expected because of the comparatively slow rise and fall of the flux density, this setting up regular eddies with their well-known damping action retarding the growth of the flux.

ADDRESS TO THE STUDENTS' SECTION.

By Dr. A. RUSSELL, Member.

(*Address delivered 20th November*, 1912.)

In your own interests, as well as for the welfare of the industry and the nation, it is highly desirable that you begin to develop the critical faculty. One of the best ways of doing this is to write a paper on a subject in which you are interested, and then read it to this Students' Section.

The next best thing to writing a paper is to criticize one. If possible, let your criticism help the author, but if destructive criticism be called for, do not hesitate to make it. To be offended by honest criticism on these occasions is a sign of weakness. You will find that every sensible engineer not only welcomes criticism and profits by it, but is grateful to his critic.

Some students are engaged in original work, and some are strongly attracted by a particular engineering problem. These students will have no difficulty in choosing a subject for a paper. But there are many, even amongst the best students, whose complaint is that they have nothing to write about. I have thought, therefore, that the best thing I could do to-night was to give a few hints on writing papers for the Institution *Journal*, and to suggest a few fields of research which seem promising at the present moment.

Before making any suggestions, however, a few preliminary warnings may be helpful. First, remember that your reasoning must always be rigorous. You can assume, for instance, that an electron is almost inconceivably small, but that it has a finite amount of energy stored up in it or around it. But you must deduce logical conclusion from your own premises. Simply because the conclusion is correct it does not follow that the reasoning is correct. In many cases hard thinking is necessary in order to detect the fallacy, one has therefore always to be on the watch for loose reasoning. One sees at a glance, for instance, the fallacy of saying, "If all metals are yellow, gold is yellow. But gold is yellow, and therefore all metals are yellow." When put in symbols, however, such reasoning as: "If A is B, X is Y; but X is Y and therefore A is B," is unfortunately not uncommon.

The papers that are wanted are short, clearly written papers in which something novel is brought forward, some old world theoretical shibboleth is criticized, or a succinct account of some recent development or discovery is given. In electrical engineering there are numerous unexplored fields, plenty of crude theories to be exposed, and many developments of which descriptions are wanted.

The old-fashioned fluid theory of electricity survives, and is likely to survive for many years to come in such words as current, pressure, quantity, capacity, etc. I cannot help thinking that the ideas suggested by these words often prevent us from getting a better insight into the physical processes taking place in many electrical phenomena.

I think that everyone ought to have a smattering of the modern atomic theory of electricity. Some may object that this theory is in a continual state of flux, and that they are waiting until its foundations are complete. You cannot afford to do this. For instance, you will find it useful to look on a current of electricity in a solid conductor as a flow of electrons in the interstices between the molecules of the solid conductor. Each of these electrons carries a definite charge, and they are continually colliding with one another and with the molecules of the solid. Applying the ordinary laws of dynamics, we prove at once both Ohm's Law and the formula for the magnetic force near a current, which is generally called Laplace's Law. Those who have a mathematical bent will find the subject entrancing, as it is full of infinite possibilities. The mathematical electrician will find plenty to reward him in Heaviside's new volume. An expanded proof of some of the many theorems given there would form most useful and interesting papers or communications.

Another subject of not less interest, and of more immediate practical importance, is a study of the effects that will be produced when the recent substantial reductions in the price of electricity for heating purposes made by some of the supply companies become general. When electric heating is universal, chimneys for smoke will be relics of barbarism. Black fogs will be unknown, and every house in the city will have a roof garden. Architects are fully alive to the many possibilities of electric heating, but I am afraid that few electricians have seriously studied the problem. The kind of advice sometimes given to consumers is: "Get a radiator and brick up the fireplace." But this leads to great difficulty in obtaining proper ventilation; this point is deserving of the most careful consideration. In this connection, Dr. Shaw's little book on "Air Currents and the Laws of Ventilation" will be found very helpful.

The industrial changes that are being effected by cheap power form another subject which will well repay close study.

In connection with the many systems of radio-telegraphy there is a practically boundless field for mathematicians, physicists, and engineers, and perhaps in no field of research is there a greater demand for skill and trained intelligence of the highest order. Dr. W. H. Eccles has shown that the study of "atmospherics" by means of extremely simple apparatus will not only prove most useful to meteorologists, but may also throw some light on the apparently almost insolvable problem of the earth's magnetism. There is no doubt that some of the signals received are due to disturbances in the sun and other celestial bodies, and so it is most important to notice whether they coincide with magnetic storms.

To pass to humbler problems, I would suggest that there is still much to be discovered by studying the phenomena of brush discharge and the voltages at which these discharges occur on electrified bodies. This Students' Section has already discussed some important papers on the subject. Kemp and Stephens, for instance, made a valuable research on the sparking distances between equal spheres two years ago.* Recent papers by Professor J. B. Whitehead,† of the Johns Hopkins University, and F. W. Peek,‡ of the General Electric Company of America, not only prove that this field is a particularly rich one, but also that nearly all of it is unexplored. The apparatus required for this research on the lines laid down by Whitehead is in practically every laboratory. By means of a simple electroscope he proved that the potential gradient at the surface of an electrified cylinder of radius a when ionization first started was given by the expression $32 + 9{\cdot}5/(a)^{\frac{1}{2}}$ in kilovolts per centimetre when the temperature was 21° C. and the pressure 76 cm.

Peek experimenting on a full-sized model transmission line proved that the maximum potential gradient R between cylindrical wires when the corona first appears is given by $R = 32{\cdot}5 + 11/(a)^{\frac{1}{2}}$ kilovolts per centimetre at 0° C. and 76 cm. pressure, where a is the radius of either wire.

At t° C. and H cm. pressure, R is given by—

$$R = (3{\cdot}6\,H/T)\,[32{\cdot}5 + 11/(a)^{\frac{1}{2}}],$$

where T is the temperature (Kelvin), being the same as $(273 + t)$, where t is the temperature Centigrade.

At 21° C. and 76 cm. pressure this becomes—

$$R = 30 + 10/(a)^{\frac{1}{2}} \text{ kilovolts per centimetre approximately.}$$

Considering the very different methods used by these experimenters the agreement between the results is extremely satisfactory.

Peek distinguishes between the disruptive critical stress and the "visual" stress, *i.e.* the electric stress at which the corona first appears. The difference between these values, however, is very small. He proves also that the corona loss is proportional to the square of the difference between the actual voltage and the critical voltage. These results are obviously of primary importance in the design of high-tension overhead transmission systems.

A theoretical explanation of these formulæ is urgently wanted. By considering the analogy of the convection of heat from a hot cylinder immersed in a liquid, with the convection of electrons from an electrified cylinder, I was led to believe that a formula of the above form would express Whitehead's experimental results. The analogy was vague, and so I was surprised to find how accurately the experimental

* *Journal of the Institution of Electrical Engineers*, vol. 45, p. 685, 1910.

† *Proceedings of the American Institute of Electrical Engineers*, vol. 29, p. 1059, 1910; vol. 30, p. 1079, 1911; vol. 31, p. 8[illegible]9, 1912.

‡ Ibid., vol. 30, p. 1485, 1911; vol. 31, p. 1085, 1912.

results could be expressed by a simple formula of this nature. I shall therefore go a step farther and, using the same analogy, suggest that the maximum stress R between spherical electrodes at the instant of discharge is given by—

$$R = [32{\cdot}5 + 12/(a)^{\frac{1}{3}}]\,[3{\cdot}6\ H/T]$$

when the distance between the spheres is greater than one-twentieth the radius a of either. For given sized spheres R is a constant. I think it will be found that a formula of this nature will enable us to predict the sparking voltages between spheres. The values of the "constants" 32·5 and 12, however, may need a little alteration.

Another interesting research is to make a stroboscopic study of the corona on wires of different metals. If we have two copper wires, for example, attached to the live terminals of a high-tension transformer, each of them appears surrounded by a bluish spray having numerous red spots or beads of light on it. If we look at the wires through the stroboscope, however, we see that the bluish white spray is on the positive wire and the red beads of light are on the negative wire. A careful study of these phenomena will probably bring to light many interesting facts.

Another interesting subject is the grading of cables so as to make them suitable for very high pressures. The most promising method is undoubtedly the conducting layer method. In this method the various layers are separated by thin conducting cylindrical layers, which are maintained at the potentials necessary to produce uniform stress over the whole of the insulating materials. The first thing to find out is what the values of the potential differences between the layers must be in order that this may be effected. The next is to invent methods of maintaining these layers at the required potentials. We then compute what are the capacity currents in the various layers. The latter is an extremely interesting problem which leads to very curious results, setting very definite limits on the use of the method with alternating currents. So far as I know, these results are only published in patent specifications. There are many other most interesting problems in connection with the grading of cables for use on high-tension continuous-current systems.

The favourite subjects for mathematical papers at the present time are in connection with the calculation of coefficients of self and mutual induction. The important lists of these formulæ recently published by the Bureau of Standards are very helpful, and Professor Morgan Brooks of Illinois University has shown that in certain practical cases the computation can be made quite simply. There are many simple theorems which seem to be practically unknown. You are probably not aware that the mutual inductance between a circuit formed of two parallel wires and a circle placed on them is simply 4π times the distance between the wires. It is independent both of the radius of the circle and of the position of its centre. It is certainly a neat theorem and it is very easily proved. A more general case was set as a problem in the

Mathematical Tripos for 1896, and it seems to me to be worth rescuing from oblivion.

A well-known problem discussed by Maxwell and Gauss is the form of the circular coil which has the maximum self-inductance, the length of the wire being fixed. Their conclusion is that the radius of the circular axis ought to be 3·22 times the radius of the cross section. But the formula they give for the self-inductance is about 3 per cent too small. I have recently found that the maximum value of the self-inductance can be computed by the formula $0{\cdot}61\, n^{\frac{1}{3}}\, l^{\frac{5}{3}}$, where l is the length of the insulated wire and n is the number of turns per square centimetre of cross section. So long as the ratio of the radius of the cross section to the radius of the circular axis lies between the values 0·3 and 0·4, the above formula will give the self-inductance with a 1 per cent accuracy. It follows readily that the longer the wire the less the time constant of the coil. This is obviously of importance in the design of reactive coils.

Fritz Emde has recently found that for a given length of wire wrapped as a single layer coil on a cylinder a maximum self-inductance is obtained when the diameter of the cylinder is 2·4525 times the length of the axis of the coil. The maximum value of the self-inductance in this case only varies as $l^{\frac{3}{2}}$, and so the utilization of the wire is not so effective as for a circular coil of circular cross section.

If we assume the electron theory, it is easy to see that the methods of computing self-inductance are at fault, for we neglect the kinetic energy of the electrons. A paper discussing this, and making an estimate of the magnitude of the error introduced by this neglect, would be of value.

The final mathematical subject I shall mention is the method of duality. In geometry the method of duality enables us when we are given a theorem concerning points and lines to deduce a reciprocal theorem concerning lines and points. It follows, for instance, from the theorem that the three diagonals of a hexagon described round a circle meet in a point, that the points of intersection of the opposite sides of any hexagon inscribed in a circle lie on a straight line. An exactly similar method can be applied in technical work. For instance, when Ayrton and Sumpner described the three-voltmeter method of measuring power, Fleming at once invented the three-ammeter method. Dynamos, motors, choking coils, condensers, electromagnetic instruments and methods, etc., reciprocate into motors, dynamos, condensers, choking coils, electrostatic instruments and methods. Series reciprocates into parallel, and star into mesh. Inventors frequently seem to be guided to their discoveries by the conscious or unconscious use of this method.

Another most promising subject is an investigation of the conductivity of dielectrics, when tested with alternating currents of high frequency. I should advise all of you who have not already done so, to study Dr. Fleming's paper on this subject, published in the *Journal* *

* *Journal of the Institution of Electrical Engineers*, vol. 49, p. 323. 1912.

last session. The extraordinary variations in the temperature coefficient of celluloid seem well worth further investigation, both theoretical and experimental. Dr. Fleming's conclusion that at zero Kelvin all good dielectrics have zero conductivity, and all pure metal conductors have infinite conductivity, seems to form a sound basis on which to build a theoretical superstructure.

At the present time it is very necessary to keep an open mind. A hundred years ago the inhabitants of London were delighted by the installation of gas lamps on Westminster Bridge. This feeling has apparently worn off; but he would be rash who would prophecy what kind of illuminant will be used for street lighting a hundred years hence. We are on the eve of discovery of new sources of energy. You will have plenty of work in devising methods for converting them into light and heat with the highest efficiency.

Beware of despising what are apparently only petty laboratory experiments. It is worth remembering that sixteen years ago our President, William Duddell, was only a student of this Institution. His most valuable discoveries—for which last week the Royal Society awarded him a Hughes medal—were made when experimenting in the laboratory of the City and Guilds Central Technical College. His predecessor, Dr. S. Z. de Ferranti, when little more than a boy, was led to the invention of the Ferranti machine by studying by means of a little compass needle the magnetic field round a Siemens machine on which he was working. Cavendish and Coulomb when experimenting with pith balls were doing as important work as any that has been done since.

Our experiments on radio-activity with the help of electroscopes will doubtless seem crude to posterity, but they will envy us the opportunities we possess for widening the boundaries of human knowledge.

Macaulay, in 1835, suggested that mechanical powers might be discovered which would supersede the use of steam. I do not suppose that his words carried much weight with the engineers of the period. Yet petrol engines used in motor omnibuses and aeroplanes would doubtless have astonished them. I am quite certain that if we could see fifty years ahead we would be vastly more astonished at the progress of engineering than Macaulay's contemporaries would have been if they had seen aeroplanes and listened to a voice speaking on the other side of the Atlantic. Engineering advances in a geometrical progression. There are infinite possibilities in the future, and it would be foolish to suppose that engineering is nearing finality.

It is certain that some of you will play an important part in the coming industrial revolution. It is meet, therefore, that you equip yourselves to the best of your ability with those theoretical tools which are becoming more and more necessary, the more refined, the more difficult, the more abstruse the problems become which have to be solved. May you all quit yourselves like men, and with the help of the illuminating torch of science make natural forces more and more the servants of humanity.

// JOURNAL

OF

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Proceedings of the Five Hundred and Forty-ninth Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 13th February, 1913—Mr. W. DUDDELL, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 23rd January, 1913, were taken as read, and confirmed.

Messrs. C. W. Smith and C. C. Hawkins were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows :—

ELECTIONS.

As Members.

Henry Newton Baker.
Francisco Bhering.
William Williams Blunt.

As Associate Members.

Harold Edwin Annett.
Charles Leonard Arnold.
Frank Harrison Barnett.
William Skingley Barter.
Frederic Charles Baumann.
Robert George Beer.
John Bemrose.
Joseph Charles Bentley.
Louis Frank Bickell.
William Thomson Bottomley.
Henry Brown.
John Elliott Brown.
Arthur Horace Cheeseman.
Leonard Owen Cox.
Harold Belbin Fisk.
Hugh Whitmore Franks.
Bertram James Grigsby.
Daniel A. Hackett.
William Gathorne Hardy.
Charles Stewart Jeffrey.
William Kidd.
Guy Hatchard Langdon.

ELECTIONS—*continued.*

As Associate Members—continued.

William John Lee.
Samuel Lees.
Gerald Bruce Lincolne.
James Kenneth MacDougall.
Edgar Moxon.
Ernest Douglas Pearmain.
Cecil Marsden Perrin.
Geoffrey Porter.
Norman Vincent Raven.
Richard Homewood Redman.
George Eli Riley.
Heinrich Rocky.
David Barclay Ross.
Ernest Rothwell.
Robert Sell.
Hermann Sloog.
Harry Berriman Smith.
Arthur Garnet Tucker.
James Alfred Whatnall.
William Smith Wilson.
James Young.

As Associates.

Adam Gowans Whyte.

As Graduates.

Robert Struthers Begg.
Henry Graville Bennett.
Robert Chalmers Black.
Eric James H. Bluett.
Henry Isaac Booth.
Christopher J. Brady.
Edward Tindall Cook.
Edward Edwards.
Stuart Aitchison Laird.
Amar Nath.
George Ollier.
James William Slorach.
Warren Storey.
Edmund Ernest Walker.
William Blackmore Ward.
William John T. Williams.
Arthur Noel Glover Wood.

As Students.

Alfred Thomas Bradley Betts.
Vernon Hazzard Gutteridge.
James Mark Heslop.
Arthur Stafford Kettle.

TRANSFERS.

From the class of Associate Members to that of Members :—

Arthur Hinton Allen.
Robert Harry Campion.
George Pollard Dennis.
Edgar Isaac Everett.
William Fennell.
Arthur William Fithian.
Edward Graham Fleming.
Percy Good.
Alfred Henry Irvine Graham.
Selwyn Seafield Grant.
Adolf William Isenthal.
Francis Maddison Long.
Henry Ashley Madge.
Harry Bryant Matthews.
John Frederick Nielson.
John Digby Pember.
Harry Leonard Percy.
William Griegson Pickvance.
Hugh Innes Rogers.
Edward Churchill St. John.
John Severs.
Cyril Morley Shaw.
William Bolton Shaw.
Ernest John Summerhill.

From the class of Associates to that of Members :—

Rowland Edward Dixon.
Arthur Edward Hadley.
Henry James Nisbet.
Ganendro Prosad Roy.
Harry Scholey.
Robert Ellis Shawcross.

From the class of Associates to that of Associate Members :—

Montague James Allward.
William James Rendle Baker.
Sydney Dale.
John Alfred Edmondson.
Henry Okazaki Fleetwood.
Walter Graham Hodgson.
Henry Stockdale Ingleby.
Ernest James Marsh.
Arthur Marston.
John Frederick North.
Abraham Denby Raine.
Albert Edward Salisbury
Robinett Scruby.
Claude Edward Vance.
Charles Jebb Vaughan.
James Young.

From the class of Students to that of Associate Members :—

Rex Thomas Challoner.
Reginald Woolton Fowler.
William Rolande Harding.
Henry Moore Harrison, B.Sc. (Tech.).
Hugh Melbourne Hart.
William Eccles Jones.
Percy William Scholefield.
Edgar Harold Turle.

From the class of Students to that of Graduates :—

Meer Laig Ali.
John Raldolph Boyer.
Arthur Frederick N. Chandler.
Ralph Larkworthy.
John Bernard Murray.
Frederick George Ride.
Antonio Rodrigues Teixeira.
Roger Beverley Walker.

A paper by Mr. A. R. Everest, Member, entitled "Some Factors in the Parallel Operation of Alternators" (see page 520), was read and discussed, and the meeting adjourned at 9.10 p.m.

SOME FACTORS IN THE PARALLEL OPERATION OF ALTERNATORS.

By A. R. EVEREST, Member.

(*Paper first received 12th December*, 1912, *and in final form 15th January*, 1913; *read before* THE INSTITUTION 13*th February, and before the* BIRMINGHAM LOCAL SECTION 12*th February*, 1913.)

In connection with the parallel operation of alternators it is well known that serious trouble from "hunting" may occur unless the system has such characteristics as to prevent the natural oscillating frequency from approaching resonance with the frequency of the engine impulse.

Many treatises have been written upon the subject, giving formulæ for determining the natural frequency from the alternator characteristics and a given amount of flywheel effect, or conversely to determine the amount of flywheel needed to give the system a predetermined natural frequency. Unfortunately, however, the results obtained in actual practice often differ seriously from those indicated by the formulæ. Attention has been called to these discrepancies by various writers at different times, showing that they are due to the neglect of some important factors; but it is believed that no practical treatise including the needed correction exists at present, at least in the English language.

These notes aim to show clearly the nature and importance of the correction necessary, and a means of applying it. Some practical cases are given in illustration. Notes are added upon the effect of load, the calculation of combinations of dissimilar sets, and the general specification of requirements to be met by the flywheel.

THE NATURAL OSCILLATING FREQUENCY.

Formulæ for calculating the natural oscillating frequency of an alternator as one of a similar pair, or as a single machine connected to a system of indefinitely large capacity, have been published many times.

All methods are based on the well-known equation for oscillatory motion :—

$$T = 2\pi\sqrt{\frac{\text{displacement}}{\text{acceleration}}},$$

where T is the time in seconds for one complete oscillation.

This in the case of an alternator may be conveniently expressed in the form—

$$f_o = 9{\cdot}76\sqrt{\frac{K_w \times \text{cycles}}{\text{foot-tons}}},$$

where—

f_o = the frequency of the oscillations per minute ;
foot-tons = the stored energy of the revolving parts at normal speed ;
K_w = the kilowatts of synchronizing power corresponding to one radian (electrical) of displacement from the mean position of uniform rotation.

The derivation of this equation is given in the Appendix. It is

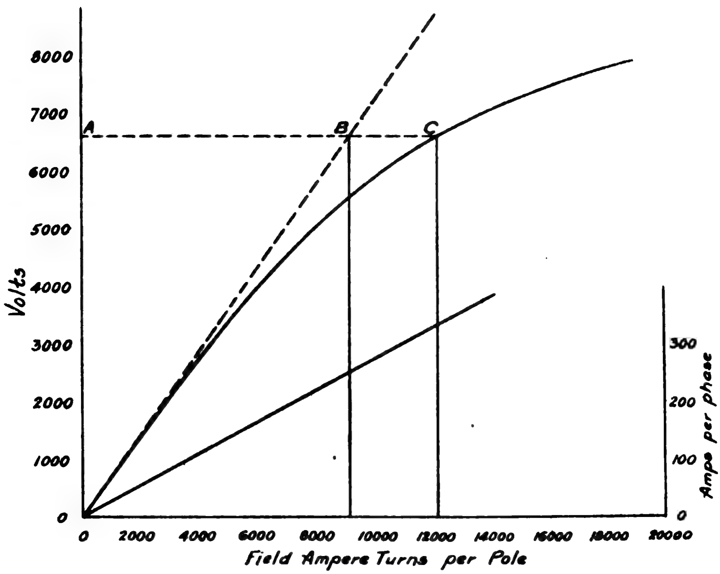

FIG. 1.

important to note that in whatever form the equation for oscillating frequency may be expressed it always contains a term corresponding to K_w, the synchronizing power proportional to unit angular displacement from the mean position of uniform rotation.

Fig. 2 represents the E.M.F. triangle for two machines when dephased. It is readily seen that the cross-voltage or impedance E.M.F. consumed in one machine (C A) for a small angle of deviation from mean position is proportional to the radius voltage for one radian of deviation. The value of K_w is therefore determined by the amount of current which flows as cross-current to consume in one machine a cross-voltage equal to the working voltage.

It is in the proper determination of the K_w value that errors arise. The usual methods indicate that this shall be determined from the

current which will circulate in the machine on short-circuit with excitation corresponding to the air-gap excitation for no-load working voltage.

(Some writers employing the method just described insert the full ampere-turns of open-circuit excitation instead of the ampere-turns required at the air-gap. This is obviously wrong. In Fig. 1, which is a typical curve of open-circuit excitation, only the part A B represents the magnetizing force at the air-gap; the remainder, B C, being consumed in saturating the magnet poles, should therefore not enter into the short-circuit test where saturation is absent. The effect of this discrepancy will be referred to later.)

But in the case of power exchange current between two machines, the amount of current flowing due to a given cross voltage is much larger than when consuming the same voltage on short-circuit, hence the synchronizing power as determined from the short-circuit test is usually far too low.

With the small angles of deviation which occur while "hunting" in parallel operation, the cross voltage to which the exchange current is due is sensibly at right angles to the machine voltage. The circuit through two machines being highly inductive, the circulating current

Ratio $\frac{\text{pole arc}}{\text{pole pitch}}$.	Ratio $\frac{\text{cross reactance}}{\text{opposing reactance}}$.	Reciprocal.
1·0	1·00	1·00
0·9	0·73	1·37
0·8	0·51	1·97
0·7	0·35	2·85

lags nearly 90° behind the cross voltage which produces it; thus on the short-circuit test, or with two machines 180° apart, the circulating current is dephased 90° from the machine voltage; it exerts a direct demagnetizing effect upon the field poles, and produces no synchronizing power.

But when the cross voltage is at right angles to the machine voltage, the resultant cross current is in phase with the machine voltage; therefore it is true power current, and as a non-inductive current exerts distorting effect upon the field flux, instead of direct demagnetization.

The reluctance of the cross magnetization path is usually quite different from that of direct demagnetization, and the current flowing to wipe out a given amount of cross voltage will often be two or three times as much as when cancelling the same amount of voltage by direct demagnetization of the main poles. In other words, the real synchronous impedance which is active when "hunting" may differ widely in value from that measured on short-circuit tests; and the

latter quantity cannot give the correct value for synchronizing current. This effect was pointed out by Goldschmidt * as far back as 1902. He gave the correction factors shown in the table on p. 522.

Hobart and Punga in 1904 † gave a practical treatise on the analysis of these distortions.

Punga has more recently ‡ given results of actual tests, pointing out the large errors resulting from neglect of this feature.

Schüler has given correction factors § which he found necessary to bring calculations by the ordinary method into agreement with observed results.

Rezelman‖ has discussed the same subject, including the modifying effects of saturation.

In order to determine the proper value of synchronizing power

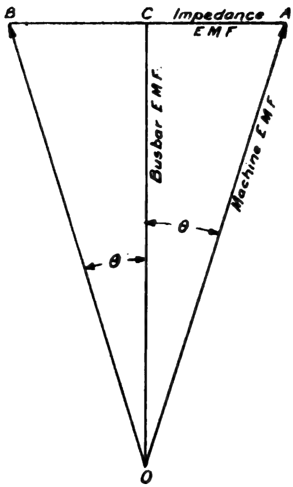

FIG. 2.

(K_w) to be inserted in the equation for oscillating frequency, we may therefore employ the value obtained from the short-circuit test multiplied by a factor depending principally on the shape of the poles.

In Fig. 2, O A, O B represent the open circuit E.M.F.'s of two machines which are dephased, one behind and one ahead of the mean position by the angle θ. If the machines are in parallel, O C represents the mean or busbar E.M.F., and it is assumed that cross current will flow between the machines sufficient to consume the cross E.M.F. A C or C B in the synchronous impedance of one machine.

* *Elektrotechnische Zeitschrift*, vol. 23, p. 980, 1902.
† *Transactions of the American Institute of Electrical Engineers*, vol. 23, p. 291, 1904.
‡ *Elektrotechnische Zeitschrift*, vol. 32, p. 385, 1911.
§ Ibid., vol. 32, p. 1199, 1911.
‖ *Lumière Électrique*, vol. 15, p. 67, 1911.

But a method based on Hobart and Punga's analysis appears preferable.

In Fig. 3 the angle A O C of mechanical displacement is divided into two parts, and ϕ is the amount by which the flux axis is displaced from the pole axis by distortion due to the cross current flowing. The actual E.M.F. triangle is D O C (for one machine), and the real cross voltage D C is consumed as reactive drop in the machine winding.

The angle of mechanical displacement corresponding to any assumed value of cross current, for instance, normal full-load current, is readily found. The angle D O C is given at once by the reactive drop, and we find the angle of flux distortion for any type of pole by the table given below.

$$\text{Distortion angle} = \frac{\text{armature ampere-turns per pole, all phases}}{\text{field ampere-turns per pole at air-gap}} \times K.$$

Ratio $\frac{\text{pole arc}}{\text{pole pitch}}$.	K in Degrees.
0·4	7·0
0·5	10·0
0·6	13·5
0·7	18·0
0·8	24·0
0·9	31·0
1·0	40·0

NOTE.—When the air-gap increases in depth from the centre towards the edges the effect is further to decrease the effective width of the pole arc. For such cases a correction of 10 per cent is suggested.

These two angles together give the displacement angle for one machine at the chosen value of cross current, and hence the cross current and synchronizing kilowatts proportional to one radian displacement are at once known.

The values indicated for flux distortion in this table are derived from Hobart and Punga's analysis referred to above, and are believed to be in good agreement with the relations determined by other writers upon the subject of armature reaction. The references given will assist those who wish to obtain greater accuracy in the case of high saturation or other special features.

In the case where the field magnet is a continuous cylinder with

pole arc equal to pole pitch it will be found that this method gives substantially the same result as the short-circuit method. As an instance, let the total armature ampere-turns per pole be 60 per cent of the field ampere-turns expended at the air-gap. From the table the distortion angle is $0{\cdot}6 \times 40 = 24°$. The short-circuit test, taking as usual the E.M.F. of armature reaction to be $0{\cdot}707$ of the ampere-turns, would give (if winding reactance were negligible) an impedance E.M.F. $= 0{\cdot}6 \times 0{\cdot}707 = 42{\cdot}4$ per cent of the open-circuit voltage, which as tangent corresponds to an angle of $23°$ in Fig. 2.

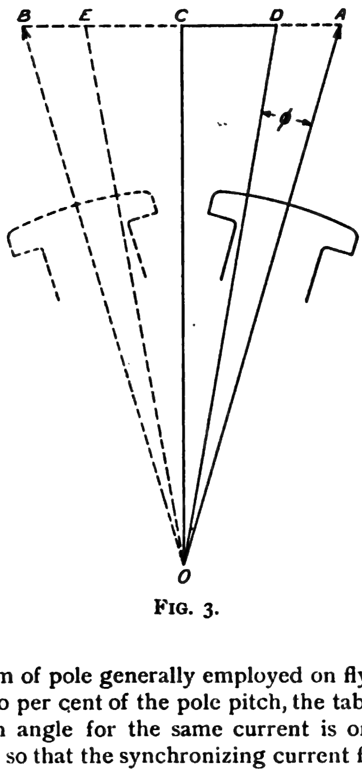

FIG. 3.

But for the form of pole generally employed on flywheel alternators, if the pole arc is 60 per cent of the pole pitch, the table on p. 524 shows that the distortion angle for the same current is only one-third that in the former case, so that the synchronizing current flowing for a given angle of displacement would be very much larger than indicated by the short-circuit method.

The value of winding reactance employed in this method should, as suggested by Hobart, include only that part which lies within the armature surface, and must not include that part due to reactance flux which crosses the air-gap and passes along the pole face, since this portion is taken into account in the flux distortion. In general, it is sufficiently accurate to take a value of winding reactance E.M.F. one-half that determined in the usual way.

It may be mentioned here that wherever armature ampere-turns are referred to in this paper the "effective value" as corrected for coil pitch and distribution factors should be understood.

Some practical applications will best show the inaccuracy of the ordinary method :—

Case 1.—1,500-kw. machine with constants shown below. The oscillating frequency calculated by the short-circuit method was 40·5 per minute. The "distortion" method indicated a natural frequency of 68.

The frequency by actual test was 68. This test was made by throwing two similar machines together while slightly out of phase, and timing the oscillations of the instrument needles.

Machine, 1,500-kw., 2-phase, 50 cycles.
Volts, 6,600.
Amperes per phase for 1,500 k.v.a. = 114.
Armature ampere-turns per pole (effective), 4,940.
Armature reaction, 3,500.
Field ampere-turns on short-circuit, 4,150.
Reactance volts (deduced), 7·2 per cent.
Total flywheel effect at normal speed = 9,600 foot-tons.

Fig. 1 shows the open-circuit and short-circuit tests on this machine.

Short-circuit Method.—

Open-circuit excitation at air-gap ... ... 9,200.
Short-circuit excitation for 1,500 k.v.a. ... ... 4,150.

Value of K_w ... ... $1500 \times \frac{9200}{4150} = 3{,}330.$

Oscillating frequency $= 9{\cdot}76\sqrt{\frac{3330 \times 50}{9600}} = 40{\cdot}5$ per minute.

"*Distortion*" *Method.*—

$$\frac{\text{Pole arc}}{\text{Pole pitch}} = 65 \text{ per cent.}$$

(Gap was twice as wide at pole edges as at centre, therefore equivalent pole ratio = 60 per cent.)

For 114 amperes per phase = 1,500 k.v.a.

Reactance volts $\frac{7{\cdot}2}{2} = 3{\cdot}6$ per cent.

Corresponding angle = 2·0 degrees.

Distortion angle $\frac{4940}{9200} \times 13{\cdot}5 = 7{\cdot}2$ degrees.

Displacement angle 9·2 degrees.

K_w proportional to one radian displacement—

$$1500 \times \frac{57{\cdot}3}{9{\cdot}2} = 9{,}300\ K_w.$$

Natural frequency—

$$9{\cdot}76\sqrt{\frac{9300 \times 50}{9600}} = 68 \text{ per minute.}$$

NOTE.—Without the correction for variable air-gap this works out to a frequency of 63.

Case 2.—Another interesting case which occurred some time ago was that of two large low-speed sets designed for close regulation, and operating at 94 revs. per minute. The sets were intended to operate around 2,100 volts, but while fairly steady at 2,200 volts could not (in their original condition) be operated together between 2,000 and 1,700 volts, due to the excessive hunting which occurred within this range, indicating resonance between the natural frequency and the engine speed at about 1,800 volts.

Experiments were made in raising the poles to reduce the air-gap excitation, cutting out part of the stator windings, and inserting reactance between the machines.

Short-circuit and open-circuit characteristics were determined for each of the conditions (except the last). Below are shown the natural frequencies calculated by the short-circuit method, and by the "distortion" method, for the observed critical voltage for each condition.

Condition.	Approximate Critical Voltage.	Calculated Natural Frequency at Critical Voltage.	
		By Short-circuit.	By "Distortion."
(*a*) Original	1,800	68	96
(*b*) Gap reduced on one machine ...	1,900	65	97
(*c*) One machine gap reduced and $\frac{1}{8}$ of stator turns cut out... ...	1,800	65	95
(*d*) As (*b*) with $\frac{1}{8}$ of stator turns cut out on both machines ...	1,650	61	90
(*e*) As (*a*) with reactance between machines ...	2,200 (?)	—	—

Calculated by the short-circuit method, the flywheel actually present should have given at 2,200 volts a natural frequency of 74, thus safely below the engine speed, and with increasing margin for lower voltages. By the "distortion" method the natural frequency at 2,200 volts is 111 per minute, and approaches still nearer to the engine speed as the voltage is reduced. Actually, as we see, the operation becomes worse with

changes which are known to reduce the natural frequency, such as reducing the operating voltage, reducing the air-gap, or adding reactance, while the stability was increased by reducing the active turns of the stator winding so as to increase the frequency. All this shows that the original natural frequency was above the engine speed, as the "distortion" method indicates.

It may be here mentioned that if calculated by the short-circuit method, taking not only the ampere-turns at the air-gap but also those absorbed in saturating the pole as all available in producing 2,200 volts at the air-gap, a frequency of 91 instead of 68 is obtained. The fact that a near approach to observed results is obtained by this means, no doubt accounts for the inclusion of these ampere-turns lost in saturation by some who still advocate the short-circuit method. Several other records of actual tests showing the inaccuracy of the short-circuit method will be found in the references previously given.

The Effect of Load.

So far the no-load conditions only have been considered. It is important to know how the results are modified when operating under load.

Some writers have advised increasing the short-circuit ratio in proportion to the increased excitation under load. Some, on the other hand, have claimed to find no observable difference in the natural frequency due to the presence of load.

Schüler, in the paper referred to, advised increasing the ratio by an amount equal to the increased excitation for *non-inductive* load, and suggested 10 per cent increase in the assumed field strength.

Görges * and Boucherot † indicate that for any load condition the natural frequency should be calculated, not on the terminal voltage, but on the higher voltage which must be generated internally to overcome the impedance drop of the machine windings. This method would appear to be correct.

Dissimilar Machines.

The discussion so far has been limited to the case of two similar machines, or one machine connected to a system of indefinitely large capacity. It more often happens that a new machine is required to operate with one of older type. The new machine may be a turbo-alternator, which while introducing no periodic disturbance yet might introduce such constants into the system as to make it approach resonance with an existing steam engine.

The following method of treatment has been found useful :—

In the case of two similar machines, A and B, let Z represent the synchronous impedance for non-inductive current in each machine. If the electrical constants of machine B are now so changed that

* *Elektrotechnische Zeitschrift*, vol. 21, p. 188, 1900, and vol. 23, p. 1053, 1903.

† *Bulletin de la Société Internationale des Électriciens*, vol. 4, p. 495, 1904.

operating as one of a similar pair the synchronizing power is halved, then $Z_b = 2\ Z_a$.

With A and the changed B machine operated together, the synchronizing power corresponding to any displacement angle will be two-thirds that of the original pair.

In general, and writing K_a, K_b, for the K_w values of A and B respectively, each calculated as one of a similar pair, then the resultant value for machines A and B operating together is—

$$= 2\frac{K_a K_b}{K_a + K_b},$$

which expression may be substituted for K_w in the numerator of the equation for natural frequency.

Similarly with two unequal flywheel effects the resultant value may be expressed in the form—

$$2\frac{ab}{a+b}.$$

For, starting as before with a similar pair, and leaving electrical constants unchanged, let the foot-tons of B machine be doubled. The synchronizing power being equal in both machines at any instant, B machine will now only swing half as far as A in the given time.

When machine A has swung through a given angle θ, the total separation angle is now only $1{\cdot}5\ \theta$ instead of $2\ \theta$. The resulting cross voltage, and therefore synchronizing current, is diminished in the same proportion, the effect being equivalent to an increase in "foot-tons" to four-thirds the original value.

Writing T_a, T_b, for the foot-tons of flywheel effect in machines A and B, the resultant effect is—

$$2\frac{T_a T_b}{T_a + T_b}.$$

For two machines with any values of electrical constants and flywheel effect, we then have the natural frequency—

$$= 9{\cdot}76 \sqrt{\text{cycles} \times \frac{K_a K_b (T_a + T_b)}{T_a T_b (K_a + K_b)}}.$$

Flywheel Requirements.

When specifying the flywheel to be supplied in connection with an engine-driven alternator it is usual to stipulate that the total amount of flywheel effect present must be such as to satisfy these two conditions :—

(*a*) The angular deviation from uniform rotation due to cyclic irregularity of the engine must not exceed $2\frac{1}{2}$ electrical degrees (shaft degrees multiplied by the number of pairs of poles). An expression in

terms of cyclic speed irregularity is usually preferred by the engine-makers. The following is a convenient expression :—

Permissible limit of cyclic speed irregularity $= \frac{1}{a}$, where—

$$\frac{1}{a} = \frac{k}{6 \times (\text{number of poles})}.$$

k is the number of engine impulses per revolution. (The derivation of this equation is given in the appendix.) But in no case should the irregularity be worse than 1/150.

(*b*) To avoid trouble from resonant hunting sufficient flywheel effect should be provided to ensure that the natural frequency of the machine shall be at least 20 per cent different from the frequency of the predominating engine impulse. The predominating impulse in a 2- or 4-stroke cycle engine always occurs with the frequency of the cam-shaft, and is independent of the total number of cylinders contributing to the turning moment diagram. Thus in a 2-cycle engine running at 200 revs. per minute the flywheel would be designed to give a natural frequency below 160, but in a 4-cycle engine running at 200 revs. per minute the cam-shaft speed being 100 revs. per minute, it would be necessary to design the flywheel to keep within a natural frequency of 80 per minute.

For operating alternating-current generators of ordinary regulation, the total amount of flywheel effect required for each kilowatt of rating may be indicated by the following formula—

The flywheel effect in foot-tons of stored energy at normal speed

$$= \frac{1{\cdot}3 \times \text{poles} \times (\text{strokes per engine cycle})^2}{\text{r.p.m.}}.$$

APPENDIX.

1. Cyclic Speed Irregularity.

Let $\frac{1}{a}$ represent the cyclic speed irregularity,

$$= \frac{\text{max.–min.}}{\text{mean}} \text{ speed},$$

$n =$ revs. per second,

$k =$ impulses per revolution,

$n\,k =$ frequency of engine impulses per second.

Mean speed $\frac{\text{radians}}{\text{second}} = 2\,\pi\,n.$

Maximum excess speed $= 2\,\pi\,n \times \frac{1}{2a} = \frac{\pi\,n}{a}.$

Mean excess speed $\frac{2}{\pi} \times \frac{\pi n}{a} = \frac{2n}{a}$.

This in time of $\frac{1}{4}$ engine cycle $\frac{1}{4nk}$ gives maximum displacement—

$$= \frac{2n}{a} \times \frac{1}{4nk} = \frac{1}{2ak} \text{ in shaft radians.}$$

This in electrical degrees—

$$= \frac{1}{2ak} \times \frac{57{\cdot}3\,p}{2} \quad (p = \text{No. of poles}).$$

$$\text{Degrees displacement} = \frac{14{\cdot}3\,p}{ak}.$$

$$\frac{1}{a} = \frac{k \times \text{degrees displacement}}{14{\cdot}3 \times \text{poles}}.$$

For 2·4 degrees displacement—

$$\frac{1}{a} = \frac{k}{6 \times \text{poles}}.$$

2. Flywheel Effect Required per Kilowatt.

$$= \frac{1{\cdot}3 \times \text{poles} \times (\text{strokes per engine cycle})^2}{\text{r.p.m.}}.$$

Derivation.—Fundamental simple harmonic equation—

$$T = 2\pi\sqrt{\frac{\text{displacement}}{\text{acceleration}}} \quad \ldots\ldots\ldots\ldots \quad (1)$$

This may be expressed in the form—

$$f_o = 9{\cdot}76\sqrt{\frac{K_w \times \text{cycles}}{\text{foot-tons}}} \quad \ldots\ldots\ldots\ldots \quad (2)$$

or—

$$f_o = 9{\cdot}76\sqrt{\frac{K \times \text{cycles}}{\text{foot-tons per kilowatt}}} \quad \ldots\ldots \quad (3)$$

where f_o is the oscillating frequency per minute, K_w the synchronizing power proportional to unit displacement from mean position of uniform rotation for one machine, and K is the K_w value divided by the kilowatt rating of the machine.

Again, f_o is not to exceed 80 per cent of the engine impulse frequency—

$$f_o = \frac{0{\cdot}8 \times (\text{r.p.m.}) \times 2}{\text{strokes per engine cycle}} \quad \ldots\ldots\ldots \quad (4)$$

$$(f_o)^2 = \frac{2{\cdot}56 \times (\text{r.p.m.})^2}{\text{strokes}^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

From (3) it is also—

$$= \frac{95 \times \text{K} \times \text{cycles}}{\text{foot-tons per kilowatt}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Combining (5) and (6)—

$$\text{foot-tons per kilowatt} = \frac{95 \times \text{K} \times \text{cycles} \times \text{strokes}^2}{2{\cdot}56 \times (\text{r.p.m.})^2}$$

$$= \frac{37 \times \text{K} \times \text{cycles} \times \text{strokes}^2}{(\text{r.p.m.})^2} \quad . \quad . \quad . \quad (7)$$

or, substituting poles for cycles—

$$\text{cycles} = \frac{(\text{r.p.m.}) \times \text{poles}}{120}$$

$$\text{foot-tons per kilowatt} = \frac{37 \times \text{K} \times (\text{r.p.m.}) \times \text{poles} \times \text{strokes}^2}{120 \times (\text{r.p.m.})^2} \quad (8)$$

$$= \frac{0{\cdot}308 \times \text{K} \times \text{poles} \times \text{strokes}^2}{(\text{r.p.m.})} \quad . \quad . \quad . \quad (9)$$

Taking $\text{K} = 4{\cdot}2$ at full load for a machine of ordinary regulation (6 per cent to 7 per cent at unity power factor) we get—

foot-tons per kilowatt of generator rating

$$= \frac{1{\cdot}3 \times \text{poles} \times (\text{strokes per engine cycle})^2}{(\text{r.p.m.})} \quad . \quad . \quad . \quad (10)$$

NOTE.—The generator rating is given in kilowatts as being more convenient to the engine-builders, but with the assumption that the generator is designed to carry its rated kilowatts at 0·8 power-factor; the kilowatt rating is therefore 80 per cent of the k.v.a. rating.

3. Derivation of the Equation for Oscillating Frequency.

$$\text{Oscillations per second} = \frac{1}{2\pi}\sqrt{\frac{\text{acceleration}}{\text{displacement}}} \quad . \quad . \quad . \quad (1)$$

Let $\frac{p}{2}$ = number of pairs of poles, then for any angle θ of displacement of the flywheel from the mean position of uniform rotation, the electrical displacement angle is $= \frac{p\,\theta}{2}$.

Let $\text{K}\omega$ = the synchronizing power in kilowatts corresponding to unit angle of electrical displacement, then for a displacement angle θ of the flywheel the accelerating power is—

$$\text{K}\omega \frac{p\,\theta}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This in pounds force at 1 ft. radius becomes—

$$= \frac{K\omega \times 737 \times 60}{2\pi N} \times \frac{p\theta}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where N = r.p.m.

Since acceleration $= \frac{\text{accelerating force}}{\text{moment of inertia}}$,

and the moment of inertia of the rotating parts $= \frac{\text{lb.-ft.}^2}{32}$. (4)

$$\text{the acceleration} = \frac{K\omega \times 737 \times 60 \times 32 \times p\theta}{2\pi N \times \text{lb.-ft.}^2 \times 2} \quad . \quad . \quad . \quad (5)$$

Also since the electrical frequency ($\sim$) $= \frac{N}{60} \times \frac{\text{poles}}{2}$, the acclera-tion may be written—

$$\frac{K\omega \times 737 \times 60 \times 32 \times p\theta}{2\pi N \times \text{lb.-ft.}^2 \times 2} \times \frac{\sim \times 60 \times 2}{Np} \quad . \quad . \quad . \quad . \quad (6)$$

This becomes—

$$\text{acceleration} = \frac{K\omega \sim 13{\cdot}5 \times 10^6 \theta}{N^2 \times \text{lb.-ft.}^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

From (1) the oscillating frequency per minute

$$f_o = \frac{60}{2\pi}\sqrt{\frac{\text{acceleration}}{\theta}},$$

substituting the value for acceleration from (7), this becomes—

$$f_o = \frac{60}{2\pi}\sqrt{\frac{K \sim 13{\cdot}5 \times 10^6 \theta}{N^2 \,\text{lb.-ft.}^2 \theta}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$= \frac{60}{2\pi} \times \frac{3680}{N}\sqrt{\frac{K\omega \times \sim}{\text{lb.-ft.}^2}}$$

$$= \frac{35300}{N}\sqrt{\frac{K\omega \times \sim}{\text{lb.-ft.}^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

For general use it is preferable to express the flywheel effect in terms of stored energy at normal speed, eliminating the actual r.p.m. and the number of poles.

The stored energy of the revolving parts is given by—

$$\text{Foot-tons} = \frac{\text{lb.-ft.}^2}{2 \times 32} \times \left(\frac{2\pi N}{60}\right)^2 \times \frac{1}{2240}$$

$$= \text{lb.-ft.}^2 \times N^2 \times 7{\cdot}65 \times 10^{-8} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

From (9) and (10), therefore, the oscillating frequency per minute

$$= 9{\cdot}76\sqrt{\frac{K\omega \times \sim}{\text{Foot-tons}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Discussion.

Professor Walker.

Professor MILES WALKER: This subject is assuming more and more importance every day now that the Diesel engine has given a new lease of life to the engine-driven alternator, a machine which had somewhat waned owing to the popularity of the turbo-generator. We may in the future often have to use formulæ of the kind given in this paper, and it is well to know those which give somewhat closer results than the crude formula hitherto in use. We must not suppose, however, that the old formula was so very far from accurate, because as a matter of fact in the way in which it was applied it used to give not such very bad results. The author has said that it is not right to take the ampere-turns upon the pole face when considering this question, but only to consider the ampere-turns impressed upon the air-gap. That is quite correct so long as we have no saturation in the pole face or in the teeth of the armature; but where there is saturation in the pole face—many turbo-generators have a considerable amount of saturation there; but I do not suppose the author had those in view, his remarks applying more particularly to the engine type of machine—then the ampere-turns on the teeth or the pole face are of just as great importance as the ampere-turns on the air-gap. We ought therefore to take a rather higher value than the ampere-turns on the gap. I am sure that most of the designers who used the old formula, based on the short-circuit current, knew that it was not exact. They were aware that it was only a first approximation, and they knew that the synchronizing current would actually be rather greater than the value obtained by the simple calculation. For that reason, as a sort of compromise, they took the whole ampere-turns on the pole, as that gave a higher value. It was a rather crude method, but it gave good results. As a matter of fact, the old formula has been of very great service to designers in specifying flywheels. We do not try to ascertain which particular flywheel will resonate; all we want is to design a flywheel that will not resonate, and we therefore try to get as far away as we can from the danger-point. For that reason it was not very necessary to know *exactly* where the danger-point lay. The author's paper enables us to find out more accurately the position of the danger-point. His formula will also be of great service in connection with the design of rotary converters. I am quite sure that a great deal of the bad reputation of high-frequency rotary converters in the past was due to resonance with some disturbance having the frequency of the supply. In the old days, when everybody thought that all that was necessary in order to design a rotary was to fit slip-rings to a continuous-current armature, it was only natural that machines should have been put upon the market which often resonated with cyclical disturbances on the system. This was one cause, though of course not the only cause, of hunting with these early rotary converters.

It may be of interest to show a tachograph record taken with a 60-cycle rotary converter whose natural period of swing happened to

Professor Walker.

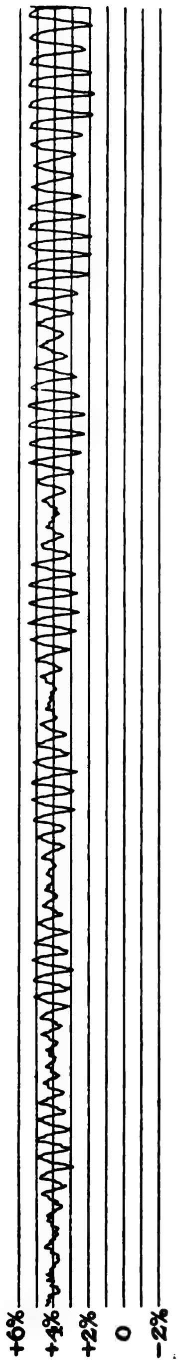

FIG. A.—Tachograph Record of Badly Designed Rotary Converter, the Natural Period of Phase-swing being nearly the same as the Period of the Disturbance.

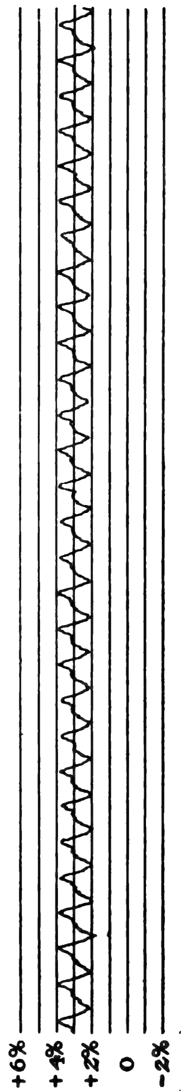

FIG. B.—Tachograph Record showing a Disturbance having 75 Periods per Minute.

Professor Walker.

be almost the same as the period of a disturbance on the engine. Fig. A shows the oscillating change of speed of a 60-cycle rotary converter when switched on to the supply. It will be seen that the converter first gets into step with the disturbance, then goes out of step, then into step again for a rather long period, then out of step again, and finally it breaks step and stops altogether. This matter is of great importance commercially, because if we are to avoid that kind of behaviour in rotary converters it is necessary to inquire very closely into what are the disturbances in a circuit, and then we must apply the author's formula so as to find out what constants we should give to the machine in order to avoid resonance with any disturbances that are known to exist. An interesting case arose a short time ago where it was necessary to build a 1,000-kw. rotary converter to deliver 5,000 amperes. This necessitated a machine with a large number of poles—a machine which one would expect to be rather sensitive to resonance with a low-frequency disturbance. In that case the precaution was taken of finding out beforehand what was the frequency of the disturbance. A tachograph was put upon the engine supplying power to the circuit, and a comparatively small disturbance having a frequency of 115 cycles per minute was observed. Under certain other circumstances, with another engine running, there was a rather larger angle of variation, the frequency being 75 cycles per minute (Fig. B). It was necessary, therefore, to design a rotary converter which would not resonate either with 75 or with 115 cycles per minute. When an ordinary commercial machine was calculated out, it was found that the machine would have a natural period of phase-swing of about 75, so that if this investigation had not been made the machine was doomed to failure. It was also intended to change the continuous-current voltage of the machine by varying its excitation and using a reactance in series with the alternating-current side. That of course would have made a considerable change in the frequency of resonance, and even with considerable change in the design of the machine some value of the excitation would almost certainly have been found where the resonance would have become hopeless. What was done was to change the method of varying the voltage. An alternating-current booster was fitted upon the rotary converter so as to bring the rotary's excitation completely under the control of the operator, and independent of the supply voltage; then the air-gap was changed to raise the period of phase swing. We found it would be possible to make the natural period of vibration about 94 oscillations per minute, which would come fairly well between the 75 and the 115, and the result has shown that it is perfectly satisfactory. The rotary converter runs well and commutates well. The speed of course changes with the speed of the engine. Every good synchronous machine ought to keep in synchronism whatever the variation of speed may be, and it is not a fault of the machine if it goes in jerks when the engine goes in jerks. The point is that we must not have resonance there so as to make the swinging worse than it ought to be. Any rules therefore that the author has given us to

ascertain more accurately by calculation the position of the danger-point are really of very great importance. The old rules we had were certainly a little at fault though they have served us very well. This is shown in the curves reproduced from Dr. Rosenberg's paper on the subject, and by his remarks on Mr. Everest's paper. Professor Walker.

Dr. S. P. SMITH: Mr. Everest has certainly enabled us to make our calculations a little more exactly in such special cases of hunting as those to which he refers; the more we go into this subject, however, the more complicated it seems. There is a large amount of information available, but the difficulty is to know how to find any general rules for dealing with the matter. For example, in Arnold's new book, which was published at the end of last year, there are three long chapters on this subject. One of the most interesting cases dealt with, which shows how difficult the subject is, is with respect to the parallel working of gas-engine sets. He refers to three gas engines which would not work in parallel; two were fed from a common supply pipe and the third from an independent pipe. Although it was found that the three alternators would work on a fairly big load, bad hunting occurred at light loads, so that alterations were necessary. A choking coil was introduced, a device mentioned by Mr. Everest, but that alone was not sufficient. The air-gap was also altered; this again was not sufficient. Calculations showed that there was resonance between the gas in the supply pipe and the natural frequency of the set; in the end it was found necessary to decrease the weight of the flywheel. It has often occurred with internal combustion engines that too large a flywheel had been provided, and by removing some of the flywheel effect stable running was obtained. I should like to ask the author what has been his experience with damping windings and how he calculates their effect. The damping winding is not of much use on one machine running alone, unless perhaps in the case of a converter. But when there are several machines in parallel and an angle of displacement occurs between the pole system and armature field, a very strong pull will be set up if the damping winding is sufficiently ample. In the case of the machine lagging, the damping winding will act like an induction motor and tend to pull the machine into synchronism; or, if the machine is trying to get ahead, the winding will act like an induction generator and hold it back. Dr. Smith.

The author shows in his paper—in fact, it is probably his chief object—that the effect of the polar arc has to be taken into account. It seems to me that if the polar arc has to be taken into account the power factor should also be considered; because, after all, the position of the flux is affected not only by the load but by its phase with respect to the pole centre. Hence to calculate with the distortion method shown might not always lead to correct results owing to a difference of power factor, say, between the day (motor) load and the night (lighting) load. In such a case, the lamps might burn steadily at one time but not at another, owing to the variation of the power factor. One way to get rid of this effect of the power factor might be to use the non-salient

Dr. Smith. pole turbo-generator. I have often thought in considering parallel working that the non-salient pole machine is about as near the ideal as we can get, provided it has a constant air-gap, so that whatever the position of the flux its path has a constant reluctance. In this machine, therefore, the flux can move over the armature in any desired manner, because the distribution of both the exciting and the armature magnetomotive forces is practically sinusoidal, therefore the sum of these two sine waves will always give a resultant sine wave of flux no matter what the load is. The flux simply moves over the armature, and in that way the non-salient pole machine with a constant reluctance throughout the whole circuit for all positions really appears to be the ideal machine from this point of view. I should like to ask the author if he has had any experience on this matter, more especially because such a rotor is eminently adapted for a damping winding when the slots are closed by wedges made of brass or some other metal and well fitted on to the winding covers at each end, whereby a perfect squirrel-cage winding is obtained. Such a machine should have a very powerful damping effect in case of hunting. Experience has shown that when damping windings are used they should be very ample; that is to say, a damping winding of small section, or an incomplete squirrel-cage winding, is only partially effective. A really good damping winding should be made with as low a resistance as possible, and practically a complete squirrel-cage.

Dr. Rosenberg. Dr. E. ROSENBERG (*communicated*): Mr. Everest's paper is valuable in so far as it deals with the difference between the actual synchronizing power for running conditions and that determined from the short-circuit test. I have always used short-circuit test figures, but I am sure Mr. Everest is correct in stating that for salient pole rotors the synchronizing currents for running conditions are considerably higher than those calculated from the short-circuit test, especially if, according to his theoretical considerations, only the air-gap ampere-turns are taken into account. F. Punga, in the *Elektrotechnische Zeitschrift*, vol. 32, p. 385, 1911, gave some excellent examples of this. Previous to this, Dr. G. Huldschiner had published some experiments to determine the relation of the synchronizing forces, and these are in good general agreement with Punga's results. I have recently made some experiments myself with two sets of synchronous motor-generators, the motors having 14 and the generators 6 poles. These machines could be paralleled on the 14-pole sides consecutively in different ways, so as to bring a given pole of one synchronous motor in coincidence with different poles of the second motor. Thus it was possible to obtain a measurable angle between the vectors of the two 6-pole generators before paralleling them. After the generators were paralleled this angle would be divided between the generators and motors, and an exchange of power would take place: the first 6-pole machine, for instance, acting as generator, the second 6-pole machine as motor, the second 14-pole machine as generator, the first 14-pole machine as motor. The measurement showed quite clearly what Messrs. Punga and Everest say: that the synchronizing power for a given angle of devia-

tion is considerably greater than that which would be measured from the short-circuit test with no-load excitation. Theoretically, therefore, I am in full agreement with Mr. Everest. As to the practical application, I must say, however, that if not only the field ampere-turns for magnetizing the air-gap, but the full field ampere-turns for no-load and full-load excitation are taken into account, the figures obtained from short-circuit tests have in my experience always proved satisfactory to avoid the "danger zone" for hunting in calculating the flywheel effect from these figures. I have never considered it possible to calculate the critical value of the flywheel effect to an accuracy of a few per cent, and then to make the flywheel just 20 per cent heavier than the critical value. If the flywheel could not be made *much* heavier, I should certainly use dampers, and take care that the engine impulses of the critical duration were sufficiently equalized so as not to be too high for the damper, which can always be easily achieved if proper precautions are taken in adjusting the cylinders. Professor Görges, discussing the question of the use of calculations for parallel operation, once remarked very properly that here it was not the aim to hit the bull's eye on the target, but to shoot as far as possible away from the target. For this purpose, of course, an approximate calculation is sufficient. I have found in practically all cases which have come to my notice that, if one starts with the total field ampere-turns, the simple short-circuit calculation tallies sufficiently well with the actual results, an experience which, judging from the second paragraph on page 528, Mr. Everest also seems to share. It is true that two inaccuracies are thereby introduced, but these fortunately counteract each other and give a reasonably accurate result.

Dr. Rosenberg.

The distortion method introduced by Hobart and Punga, and advocated by Mr. Everest, is considerably more complicated than the short-circuit method, and is based on a good number of calculated constants and coefficients. In the cases which Mr. Punga and Mr. Everest investigated, the calculated results check exceedingly well with those measured. If this should be borne out also by other experimenters, and with machines of very different construction and magnetic data, it would certainly be worth while to take the more complicated method in order to obtain greater accuracy. It would then be possible, in determining the required size of flywheel, to reduce the factor of safety which is now used and which has to cover inaccuracies of calculation. In some cases where the size of the flywheel cannot be increased beyond a certain amount, and where I should use a damper because the short-circuit calculation with the factors of safety would still indicate the "danger zone," it might be possible to ascertain by the accurate method that we are just outside the "danger zone," and can therefore dispense with a damper. It is highly desirable that other engineers should also publish valuable investigations such as those of Mr. Everest.

I am not in agreement with the first of Mr. Everest's recommendations, (*a*) on page 529, with regard to "flywheel requirements."

Dr.
Rosenberg.

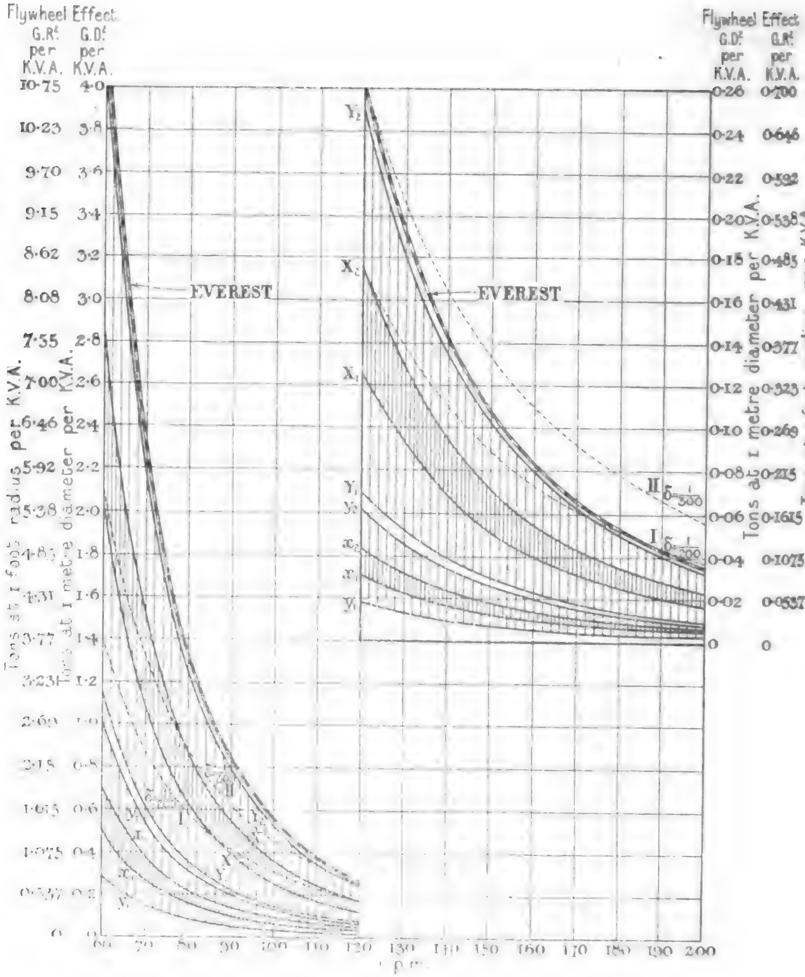

FIG. C.

X_1 and X_2 are critical values of flywheel effect for 50 periods, and a short-circuit current equal to 2·5 and 3·5 times normal current, when the time of one period of oscillating torque equals the time of one revolution.

x_1 and x_2 are critical values of flywheel effect for 50 periods, and a short-circuit current equal to 2·5 and 3·5 times normal current, when the time of one period of oscillating torque equals the time of half a revolution.

For any other frequency or short-circuit current, the flywheel effect will be varied in direct proportion to the frequency or short-circuit current.

I. Average flywheel effect of a two-crank cross-compound steam engine for a cyclic irregularity $\delta = \frac{1}{200}$.

II. Average flywheel effect of a two-crank cross-compound steam engine for a cyclic irregularity $\delta = \frac{1}{300}$.

Dr. Rosenberg.

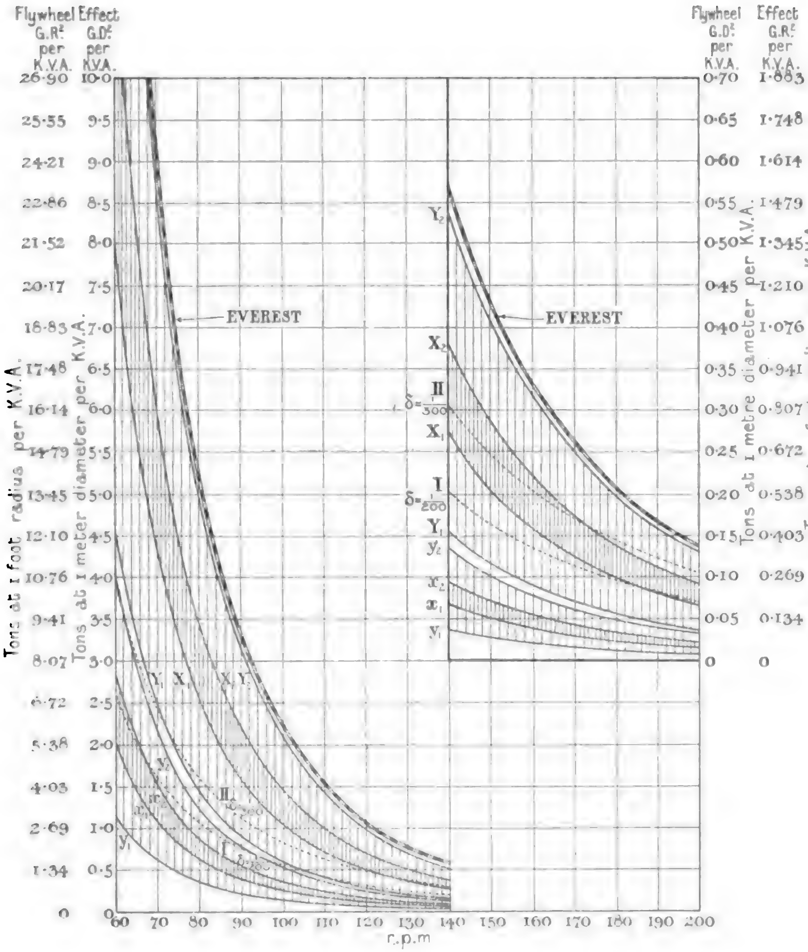

FIG. D.

X_1 and X_2 are critical values of flywheel effect for 50 periods, and a short-circuit current equal to 2·5 and 3·5 times normal current, when the time of one period of oscillating torque equals the time of two revolutions

x_1 and x_2 are critical values of flywheel effect for 50 periods, and a short-circuit current equal to 2·5 and 3·5 times normal current, when the time of one period of oscillating torque equals the time of one revolution.

For any other frequency or short-circuit current the flywheel effect will vary in direct proportion to the frequency or short-circuit current.

I. Average flywheel effect of a gas engine having 4 impulses per revolution for a cyclic irregularity $\delta = \frac{1}{200}$.

II. Average flywheel effect of a gas engine having 4 impulses per revolution for a cyclic irregularity $\delta = \frac{1}{300}$.

Dr Rosenberg.

I much prefer a certain minimum cyclic irregularity to be specified than a permissible angular deviation of $2\frac{1}{2}$ electrical degrees, such as the author gives. For high-speed machines this "permissible" deviation gives quite impossible results. Consider, for instance, a three-crank double-acting steam engine, running at 300 revs. per minute, and driving a 25-cycle generator; putting in the author's formula on page 530 for k (the number of engine impulses per revolution) the figure 6, and for the number of poles 10, the "permissible" limit of cyclic speed irregularity works out as $\frac{1}{10}$th. Even if we took into account the fact that the cylinders with different loads may contribute quite different forces, and if we only considered the two impulses per revolution, we should get a "permissible" irregularity of $\frac{1}{30}$th. I am afraid that engine makers would not know how to deal with such requirements, and that the customer would not find such a speed variation permissible. In his second recommendation Mr. Everest does not consider the necessity, which often arises in connection with multiple 4-stroke cycle gas engines, of working with a flywheel that lies between the critical values for the impulses the period of which equals respectively the time of one revolution and two revolutions. In this connection I take exception to the expression "frequency of the predominating engine impulse," as this expression may give rise to a serious misunderstanding. In the torque diagram of a single-crank double-acting steam engine, where every stroke represents an impulse of nearly the same magnitude, we should certainly be inclined to ascribe to the "predominating impulse" a frequency of double the number of revolutions. This, however, is nearly always a perfectly harmless impulse, the frequency being far removed from the critical frequency. The oscillating force, equal in frequency to the number of revolutions, is far more dangerous. As this force is generally very small compared with that of twice the frequency it seems rather an unfortunate contradiction to call it the "predominating impulse."

Mr. Everest has said nothing about the use of damper windings on the field poles. These are of the greatest importance in the case of low-speed two-cylinder double-acting four-cycle gas engines, and by their use in that case many tons of flywheel can be saved. The formula which Mr. Everest gives for the flywheel effect required for generators of ordinary regulation is simple and handy if one is used to work with the stored energy in the flywheel. In Figs. C and D I reproduce Figs. 11 and 14 of my paper on the parallel operation of alternators,* with the values marked in as calculated in accordance with Mr. Everest's formula. It will be seen that Mr. Everest's formula agrees well with the curve Y2, which represents the flywheel effect required if it is desired that the natural frequency of the machine should be below the frequency of the engine oscillations.

Professor Marchant.

Professor E. W. MARCHANT (*communicated*): The cases dealt with by Mr. Everest in his paper are of great interest, and his table of constants should be of value to those engaged in calculations of this

* *Journal of the Institution of Electrical Engineers*, vol. 42, pp. 540 and 545, 1909.

kind, but it would be interesting to know whether these constants apply to types of machines other than those dealt with in the paper. The formulæ certainly fit the experimental results well for the alternators to which they have been applied, but it seems doubtful whether the same accuracy would be obtained with different designs. The actual magnetizing current flowing between two machines when they are out of step varies with the periodicity of the variations. It was shown by Professor Miles Walker in his paper on the short-circuiting of turbo-alternators that in a turbo-alternator the armature reactance for rapidly changing currents might be one-twentieth of the ordinary short-circuit value. The ratio would not be expected to be so large for the flywheel-type alternator, but the reactance might be less than it is when calculated from "short-circuit" constants. In the case calculated by Mr. Everest on page 526 the cross-current reactance is assumed to be half that calculated from the short-circuit reactance. To bring the calculated and observed values of the free period into agreement it would have to be one-third, and it would be of interest, if experimental results were available, to see how nearly this ratio agreed with experimental results obtained by the sudden short-circuiting of the alternator. It seems somewhat doubtful whether the variations due to distortion penetrate much beyond the pole face at the frequencies assumed, and it would certainly be simpler to regard the whole difference between observed and calculated values as due to the variation in the armature reactance for sudden or comparatively sudden variation in the armature current, as proposed by Goldschmidt and Schüler. In Dr. Rosenberg's paper * it was shown that hunting troubles might arise from a natural period corresponding with a multiple of the number of revolutions per second of the engine, and it would be interesting to know whether Mr. Everest has ever come across a case of this kind. The paper is valuable in that it lays stress on the necessity for some correction in the value for the short-circuit reactance obtained by steady short-circuit tests. Professor Marchant.

Mr. W. H. F. MURDOCH (*communicated*): The effect of damping in wiping out oscillations does not appear to be dealt with in the paper, although I always understood *amortisseurs* frequently saved much "flywheel effect." The ordinary equation for oscillations with damping might be written— Mr. Murdoch.

$$\frac{d^2\theta}{dt^2} + k_1\frac{d\theta}{dt} + k_2\theta = 0,$$

where k_1 and k_2 are constants. For the forced oscillations we should have—

$$\frac{d^2\theta}{dt^2} + k_1\frac{d\theta}{dt} + k_2\theta = \mathrm{F}\cos pt,$$

where F is the "forcive" causing the forced oscillations to continue. The difficulty in solving the problem seems to arise from the fact

* *Journal of the Institution of Electrical Engineers*, vol. 42, p. 524, 1909.

Mr. Murdoch.

that the "forcive" may vary with load and angle of lag. A large moment of inertia merely increases the periodic time of the natural oscillations and tends to make them persist; whereas damping increases the periodic time, but tends to wipe out oscillations. Fly-wheels also are costly and waste energy in bearing friction, whereas *amortisseurs* act only when required. It has also to be remembered that without damping the running would be in unstable equilibrium. The problem in practice is also rendered more complex since, for instance, if there were five machines in parallel there would be ten different free oscillations all acting at the same time. Consequently, conditions for stable running for two machines may not apply to several, or be of general application.

Discussion before the Birmingham Local Section, on 12th February, 1913.

Professor Threlfall.

Professor R. THRELFALL: Owing to the concentrated style of the paper I have had great difficulty in following it, and I consider that the whole subject ought to be dealt with in an extended treatise. At present there appears to be a difference of opinion on important points. As one term in the formula for the "natural frequency" of an alternator appears to depend on the other machines with which such alternator happens to be in parallel, would it not be necessary to calculate a fresh natural frequency every time a new unit was added to the station? With regard to the very convenient diagrams published by Dr. Rosenberg, I should like to ask Mr. Everest to prepare some similar diagrams using the corrected values given in the paper.

Dr. Kapp.

Dr. G. KAPP: It must not be thought that the paper refers only to the older type of steam alternators; it also concerns turbo-alternators. These machines, although by themselves not liable to the cyclic disturbances arising with reciprocating engines, may nevertheless experience disturbing influences of this kind if they have to work in parallel with reciprocating sets. The author has quoted some figures given by Goldschmidt in an article published ten years ago. The author does not base his arguments on Goldschmidt's values, but on the method used by Punga, yet it seems desirable that a discrepancy which exists between Goldschmidt's formula and his figures should be cleared up. Taking Goldschmidt's formula I have obtained figures differing appreciably from those given by Goldschmidt and republished by Mr. Everest in the table on page 522. Thus taking the value 0·8 for the ratio pole/pitch Goldschmidt's formula gives 0·622, whilst the figure in the table is 0·51. For a ratio of 0·7 the respective figures are 0·46 and 0·35. These discrepancies are too large to be accounted for merely by inaccuracies in reading a slide-rule, and it seems desirable that the author in his reply should state whether he considers Goldschmidt's formula or Goldschmidt's figures correct. Another point which the author should clear up is the statement on page 525 where a distinction is made between that part of the self-induced

flux which misses and that part which passes through the pole face. Since neither part is interlinked with the field winding and both are interlinked with the armature wires there does not appear any physical reason for assuming that one part (namely, that which misses the pole face) is alone producing the E.M.F. of self-induction, whilst the other part is alone responsible for distortion. Moreover, no reason is given why the flux to be taken into account in calculating the E.M.F. of self-induction should be just one-half of the total self-induced flux. Dr. Kapp.

Reference has been made to the difference between the author's and Dr. Rosenberg's evaluation of the magnitude of K_w in the formula on page 521. If K_w is taken as the product of induced voltage at no load and the short-circuit current at that excitation, then certainly the periodic time would be overestimated. In reality the synchronizing power is much greater, and this makes the natural swing of the dynamo quicker; Dr. Rosenberg, however, does not take the value of K_w at the low no-load excitation, but at the full-load excitation, and this tends to make K_w larger, *i.e.* nearer the true value. This does not mean that it necessarily gives the correct value for the periodic time. From a careful investigation made by Punga on machines installed in four central stations it is shown that in all cases the time of swing is shorter than corresponds to Rosenberg's formula, so that I am inclined to consider Punga's method, which Mr. Everest has adopted, to be the more reliable method; but it should not be marred, as the author has done, by the introduction of Hobart's very arbitrary correcting factor of $\frac{1}{2}$. If this correction is not made it will be found that the value of K_w on page 526 is 7,700 kw. instead of 9,300 kw. This would make a difference of 11 per cent in the predetermined periodic time. It may perhaps be thought that so small a difference is not of much moment, since a careful designer will in any case aim at a difference between engine time and dynamo time much greater than 11 per cent, but it should be remembered that unless a designer knows the sign and magnitude of this difference exactly, he may in his desire to get farther away from the dangerous condition, by some alteration in flywheel inertia actually approach closer to it.

Dr. W. E. SUMPNER: I wish to draw attention to the simplification resulting from using a consistent system of units in all engineering formulæ, and to urge in particular that electrical engineers should try as far as possible to express all quantities in electrical units. Not only would this lead to the standardization of formulæ, but the formulæ themselves would appear much simpler and more intelligible. Thus, instead of reckoning the flywheel energy in foot-tons per kilowatt (at normal speed and full rated load), it is convenient to regard this energy as so many seconds of full load. The machine instanced in case 1, on page 526, has an output of 1,500 kw. and a flywheel energy of 9,600 foot-tons. A foot-ton is equivalent to an output of 1 kw. for 3 seconds, whence the flywheel energy in the case cited corresponds with the full load for 19·2 seconds. If this flywheel time in seconds be called t, and if ρ denote the ratio between the short-circuit current and the full-load Dr. Sumpner.

Dr. Sumpner.

current of the machine, and if f be the current frequency, the ordinary formula for the period of the hunting becomes

$$2\sqrt{\frac{\pi t}{\rho f}}.$$

I have used this formula for many years, though not with satisfactory results, owing to causes which form the basis of Mr. Everest's paper. The only uncertain quantity in the formula is ρ. Taking, as on page 525, the armature impedance voltage as 42·4 per cent of the open-circuit voltage, the value of ρ would be the reciprocal of 0·424, *i.e.* 2·4, whence the formula for case 1, with $t = 19{\cdot}2$ seconds, and $f = 50$, gives 1·4 seconds for the period of the hunting; that is too long a time, since the oscillation frequency actually found was 68 per minute. But if corrections are introduced for field reaction or distortion in accordance with the author's views, the above value of ρ must be doubled. The calculated time, 1·4 seconds, would then be reduced in the ratio of $\sqrt{2}$ to 1, and would approach close to the value actually found. There can be no doubt that it is incorrect to treat the armature of the machine merely as a coil having self-induction, as is always done in the simple theory of the hunting of alternators. On the other hand, the experimental factors introduced into the formula by Mr. Everest's method, although leading to correct results, are not very satisfactory from a theoretical point of view.

Dr. Kahn.

Dr. M. KAHN: I agree with Mr. Everest that the distortion method gives the most accurate results, and the reasons set out in the paper explain this fact quite clearly. I must differ from Mr. Everest, however, in one part. In calculating the synchronizing power Mr. Everest only takes one-half of the reactance into account, saying that the total reactance flux contains a part which crosses the air-gap and passes along the pole face. The total flux produced by the armature of an alternator is usually divided into that which enters the rotor and that which is only interlinked with the armature winding itself. The flux which enters the rotor can be subdivided into a cross flux, which crosses the air-gap and passes along the pole face, and a demagnetizing flux which passes through the machine in the opposite direction to the flux produced by the field excitation. The part only interlinked with the armature winding itself induces in this winding the so-called reactance voltage. This voltage can either be measured by removing the stator from the rotor and measuring the voltage necessary to pass full-load current through it, or it can be deduced from the short-circuit test, where the cross flux is nil. In neither case is there a cross flux which crosses the air-gap and passes along the pole face; no reduction need therefore be made from the reactance obtained on this account in calculating the synchronizing power. The formula for the frequency of the oscillations per minute is thus:—

$$f_o = 976\sqrt{\frac{\text{cycles}}{\text{foot-tons}} \cdot \frac{K_w}{\frac{A.T_t}{A.T_g} + E_r}}$$

where $A.T_c$ = cross ampere-turns per pole; Dr. Kahn.
$A.T_g$ = air-gap ampere-turns per pole; and
E_r = percentage reactance voltage.

The angle between the current vector and the flux vector at non-inductive full load is given in radians by

$$\frac{A\,T_c}{A\,T_g}+E_r.$$

In connection with the armature flux, it may be worth mentioning that in what Mr. Everest calls the short-circuit method the demagnetizing flux is taken into account in calculating the synchronizing power, while in the distortion method the cross flux is taken into consideration.

Mr. J. MARTIN: On page 522 Mr. Everest very properly calls attention to the incorrect method which has been used by several writers for deducing the short-circuit current of an alternator. During the short-circuit test there is only sufficient flux in the machine to send a short-circuit current through the stator windings, that is, to overcome the impedance, which will at most consume from 30 to 40 per cent of the normal voltage. It is clear that no saturation can occur; and therefore in calculating the cross-current at normal excitation only the air-gap ampere-turns can be assumed as being effective in producing it, as the short-circuit current is proportional to the flux across the air-gap, and this is proportional to the air-gap ampere-turns and not to the total ampere-turns. With regard to the reactance voltage that should be considered in constructing Fig. 3, it is stated that only half the total value should be taken, the other half being already accounted for in the distorting effect on the field. This seemed to me at first to be allowing for too much leakage flux across the air-gap. I therefore had some machines of the ordinary salient-pole engine-driven type checked, and I found that the leakage flux across the air-gap did account for about half the total leakage flux; as a matter of fact this flux varied from 40 to 60 per cent of the total leakage flux. It would be as well, however, to check this each time, as very different results might be obtained on machines built with abnormally high reactance. (This does not necessarily mean high armature reaction.) On page 528, under "Effect of Load," Mr. Everest suggests that in deducing the short-circuit current the air-gap ampere-turns corresponding to the total generated voltage should be considered. This seems to be correct; and the angle D O C of Fig. 3 can then be ascertained. It would be interesting, however, to know how much the distortion angles D O A and B O E are affected at full load by a change of power factor. It seems to me that one of these angles will be decreased and the other increased, as O A and O B will no longer pass through the centres of the poles. On page 530, in connection with the flywheel effect which should be provided to avoid trouble from resonant hunting, Mr. Everest points out that for a 4-cycle engine running at 200 revs. per minute the flywheel effect should be such as to give a natural frequency of 80 cycles per minute. This we know means a heavy, and consequently Mr. Martin.

Mr. Martin. expensive, flywheel, but in my opinion it is better practice to do this than to attempt to get the natural frequency of the revolving parts to lie between a frequency corresponding to the predominating engine impulse and twice this frequency, that is, to lie between 100 and 200 per minute. It is true that many plants have been designed on this latter principle, but we all know that it is difficult to calculate with absolute exactness all the quantities entering into the equation for obtaining the natural frequency of swing of a system, and we are always liable to get too near to one or the other of the frequencies between which only satisfactory running can be obtained. I think the most that can be said for the plants designed to work with the light flywheel is that at the best they are running moderately well.

Mr. Bradbury. Mr. R. H. BRADBURY: Since the double-acting tandem gas engine of the Nurnberg type claims so good a cyclic irregularity as 1/250, good parallel operation is largely dependent upon the design of the alternators. From theoretical considerations alternators appear in a general way to be best suited for parallel running if designed to have small internal resistance and low self-induction, or in other words, if designed for good voltage regulation, because then the synchronizing current for a given angle of displacement is the greatest. My experience, however, has been rather with gas-engine-driven alternators of the same type, but designed for a special purpose to have a high armature reaction in order to get constant load independent of sudden changes in the resistances of the external circuits. The external characteristic curve for these particular alternators therefore shows that for a 25 per cent increase in current the E.M.F. at the machine terminals is reduced by about 20 per cent, since $\frac{125}{100}\,C \times \frac{80}{100}\,E = C\,E$. If therefore these machines are in parallel, then for all conditions of load the power is equally divided between them. Supposing, however, the alternators are of different design, and one machine has a much lower self-induction than the other, then the characteristic curves cross at a point P, and at this point only is the load proportionately distributed between the machines. Therefore alternators having dissimilar characteristics and running in parallel require for every load condition a different value of excitation current. This is of course a big drawback to a system, and particularly so where the load fluctuates rapidly, as in electric process work. Again, if the alternators are generating the maximum load of the engines (engines not working on the governors) then an increase of current reduces the E.M.F. of one of the machines more than that of the other, and since a redistribution of the load cannot take place to wipe out this difference of E.M.F., the net result is that one machine gives wattless lagging current and the other receives wattless leading current. Although this does not represent loss of power, yet experience goes to show that in some cases at least the above is not a condition making for good parallel operation. For engines on full load I have often experienced temporary trouble in parallel operation due to one of the alternators being over-excited. I therefore think that in designing an alternator for parallel running one

ought to compromise if necessary between the best value for the natural frequency of the alternator and the correct self-induction to give a characteristic curve similar to that of the alternator with which it is intended it should operate. Mr. Bradbury.

Mr. N. SHUTTLEWORTH (*communicated*): It is quite time there was agreement between engineers on several points raised in the paper—one of the most important is the calculation of the natural oscillating frequency of an alternator running in parallel with others. I believe that everybody will agree with the formula given by Mr. Everest on page 521, but there are wide differences in the values that have been obtained by the many writers on the subject for the synchronizing power K_w for a given displacement from the mean position. I give below a method of treatment which appears to be quite rational, and its agreement with fact, in all cases to which it has been applied, warrants its being put forward. The principle of the analysis lies in separating all the M.M.F.'s of the field and armature, determining the fluxes set up by each separately, and finally combining the E.M.F.'s Mr. Shuttleworth.

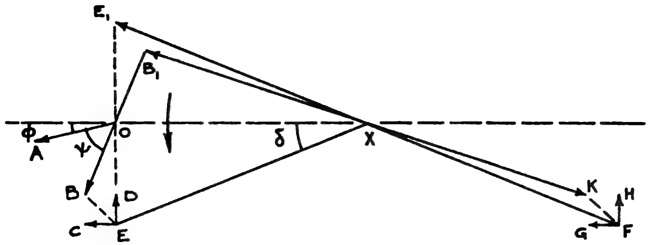

FIG. E.

generated in the armature by these fluxes in the proper phase together with resistance and reactance drops. Consider the action in the local circuit of two similar machines working in parallel when there is mechanical displacement of the two rotors from the synchronous position. In Fig. E let X E be the vector representing the generator-phase voltage, and X F the phase voltage of the second machine, which will be motoring if on no load. The component of X E which opposes X F in the local circuit is X E_1, so there still remains a component voltage E_1 E which is free to produce a circulating current round the circuit of the two machines. Under ordinary conditions this circuit is highly inductive, and the current will lag practically 90 deg. behind E_1 E, but as a more general case we may assume that there is sufficient resistance in the circuit to cause the current to lag by an angle appreciably less than 90 deg.; the final phase due all influences is shown as O A. We must now take account of the secondary effects of O A. The wattless component O A sin ϕ produces additional magnetization of the main pole of the generator and demagnetization of the lagging machine. The change in flux generates an E.M.F. E C in phase with the terminal

Mr. Shuttleworth.

voltage of the generator, and an E.M.F. F G opposed to the terminal voltage of the lagging machine. Further, the wattless component of current, viz. O A cos ϕ, produces a cross flux which will generate an E.M.F. E D in the armature of the generator and an E.M.F. F H in the armature of the lagging machine. The resultant voltage in the generator is X B, of which a component X B_1 opposes the resultant voltage X K of the lagging machine; the remaining component B_1 B is therefore the true voltage which forces the current O A through the impedance of the two armatures. Knowing the resistance and reactance of the machines the angle ψ is known. The displacement angle δ is found from the ratio of O E to the terminal voltage X E, where $\sin \delta = \tan \delta = $ O E/X E. The synchronizing kilowatts will be X E $\times$ O A $\cos \phi \times$ number of phases. In ordinary cases where the resistance of the armature is small compared with the reactance, ϕ will become negligibly small, ψ will be equal to 90 deg., C E vanish, and O B become equal to O D.

Paying attention to the simple case first, it is necessary to determine the magnitude of the voltage D E in Fig. E. The synchronizing energy current O A produces cross-magnetization, and assuming a known

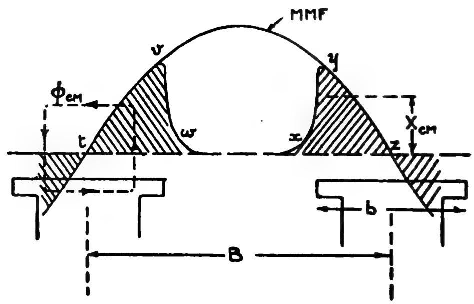

FIG. F.

value of current we may represent the M.M.F. distributed sinusoidally along the armature periphery as shown by the curve *t v y z* of Fig. F. The fluxes set up are represented by the shaded areas *t v w x y z*, the actual path being ϕ_{CM}. It will be legitimate to take *t v w x y z* as a special flux which revolves synchronously with the field poles, generating an E.M.F. in the armature conductors at right angles in phase to the main E.M.F., and which is represented by D E in Fig. E. The peculiar wave shape has a pronounced third harmonic in each phase, but this cancels out in a three-phase star-connected alternator, leaving only the fundamental effective at the terminals. Let the value of the cross-magnetizing ampere-turns equal X_{CM}. It is evident that the reluctance in the path of this M.M.F. is that of an air-gap of area equal to two half pole faces, the stator teeth and the iron of the pole face, which is not more than 5 per cent in excess of the reluctance of the air-gap over a main pole. For a known value of X_{CM} we may therefore obtain the value of voltage generated in the armature from the straight

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portion of the saturation curve without going into detailed calculation for every case, provided that the wave shape is the same for the two cases under consideration. Since, however, the wave shape is not sinusoidal in the case of the E.M.F. due to cross-magnetization, it is necessary to correct for form factor.

The actual E.M.F.'s have been obtained graphically for cases of 3 and 4 slots per pole and phase, and in comparing the voltages obtained for a given value of X_{CM}, first cross-magnetizing and second magnetizing the pole directly, the following results were obtained :—

$$\frac{\text{E.M.F. due to } X_M \text{ cross-magnetizing ampere-turns}}{\text{E.M.F. due to } X_M \text{ magnetizing ampere-turns}}$$
$= 0{\cdot}88$ for three-phase machines and $0{\cdot}85$ for two-phase machines.

Only the reluctance of the air-gap is considered in the above cases, so that the "cross-voltage" should be further reduced by 3 to 5 per cent

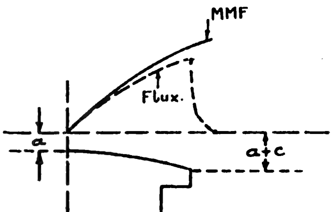

FIG. G.

to allow for reluctance of iron parts. The value of cross-magnetizing ampere-turns for full-pitch windings is given by—

$$X_{CM} = \frac{2\sqrt{2}\, m\, n\, I}{\pi} \times 0{\cdot}96 \times \frac{2B}{\pi b}\left[1 - \cos\frac{b \times \pi}{B \times 2}\right],$$

where m = number of phases ; n = number of turns in series per pole per phase of armature ; b = width of pole face ; B = pole pitch ; and I = watt component of synchronizing current.

With tapered pole faces, as shown in Fig. G, the cross-flux is reduced, with a consequent diminution in generated voltage. This reduction in flux may be taken account of most simply by assuming that the "actual" X_{CM} is reduced to a value "effective" X_{CM}, which bears the same proportion. There will be no error involved in this, since proportionality exists in the absence of saturation. With values of air-gap given in Fig. G, and full-pitch winding—

"Effective" X_{CM}

$$= \frac{2\sqrt{2}\, m\, n\, I}{\pi} \times 0{\cdot}96 \times \frac{2B}{\pi a b}\left[(a+c) - (a-c)\cos\frac{b \cdot \pi}{2B} - \frac{4Bc}{\pi b}\sin\frac{b \cdot \pi}{2B}\right]$$

By working with "effective" X_{CM}, the generated cross-voltage may be obtained directly from the saturation curve with a minimum of

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trouble. A chamfered pole piece causes an increase in the gap length, and the reduction in the cross-flux will be more apparent in the lower generated voltage obtained from the saturation curve than due to a decrease of the maximum density at the wave tip. The error involved due to neglecting chamfer and working with average gap lengths will not exceed 5 per cent in ordinary cases, and the error in the final result is proportional to the square root of this.

It is possible to apply the above method at this point to Case I given by Mr. Everest. The following particulars kindly placed at my disposal by the author apply to this machine :—

Turn in series per pole per phase	... =	24;
Pitch of armature winding	... ... =	91 per cent;
Number of poles ...	... ... =	6;
$\dfrac{\text{Air gap at pole centre}}{\text{Air gap at pole tip}}$	... ... =	$\frac{1}{2}$;
Whence $a = 1$ and $c = 1$.		
Ratio b/B ...	... ...	$= 0{\cdot}65$;
Reactance ohms per phase	... ...	$= 1{\cdot}6$ [this figure will be calculated later].

Assuming a synchronizing current of 100 amperes (entirely energy current), the cross-magnetization in ampere-turns is given by—

"Effective" X_{CM}

$$= \frac{4\sqrt{2} \times 24 \times 100}{\pi} \times 0{\cdot}96 \times \frac{2}{0{\cdot}65\,\pi}\left[2 - \frac{4}{0{\cdot}65\,\pi} \sin 0{\cdot}65 \frac{\pi}{2}\right] = 1{,}340 \text{ amp.-turns.}$$

The same amount of magnetization acting over the main pole will generate 990 volts per phase, as shown by the saturation curve. Allowing for the form factor of the wave shape in a two-phase machine and 5 per cent for the increased reluctance of path when considering cross-magnetization—

"Cross-voltage" $E\,D = 990 \times 0{\cdot}85 \times 0{\cdot}95 = 800$ volts.

$O\,B = O\,D = 100 \times 1{\cdot}60 = 160$ „

$O\,E = 960$ „

Synchronizing power $K_w = 2 \times 6600 \times 100 \times 10^{-3} = 1{,}320$ kw.

$$\sin \delta = \tan \delta = \frac{960}{6600} = 0{\cdot}145, \text{ therefore } \delta = 8{\cdot}25 \text{ deg.}$$

Synchronizing power for one radian displacement

$$= 1320 \times \frac{57{\cdot}3}{8{\cdot}25} = 9170 = K_w.$$

$$\text{Oscillating frequency} = 9{\cdot}76\sqrt{\frac{9170 \times 50}{9600}} = 67{\cdot}5.$$

In the more general case where resistance is not negligible it is necessary to determine the angles ψ and ϕ of Fig. E, and the voltage

Mr. Shuttleworth.

C E. The angle ψ is known from the ratio of reactance to resistance in the local circuit between the two alternators. For the determination of ϕ let a totally wattless circulating current I_w produce a magnetization or demagnetization $= X_D$ ampere-turns where

$$X_D = \frac{\sin \frac{b}{B} \times \frac{\pi}{2}}{\frac{b}{B} \times \frac{\pi}{2}} \times \frac{2\sqrt{2}\, m\, n\, I_w}{\pi} \times 0{\cdot}96$$

for full-pitch windings (see Fig. H).

Referring to the saturation curve at the point of normal voltage, we may determine the increase of generated voltage for an increase of X_D ampere-turns. Let this increase $= K\,E_o$, where E_o is the phase voltage of the machine. If now we consider a circulating current I,

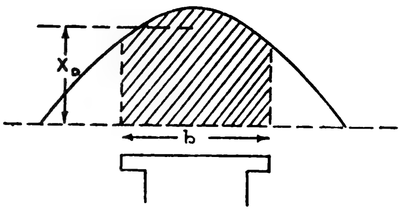


FIG. H.

which is not totally wattless but lags by an angle ϕ, the magnetizing ampere-turns become $X_D \sin \phi$, and the increase in voltage $E\,C = K\,E_o \sin \phi$. Now B O (Fig. E) equals $z\,I$, where z is the impedance of the armature of one machine; and since ψ is known, we may write—

$$C\,E = B\,D = B\,O \sin \widehat{B\,O\,E}, \text{ or } K\,E_o \sin \phi = z\,I \sin\left(\frac{\pi}{2} - \psi - \phi\right),$$

that is—

$$\frac{K\,E_o}{z\,I} = \frac{\sin\left(\frac{\pi}{2} - \psi - \phi\right)}{\sin \phi},$$

from which ϕ is determined in terms of the circulating current I. It will be evident that ϕ is constant, and that K varies in direct proportion with the values of I. Using the value of demagnetizing ampere-turns just given it is possible to determine the reactance ohms of the windings from the short-circuit test.

With stator amperes = 196 on short-circuit, and field ampere-turns = 7,140, the demagnetizing ampere-turns of armature reaction with 91 per cent pitch winding are given by

$$X_D = \frac{\sin 0{\cdot}32\,\pi}{0{\cdot}32\,\pi^2} \times 4\sqrt{2} \times 24 \times 196 \times 0{\cdot}96 \times 0{\cdot}99 = 6{,}720.$$

The ampere-turns producing flux $= 7{,}140 - 6{,}720 = 420$.

Mr. Shuttleworth.

From the saturation curve, the voltage generated in the armature $= 315$, which is absorbed in the reactance drop.

The reactance ohms per phase $= 315/196 = 1{\cdot}6$ ohms.

The author has kindly provided information which allows of the more general case being applied. Artificial resistances and reactances were added between two similar machines in the same station, and the natural oscillating frequency was reduced from 68 to 50 oscillations per minute. The conditions on this test were as follows:—

Reactance.	Resistance.	
1·6	0·16	Machine 1.
1·6	0·16	Machine 2.
11·2	7·20	Added artificially.
14·4	7·52	

Impedance $= 16{\cdot}2\ w$, or $8{\cdot}1$ ohms per machine.

The angle ψ therefore equals $62{\cdot}5$ deg.

With a wattless circulating current of 196 amperes we have seen that the magnetization of the armature equals 6,720 ampere-turns. From the saturation curve at the point of normal voltage, we see that there is a 5·3 per cent increase in voltage for an increase of 10 per cent in ampere-turns $= 1{,}220$; hence for a magnetization or demagnetization of 6,720 ampere-turns

$$K = \frac{5{\cdot}3}{100} \times \frac{6720}{1220} = 0{\cdot}292.$$

From the formula—

$$\frac{0{\cdot}292 \times 6{,}600}{8{\cdot}1 \times 196} = \frac{\sin(90^\circ - 62{\cdot}5^\circ - \phi)}{\sin\phi}$$

from which $\phi = 12{\cdot}4$ deg., $\phi + \psi = 74{\cdot}9$ deg., and the angle B O D in Fig. E $= 15{\cdot}1$ deg. These angles remain constant for varying values of synchronizing current; we can therefore calculate the displacement δ for any value of synchronizing current.

Assume a synchronizing current of 100 amperes, when

$$K = 0{\cdot}292 \times \frac{100}{196} = 0{\cdot}15,$$

then $\quad OB = 8{\cdot}1 \times 100 = 810$ volts.

$\quad EC = K E_o \sin\phi = 0{\cdot}15 \times 6{,}600 \times \sin 12{\cdot}4^\circ = 211$ volts,

also $\quad EC = OB \sin 15{\cdot}1^\circ = 810 \times \sin 15{\cdot}1^\circ = 211$ volts,

$\quad OD = OB \cos 15{\cdot}1^\circ = 781$ volts.

The cross magnetization for the same synchronizing current of 100 amperes is given by—

"Effective" X_{CM}

$$= \frac{4\sqrt{2} \times 24 \times 100 \cos\phi}{\pi} \times 0{\cdot}96 \times \frac{2}{0{\cdot}65\,\pi}\left[2 - \frac{4}{0{\cdot}65\pi}\sin 0{\cdot}65\frac{\pi}{2}\right] = 1340 \cos\phi.$$

Referring to the previous example, $ED = 800 \cos\phi = 780$ volts.

Mr. Shuttleworth.

Hence O E $= 780 + 781 = 1{,}561$ volts.

$$\sin \delta = \tan \delta = \frac{1561}{6600} = 0{\cdot}237, \text{ or } \delta = 13{\cdot}5 \text{ deg.}$$

Synchronizing power $= 2 \times 6{,}600 \times 100 \cos \phi \times 10^{-3} = 1{,}290$ kw.
Synchronizing power for one radian displacement

$$= 1290 \times \frac{57{\cdot}3}{13{\cdot}5} = 5{,}480.$$

$$\text{Oscillating frequency} = 9{\cdot}76\sqrt{\frac{5480 \times 50}{9600}} = 52.$$

There is nothing in the foregoing method of treatment to indicate that saturation, either in the stator core or pole cores, in any degree affects the natural frequency of the alternator; the same is true also of load conditions, and this would seem to be in entire agreement with experimental observations.

Mr. Everest.

Mr. A. R. EVEREST (*in reply*): Referring to Professor Miles Walker's remarks, it is quite correct that for the purpose of this calculation the ampere-turns allowed at the air-gap should include an allowance for the teeth, and also for the pole face if highly saturated. Hobart and Punga, in the treatment referred to, always mention "ampere-turns for gap and teeth."

In connection with reactance pressure, several speakers have questioned the technical justification for employing a reducing factor. It is explained in the paper that Hobart and Punga's analysis of 1904 (upon which this method is based) used a value of reactance so calculated as to exclude all leakage flux emerging from the armature, the effect of the latter being included in the "distortion" term. They specially pointed out the smallness of the resulting reactance pressure compared with that generally assumed. In fact, inspection shows that the value they employ is roughly one-half that determined from the short-circuit test in the usual way, *i.e.* with the assumption that the armature reaction is 0·70 of the total armature ampere-turns per pole, corrected for breadth factor, but with no correction for pole shape. This is confirmed by Mr. Martin's communication. Of course no reducing factor is necessary when the winding reactance pressure has been calculated employing such constants as give the reduced value, or is determined from short-circuit tests in which the demagnetizing effect of armature reaction has been properly corrected for the shape of the pole. The remark of Dr. Sumpner and others, that the experimental factors introduced are not fully explained, can be best answered by referring to Hobart and Punga's paper, in which a complete explanation is given. Mr. Shuttleworth has also contributed to this discussion a very interesting treatment which arrives at similar results in a different manner.

Dr. Kapp points out a discrepancy in Goldschmidt's article to which reference has been made, and asks whether the values from his formula or his published table are more probably correct. The values

Mr. Everest.

calculated from the formula appear to agree with those derived by others.

Several have suggested that a reference to dampers should have been included in the paper. The presence of dampers does not materially modify the natural frequency at which a machine tends to oscillate. Their principal function is to limit the amplitude of these oscillations to a reasonable value when the amount of flywheel is such as to bring the natural frequency dangerously near to the resonant condition, as discussed below in my reply to Dr. Rosenberg's remarks. In this sense they may be said to permit light flywheels: in fact, heavy wheels make dampers ineffective, and the operation of machines fitted with dampers can sometimes be improved by reducing the amount of flywheel. Dampers may also be employed to limit the amplitude of the forced oscillations resulting from irregular turning moment of the engines.

Answering Dr. Smith's question, if a damper were required on a machine with only a few poles it would be of advantage to make the rotor cylindrical and thus obtain a complete squirrel cage. But in machines with many poles the definite pole type of construction is usually employed. Damper bars are provided in the pole faces and also sometimes between the poles. In any case the rings which connect the bars should be continuous, bridging the space between the poles. The calculation of dampers is rather outside the scope of this paper.

As regards the power factor of the load and its effect upon hunting, it is true that the flux axis takes a different position on the pole face according to the nature of the load, and in the extreme case when the flux axis might be already near the edge of the pole the angular sweep for a given hunting current would be different from that with the flux axis normally in the centre.

Regarding Professor Marchant's remarks, the so-called "synchronous reactance" which is effective under ordinary conditions, and which determines the current on continued short-circuit, includes not only the voltage absorbed per ampere in winding reactance but also the reduction in generated volts due to the demagnetizing effect of the armature reaction. In the case of a sudden short-circuit, the former alone is at first active because the field flux cannot immediately die down. In the case of hunting in parallel operation, the effects do not depend on a change in flux through the pole; the flux sweeps to and fro across the pole face. The time element has little effect on this action unless dampers or their equivalent are present in the pole face; in which case, as is well known, the flux swing is largely prevented. Regarding the question of resonance to multiples of the frequency of disturbance, it is advisable always to keep the natural frequency below that of the disturbance, unless dampers are provided which can be relied upon to eliminate resonance.

Dr. Rosenberg, while admitting the correctness of the new method, says that satisfactory results have been obtained from the old method.

This argument might be admitted if troubles in parallel operation were unknown in practice. It will be seen that while the admitted inaccuracies of the old method are usually of such nature that they tend to cancel one another, this is not always the case; hence the desirability of using a method which takes the factors properly into account. The suggestion that this new method is much more complicated than the old one is not borne out by the example given on page 526. Mr. Everest.

As regards "flywheel requirements," Condition (*a*) sets a limit upon the angular displacement resulting from cyclic speed irregularity in order to limit the periodic swing on the instrument needles due to the resulting exchange current. In a machine with few poles this ceases to be a limiting feature, as the amount of flywheel supplied in ordinary practice keeps the angular displacement well below the limit here shown. In connection with Condition (*b*) relating to resonance, the expression "frequency of the predominating impulse" refers to any recurring irregular impulse which may differ from the other impulses during the cycle. In a steam engine the frequency of possible irregularity is obviously once per revolution; the same is true of a two-stroke-cycle Diesel engine, while in a four-stroke-cycle engine the frequency of any recurring irregular impulse would be once in two revolutions, and obviously independent of the number of cylinders working. At the end of the clause referring to Conditions (*a*) should be added the words "in no case must the cyclic speed irregularity be worse than $\frac{1}{150}$"; and after the clause referring to Condition (*b*) should appear a statement that the flywheel effect is expressed in foot-tons of stored energy at normal speed.

Dr. Rosenberg mentions that no reference has been made to machines driven by four-stroke gas engines and provided with such flywheels as to give a natural frequency between the crank-shaft and cam-shaft frequencies. Dr. Rosenberg has shown by his diagrams that the working zone between the two danger regions is very narrow; and even with improved methods of calculation it would be unsafe to rely upon keeping within this narrow margin at all loads. Such cases depend for safety not upon the avoidance of resonance but upon the use of dampers sufficient to prevent the machine responding excessively. Such machines usually behave badly at light load or in any condition where the impulses are irregular. The degree of satisfaction obtained with this method appears very limited. As to the effect of the new method of calculation upon the diagram which Dr. Rosenberg reproduces, the principal difference should be found in the width of the danger zone representing the condition from no load to maximum load. Apparently the diagram was laid out on the assumption that the synchronizing power increases directly with the excitation, while in the present method a much smaller increase appears proper, as explained under "The effect of load." This should increase somewhat the width of neutral space between the two danger zones, but not sufficiently to avoid the objectionable conditions already referred to. Mr. Martin also refers to this matter.

Mr. Everest.

Professor Threlfall asks whether the natural frequency of a machine changes every time a new unit is added to the system. Any number of similar machines could be connected together without changing the critical frequency at which each one of them is inclined to resonate; but if the machines are not similar in character new conditions will give new natural frequencies, as discussed under the heading "Dissimilar Machines."

The type of alternator discussed by Mr. R. H. Bradbury is quite special, since, as he shows, it is designed to give constant power, the pressure falling as the current increases. Evidently the natural frequency will vary considerably with such variations in voltage, and more difficulties in parallel operation should be expected than with an alternator having a practically constant value of flux.

Proceedings of the Five Hundred and Fiftieth Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 27th February, 1913—Mr. W. DUDDELL, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 13th February, 1913, were taken as read, and confirmed.

The list of candidates for election and transfer approved by the Council for ballot was taken as read, and it was ordered that it should be suspended in the Hall.

Donations to the *Library* were announced as having been received since the last meeting from The Adams Manufacturing Company, Ltd., Dr. H. Borns, Messrs. Brown, Boveri & Co., Ltd., Messrs. Constable & Co., W. R. Cooper, Messrs. Crosby, Lockwood & Son, The Dussek Bitumen Company, L. Gaster, B. Gáti, J. W. F. Hoffman, Mrs. Andrew Jamieson, H. R. Kempe, Dr. A. E. Kennelly, W. P. Maycock, R. K. Morcom, W. H. Patchell, W. E. Schall, and S. P. Smith, to whom the thanks of the meeting were duly accorded.

The PRESIDENT: I have to announce that Mr. Jenkin has presented to the Institution a medallion portrait of Lord Kelvin, which was modelled from life by Miss Giles, now Mrs. Jenkin. I think it is very appropriate indeed that we should have had this presentation of such an excellent portrait of Lord Kelvin on the occasion of one of our Kelvin Lectures. I ask you to give a most hearty vote of thanks to Mr. Jenkin for his presentation.

The vote of thanks was heartily accorded.

Dr. R. T. GLAZEBROOK, C.B., F.R.S., Past President, then delivered the Fourth Kelvin Lecture (see page 560), and the meeting adjourned at 9.35 p.m.

THE FOURTH KELVIN LECTURE.

THE OHM, THE AMPERE, AND THE VOLT: A MEMORY OF FIFTY YEARS, 1862–1912.

By Dr. R. T. GLAZEBROOK, C.B., F.R.S., Past President.

(*Delivered 27th February*, 1913.)

My first words must be to express my thanks to you, Mr. President, and to the Council for your renewed invitation to deliver this lecture, to fulfil the welcome task of speaking to an audience which realizes very fully all we owe to Lord Kelvin, of some small portion of his work.

I cannot claim the many-sided knowledge of that work possessed by the first Kelvin lecturer—the distinguished author of his biography—nor the intimate acquaintance with detailed portions acquired by one who, like Sir Alfred Ewing or Professor du Bois, had worked under him in his own laboratory; but for years I was his disciple and friend. May I recall some personal memories?

I began to work at practical physics in 1876. The Cavendish Laboratory had not long been opened. Maxwell was equipping it with new apparatus, and almost my first task was to trace the behaviour, under various rates of discharge, of a battery of large tray Daniell cells designed by Thomson. The instrument given me to measure the E.M.F. was one of the large-pattern White quadrant electrometers with the mouse-mill replenisher. It had to be set up and adjusted, and for a novice the task did not prove easy. We were not fed with a spoon by Maxwell, but fortunately I had no examination in view. Since that time I have shunned the instrument and felt a holy reverence for its designer.

Again, a few years later with various friends I went to Largs after a British Association meeting. I had recently become Secretary of the Electrical Standards Committee. While we were there a parcel arrived from America which proved to be one of the first concave gratings sent to England, and I was told off to set it up in the drawing-room after dinner. The slit was a narrow cut in a visiting card. It stood on the piano in front of a spirit lamp with a salted flame. The grating was placed on the mantelpiece, and a circle on which the images lay was roughly marked out on the floor with footstools and cushions. The party was set to work to find the D lines. Those of the first two orders were seen by all. "We were delighted with the result:

I had never seen anything like it before," wrote Sir William in a letter to Professor Mendenhall.

Again, a little later, he was in Cambridge, and I met him at dinner at Professor Stokes's house. He was very full of ether theories, and specially interested in a labile or contractile ether whose properties he had recently described in the *Philosophical Magazine*. There were certain difficulties in the theory, and I suggested how some of these might be met. He was delighted, and took me at once to Lady Thomson and explained in his own enthusiastic way what I had done. A few days later there appeared in the *Philosophical Magazine* a note dated Train, Cambridge to Glasgow, which begins: "Yesterday evening, in Cambridge, Mr. Glazebrook pointed out to me . . . He promises a paper on the subject for the December number of the *Philosophical Magazine*."

To-day the theory of the labile ether has but little interest for any. Its films have contracted and elasticity has gone. You will understand the value of Lord Kelvin's generous appreciation to one who was just beginning to taste the pleasures of exact research and accurate experiment. From that time he was my friend until the day in the Abbey when as the representative of the Institution I was one of those who bore our greatest President to his long home and the mourners went about the streets.

A Kelvin lecturer, at any rate at present, has an ample choice of subject, and for me the selection is obvious. The volume on the table contains a reprint of the Reports of the Electrical Standards Committee of the British Association, which in great measure through the generosity of Mr. R. K. Gray have been issued to commemorate the work of Thomson and his colleagues in putting the science of electrical measurement on a firm basis. I propose to give some account of this work; to trace briefly the history of the ohm, the ampere, and the volt from the days of the first Atlantic cable, when there were no standards of resistance and the simplest applications of Ohm's law to practical problems were hardly known, up to the present time when resistances, currents, and electromotive forces are compared with an accuracy attained in hardly any other science. I shall try not to weary you with details, but I hope to impress on you the enormous debt we owe to these early pioneers whose clear-sighted vision has done so much to render possible the modern uses of electricity.

I think it was Thomson himself who compared somewhere the growth of the steam engine and the dynamo. The rapid progress of the latter depends in no small measure on the fact that the electrical quantities which occur in its theory are all capable of exact measurement, and that by an application of the fundamental laws we owe to Faraday, to Hopkinson, and to Ewing, its theory can be worked out and the results predicted with an accuracy which even now cannot be reached for the steam engine, and which until the days of Joule and Thomson was quite impossible.

Thanks to the kindness of many friends, there is on the table a most valuable collection of historical apparatus about which all of you have

read but which few have seen. You know the pictures, perhaps, of Thomson's rotating coil or of Joule's first ampere balance, or of the early forms of Wheatstone bridge: here they are to be seen at least once in a lifetime by all.

The Electrical Standards Committee of the British Association was appointed at the Manchester meeting in 1861 as the result of a paper by Sir Charles Bright and Mr. Latimer Clark proposing names for the standards of resistance, current, electromotive force, and quantity. The volt was to be the standard of resistance, the ohma of electromotive force. Thomson moved for a Committee "For Improving the Construction of Practical Standards of Electrical Measurements." In its original form the Committee consisted of Thomson, Williamson, Wheatstone, W. H. Miller, Matthiessen, and Fleeming Jenkin. To these Maxwell, Siemens, Joule, and others, were added in the following year. This first Committee lasted unto 1870. Professor Carey Foster, appointed in 1867, is its sole surviving representative.

The paper by Mr. Latimer Clark and Sir Charles Bright is printed in the *Electrician*, vol. 1, p. 3, 9th November, 1861, and is very interesting reading. The comments of the Editor, in view of our present knowledge, are perhaps more interesting. Thus he writes: "To be of any general utility, however, the proposed system of measurement must necessarily be sufficiently simple and easy of application, to meet the requirements of telegraphists. Glancing at what has already been published in reference to this important subject, we fear there is some danger that a system may be devised which will be followed exclusively by the eminent gentlemen at whose recommendation it is put forward. That this would be worse than useless, in a practical point of view, need scarcely be insisted upon." And so forth; while Latimer Clark himself, in a letter to the Editor, 17th January, 1862, writes: "The gentlemen who constitute the Committee to report to the British Association are but little connected with practical telegraphy, and there is a fear that while bringing the highest electrical knowledge to the subject, and acting with the best motives, they may be induced simply to recommend the adoption of Weber's absolute units, or some other units of a magnitude ill adapted to the peculiar and various requirements of the electric telegraph."

The units proposed in the paper were, for electromotive force, the E.M.F. of a Daniell cell, "which will probably be found sufficiently constant for this purpose"; and, for quantity, the charge on one plate of a condenser consisting of two parallel plates 1 square metre in area and 1 millimetre apart, with air for the dielectric, when connected to the poles of a Daniell cell.

Engineers present will realize what their position would now be had these simple practical units been chosen instead of the absolute units "ill adapted to telegraphy," and will estimate the debt due to Thomson and his colleagues accordingly.

The Electrical Standards Committee in their earlier Reports discuss —so far as measurements of resistance are concerned—two distinct

questions, and it is well to keep them distinct. They state that they had first to determine what would be the most convenient unit of resistance, and, second, what would be the best form and material for the standard representing that unit. With regard to the first point, two courses lay open to the Committee, viz. to adopt the absolute system of units devised by Weber and employed by Thomson since 1851, or to take as the unit the resistance of some definite portion of a material substance, *e.g.* a column of mercury. The choice was soon made. Led by Thomson, the Committee, at a meeting at which all the members but one were present, decided on the absolute system, and the choice has done more than perhaps any other single act to simplify and unify electrical measurements throughout the world.

One word as to the term "absolute." It is used, says the Committee, "in the present sense as opposed to the word relative, and by no means implies that the measurement is accurately made or that the unit employed is of perfect construction; in other words, it does not mean that the measurements or units are absolutely correct, but only that the measurement, instead of being a simple comparison with an arbitrary quantity of the same kind as that measured, is made with reference to certain fundamental units of another kind treated as postulates."

Weber, in his great work *Elektrodynamische Maassbestimmungen*, Part II, published in 1852, had put the matter clearly. In mechanics, he says, fundamental units are adopted only for length, time, and mass; the units of all other quantities considered in mechanics are defined in terms of these few fundamental units and are known as absolute units. To measure a resistance absolutely is to measure it in terms of the units of length and of time.

The three electrical units, of current, electromotive force, and resistance, are connected by Ohm's law. Two of them, *e.g.* current and electromotive force, can be defined independently in terms of the fundamental units; the third then follows naturally.

The system devised by Weber possesses another quality which the Committee had laid down as necessary. The units bear a definite relation to the unit of work, for the unit current when traversing a conductor of unit resistance does a unit of work, or its equivalent, in a unit of time. Thomson pointed this out at an early date.

For the units of length, mass, and time, the centimetre, gramme, and second were ultimately selected—the Committee first suggested the metre as the unit of length, while Weber used the millimetre—and the C.G.S. system of units was born. Thanks to this, electricians understand each other's terms throughout the world. Compare its simplicity with the confusion from which it rescued us.

The following table (see page 564), prepared by the Committee, shows the different units of resistance used in 1864; and some of the old standards employed before the introduction of the British Association unit, or Ohmad as it was originally called, are on view to-night.

An International Conference on Electrical Units is, even now,

somewhat confusing. The President of this Institution, who by his work as secretary contributed so much to the success of the Conference

TABLE I.

Description.	Name.	Absolute $\frac{\text{foot}}{\text{second}} \times 10^7$
Absolute $\frac{\text{foot}}{\text{second}} \times 10^7$ electromagnetic units (new determination)	Absolute $\frac{\text{foot}}{\text{second}} \times 10^7$	1·000
Absolute $\frac{\text{foot}}{\text{second}} \times 10^7$ electromagnetic units (old determination)	Thomson's unit ...	1·0505
Twenty-five feet of a certain copper wire, weighing 345 grains	Jacobi	2·088
Absolute $\frac{\text{metre}}{\text{second}} \times 10^7$ electromagnetic units determined by Weber (1862)	Weber's absolute $\frac{\text{metre}}{\text{second}} \times 10^7$ ...	3·015
One metre of pure mercury, one square millimetre section, at 0° C.	Siemens's 1864 issue	3·138
One metre of pure mercury, one square millimetre section, at 0° C.	Siemens (Berlin) ...	3·156
One metre of pure mercury, one square millimetre section, at 0° C.	Siemens (London) ...	3·194
British-Association unit	B.A. Unit, or Ohmad	3·821
One kilometre of iron wire, four millimetres in diameter (temperature not known)	Digney	30·40
One kilometre of iron wire, four millimetres in diameter (temperature not known)	Bréquet	32·03
One kilometre of iron wire, four millimetres in diameter (temperature not known)	Swiss...	34·21
One English standard mile of pure annealed copper wire $\frac{1}{16}$ in. in diameter at 15·5° C.	Matthiessen	44·57
One English standard mile of one special copper wire $\frac{1}{16}$ in. in diameter	Varley	84·01
One German mile = 8,238 yards of iron wire $\frac{1}{6}$ in. in diameter (temperature not known)	German mile ...	188·4

of 1908, can imagine what his labours would have been if he had had to deal with the fourteen units of the above table.

Table II, taken from the Report of the Government Committee on Submarine Cables, and printed in the first Report (p. 335), gives a comparison of the resistance of various specimens of copper and illustrates the difficulties consequent on the absence of proper standards.

TABLE II.

(All the wires were annealed.)

	Conducting Power.
Pure copper	100·0 at 15·5
Lake Superior native, not fused	98·8 „ 15·5
Ditto, fused, as it comes in commerce ...	92·6 „ 15·0
Burra Burra	88·7 „ 14·0
Best selected	81·3 „ 14·2
Bright copper wire	72·2 „ 15·7
Tough copper	71·0 „ 17·3
Demidoff	59·3 „ 12·7
Rio Tinto	14·2 „ 14·8

So far the work had been fairly simple. The absolute system had been adopted, and it was settled that 10^9 C.G.S. units of resistance was a convenient multiple of the unit for practical work, and that this should be called an ohmad or ohm; but how was a standard having this value to be realized? Of what material was it to be constructed, and when made, how was its value to be determined in terms of the unit?

The first problem was left to Dr. Matthiessen. As to the second, Weber had, in 1852, suggested various means by which the resistance of a wire could be measured absolutely, and in his paper had described the results arrived at by three methods for the resistance of a certain wire, as follows :—

Weber's 1st method	...	1903×10^8 millimetre/second
„ 2nd „	...	1898 „ „ „
„ 3rd „	...	1900 „ „ „

Maximum difference = 0·25 per cent.

An extreme difference of 5 in 1900 is no mean achievement for the first attempt, but the result differed by about 8 per cent from that found later by the Committee.

Thomson and Fleeming Jenkin were asked to make a new determination and construct standards each having a resistance of 1 ohm. The apparatus was designed by Thomson and built by White under his supervision. The method had been indicated generally by Weber a few months previously in his paper "Zur Galvanometrie," January, 1862, but the Committee do not appear to have been aware of this. The experiments were conducted at King's College by Maxwell, Fleeming Jenkin, and Balfour Stewart, then my predecessor as Superintendent of the Kew Observatory.

The apparatus is on the table. It consists, as is well known, of a circular coil which can rotate about a vertical axis. This generates an electromotive force in the coil, and the current produced is measured by the ratio of this electromotive force to the resistance. A magnet is hung at the centre of the coil, and the current is also measured by its deflections. Equating these two values for the current, we find an expression for the resistance in terms of quantities which can be measured. Let me draw your attention to the formula—

If $n =$ number of turns of wire on the coil;
$a =$ radius of the coil;
$N =$ number of turns per second made by the rotating coil;
$\theta =$ deflection of the magnetic needle at the centre of the coil;

we have $R = 2 n^2 \pi^3 N a \cot \theta$.

This neglects the effect of the self-induction of the coil and other corrections which in the aggregate are considerable. Thus if we know n and a and observe N and θ we can find R; and R is determined, be it noted, in terms of a length a and N the number of complete rotations per second, which is the reciprocal of a time. R is given absolutely in terms of the units of length and time.

The experiments were repeated in 1864 by Maxwell, Fleeming Jenkin, and Hockin. Thus the absolute resistance of a certain coil was determined, ohm coils could be compared with this by known methods, and the British Association standards were made thus.

Table III gives the results of the 1864 experiments, and the comparison with those of 1863.

In constructing the standard coil, in consideration of the much greater range of velocities used in 1864, the 1864 mean value was allowed to have five times the weight of the mean value obtained in 1863.

For the purpose of comparing two coils of nearly equal resistance the Committee used a modification of the Wheatstone bridge, due to Jenkin and indicated in Fig. 1. The bridge, made, I believe, by Messrs. Elliott Brothers for the work, is before you. It was employed recently, with quite satisfactory results, by Mr. F. E. Smith at the National Physical Laboratory to compare two coils. By its aid the Committee compared the resistance of the copper wire of the spinning coil with that of a german-silver coil before and after each spin.

Meanwhile, Matthiessen and Hockin had been at work determining the material of which the British Association Standards were to be made, and as the result of numerous experiments an alloy of 66 per cent silver and 33 per cent platinum was selected. The reasons for this choice are given in an appendix to the fourth Report, 1866, together with an account of the means taken to compare the standards with the german-silver coils used in the absolute observation. A number of coils were made and distributed to public authorities and others. Some were sold; Dr. Faraday, on behalf of the Royal Institution, was the first purchaser. The unit coils of various materials, and also two

mercury units, were prepared to be deposited at the Kew Observatory, and it is stated that "anyone possessing a copy of the British Association Unit may have it compared at any future time against one of these coils for a small payment."

Some of these coils are on the table. They were taken by Clerk Maxwell to Cambridge, and formed the basis of the work of Chrystal

TABLE III.

Time of 100 Revolutions, in Seconds.	Values found for Coil in terms of 10^7 for each Experiment.	Value of B.A. Unit in Terms of $10^7 \frac{\text{metre}}{\text{seconds}}$, as calculated from each Experiment.	Value from Mean of each Pair of Experiments.	Percentage Error from Mean Value.
17·54	4·7201	1·0121	0·9978	−0·22
17·58	4·5914	0·9836		
77·62	4·8848	1·0468	1·0040	+0·40
76·17	4·4871	0·9613		
53·97	4·6607	0·9985	0·9992	−0·08
54·53	4·6666	0·9998		
41·76	4·6279	0·9915	0·9925	−0·75
41·79	4·6275	0·9936		
54·07	4·6496	0·9961	0·9924	−0·76
53·78	4·6146	0·9886		
17·697	4·6108	0·9878	1·0007	+0·07
17·783	4·7313	1·0136		
17·81	4·6452	0·9952	1·0063	+0·63
17·78	4·7489	1·0174		
17·01	4·7567	1·0191	1·0043	+0·43
16·89	4·6187	0·9895		
21·35	4·6834	1·0034	1·0022	+0·22
21·38	4·6727	1·0011		
21·362	4·6526	0·9968	1·0040	+0·40
21·643	4·7134	1·0096		
11·247	4·8658	1·0424	0·9981	−0·19
16·737	4·5305	0·9707		

Probable error of R (1864) = 0·1 per cent.
Probable error of R (1863) = 0·24 per cent.
Difference in two values 1864 and 1863 = 0·16 per cent.
Probable error of two experiments = 0·08 per cent.

and Saunder, Fleming, Rayleigh and Schuster, and myself at the Cavendish Laboratory. For the past thirty years they have been in my charge. In 1900 they came at last to the place for which they were originally intended—the Kew Observatory, the first home of the National Physical Laboratory—and since that time have been one of the treasured objects of Mr. F. E. Smith's care. They are, I think, the oldest set of accurate electrical standards now in existence, the type and forerunner of many others. Contrast the unity which they have

brought with the confusion of 1860. May we not say: "These be thy gods, O Israel, which have brought thee out of the land of Egypt and out of the house of bondage."

During their long history they have been intercompared many times, and the results afford interesting evidence as to the permanence of the materials of which they are made. Table IV gives the results of Matthiessen and Hockin's work. The main intercomparisons at later

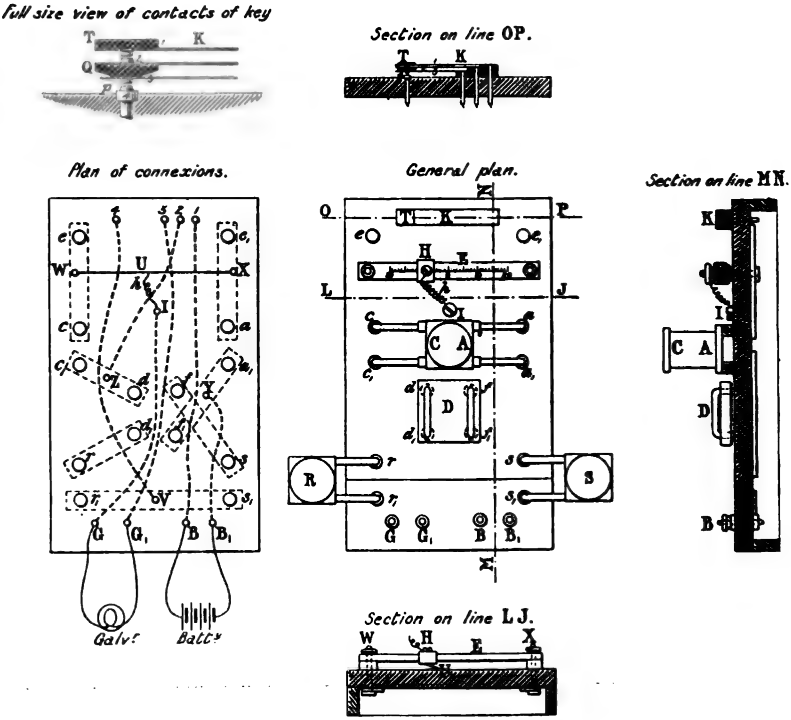


FIG. 1.—Fleeming Jenkin Resistance Bridge.

dates were made by Messrs. Chrystal and Saunder in 1876, Dr. Fleming in 1878–81, myself in 1888, and Mr. F. E. Smith in 1908. A careful discussion of the results leads to the conclusion that with the exception of the two platinum coils known as D and E, Nos. 35 and 36 of Hockin's Table, all the others have changed. Table V gives the results obtained on this assumption. It includes seven of the original coils and the two other platinum-silver coils F and Flat. In Table VI the values of the coils, expressed in terms of the resistance of mercury, are given for two dates separated by a period of 27 years. This leads to the same result as to the permanence of the platinum coils.

TABLE IV.

Material of Coil.	No. of Coil.	Date of Observation.	Temperatures at which coil has a resistance $= 10^7 \frac{m}{s}$	Observer.
			°C.	
Platinum-iridium alloy	2	4th January, 1865	15·5	C.H.
		6th June, 1865	16·0	A.M.
		10th Feb., 1867	16·0	C.H.
Platinum-iridium alloy	3	4th January, 1865	15·3	C.H.
		6th June, 1865	15·8	A.M.
		10th Feb., 1867	15·8	C.H.
Gold-silver alloy ...	10	5th January, 1865	15·6	A.M.
		10th Feb., 1867	15·6	C.H.
Gold-silver alloy ...	58	10th April, 1865	15·3	A.M.
		6th June, 1865	15·3	A.M.
		10th Feb., 1867	15·3	C.H.
Platinum	35	7th January, 1865	15·7	C.H.
		18th August, 1866	15·7	A.M.
		10th Feb., 1867	15·7	C.H.
Platinum	36	7th January, 1865	15·5	C.H.
		18th August, 1866	15·5	A.M.
		10th Feb., 1867	15·7	C.H.
Platinum-silver alloy	43	15th Feb., 1865	15·2	C.H.
		9th March, 1865	15·2	A.M.
		10th Feb., 1867	15·2	C.H.
Mercury	I.	2nd Feb., 1865	16·0	A.M.
		18th July, 1866	16·0	A.M.
		11th Feb., 1867	16·7	C.H.
Mercury	II.	3rd Feb., 1865	14·8	A.M.
		18th August, 1866	14·8	A.M.
		11th Feb., 1867	14·8	C.H.
Mercury	III.	11th Feb., 1867	17·9	C.H.

TABLE V.

Resistances at 16·0° C. in terms of the original B.A Unit (1867).

(*Values obtained through the two Platinum Coils D, E.*)

Coil.	Material.	1867.	1876.	1879–81.	1888.	1908.	Maximum Difference.
A	Pt Ir	1 00000	1·00077	1·0 056	1·00147	1·00122	147 × -10^5 B.A Unit
B	Pt Ir	1·00029	1·00121	1·00080	1·00104	1·00098	92 ,,
C	Au Ag	1·00050	1·00141	1·00101	1·00146	1·00173	123 ,,
D	Pt	1·00092	1·00092	1·00092	1·00092	1·00092	0 ,,
E	Pt	1·00152	1·00152	1·00152	1·00152	1·00152	0 ,,
F	Pt Ag	—	—	1·00016	1·00072	1·00160	144 ,,
G	Pt Ag	1·00022	1·00030	0·99982	1·00025	1·00175	193 ,,
H	Pt Ag	1 00020	—	—	1·00042	1·00044	24 ,,
Flat	Pt Ag	—	—	1·00079	1·00120	1·00125	46 ,,

But the history of the British Association coils is perhaps a digression: we are dealing with absolute units.

The original Committee was dissolved at its own request in 1870. During the next ten years further measurements of absolute resistance were made, notably by Kohlrausch and Rowland, and these seemed to show that the Committee's determination was some 1 per cent too low. The experiments of Joule, to be referred to later, led to the same result, and a similar conclusion may be deduced from Matthiessen's determination of the resistance of mercury. He states that the resistance of a metre-gramme of mercury is 13·06 B.A. units, while we now know

TABLE VI.

Giving the Values at 16·0° C. of certain Coils in cm. of Mercury in 1881, 1888, and 1908, obtained from comparisons with Mercury Standards.

Coil.	1881. Values deduced from Lord Rayleigh's determination of the specific resistance of mercury. F and Flat were used; for relative values of coils see Table V.	1888. Values at time of Dr. Glazebrook's determination. F, G, and Flat were used; for relative values of coils see Table V.	1908. Values directly determined through N.P.L. mercury standards of resistance.	Maximum Difference.
	Cm.	Cm.	Cm.	Cm.
A	104·847	104·946	104·918	0·071
B	104·872	104·901	104·893	0·029
C	104·894	104·945	104·972	0·078
D	104·885	104·888	104·887	0·003
E	104·948	104·951	104·950	0·003
F	104·805	104·843	104·959	0·154
G	104·769	104·807	104·974	0·205
H	—	104·836	104·837	0·001
Flat	104·871	104·898	104·922	0·051

that this quantity is approximately 12·79 ohms. Thus 1 B.A. unit = 0·979 ohm, or the B.A. unit is some 2 per cent less than its nominal value.

Accordingly, on the motion of Professor Ayrton, the Committee was reappointed in 1880, and a series of redeterminations took place both in England and abroad. An international congress, the first of its kind, was to be held in Paris in 1881; and that year at the Jubilee meeting of the Association the first report of the new Committee was presented, and much discussion took place as to proposals to be made at Paris. Lord Rayleigh, and Schuster, had already shown by observations with old British Association apparatus that the original result was wrong by 1·2 per cent, and Lord Rayleigh was at work with a new coil. The Congress met, with Mascart and Eric Gérard as secretaries.* Sir William took a prominent part in its discussions. He proposed

* *Nature*, vol. 24, p. 512, 1881.

the substantial adoption of the absolute system of the British Association; and after speeches by Wiedemann and Helmholtz, who favoured a mercury unit of resistance, the matter was adjourned and referred to a Committee. During the interval a compromise was arranged by the efforts of Mascart and Mr. (now Lord Justice) Moulton, and it was agreed that the fundamental units for electrical measurements should be those of the C.G.S. system—the centimetre, the gramme, and the second, while the practical units should be the ohm (10^9 C.G.S. units) and the volt (10^8 C.G.S. units). The name ampere was given to the current produced by a volt in a wire of resistance 1 ohm, 10^{-1} C.G.S. units, and it was agreed (1) that the unit of resistance 1 ohm should be represented by a column of mercury 1 square millimetre in section at the freezing-point of water, and (2) that an International Commission should be charged with the duty of determining by new absolute measurements the length of this column. Sir William, says his biographer, was the life and soul of the conference, and it was due to him that the absolute C.G.S. system was thus adopted internationally.

The article in *Nature* describing the work and dealing with the production of mercury standards concludes: "The German authorities assert that accuracy to one part in two thousand can thus be secured."

A number of determinations followed, and the Paris Congress met again in the spring of 1884. Mascart presented a table giving all the results then known for the length of the mercury column; the mean was 106·02 centimetres; and the first resolution of the Conference was: "The legal ohm is the resistance of a column of mercury a square millimetre in section and 106 centimetres in length at the temperature of melting ice."

Thomson had been one of the English representatives along with Preece, Carey Foster, and others, and had wished for the adoption of a figure more nearly in accord with recent Cambridge work. To adopt the mean of all known results, differing, as they did, among themselves by over 2 per cent, was far from satisfactory, especially as Rowland was then at work on the question and his results were unpublished. Accordingly, the "legal ohm" was never formally adopted here. By 1890 the question of establishing legalized standards in England had become ripe for settlement, and the Board of Trade appointed a Committee on Standards for the Measurement of Electricity for Use in Trade. The Committee consisted of Mr. Courtenay Boyle, Major Cardew, Mr. Graves, Mr. Preece, Sir William Thomson, Lord Rayleigh, Dr. Hopkinson, Professor Aryton, Professor Carey Foster, and myself.

Table VII and the top portion of Table VIII show the values available to guide their decision; and as the result they adopted as the resistance of the British Association unit the value 0·9866 ohm, and 106·3 centimetres for the length of the mercury column having a resistance of 1 ohm at 0° C. From these it follows that the resistance of 1 ohm in British Association units is given by—

$$1 \text{ ohm} = 1{\cdot}01358 \text{ B.A. units.}$$

TABLE VII.

	Observer.	Date.	Method.	Value of B.A. units in Ohms.	Value of 100 cm. of Mercury in B.A. units.	Value of Ohm in cm. of Mercury.
1	Lord Rayleigh ...	1882	Rotating coil	0·98651	(0·95412)	106·31
2	Lord Rayleigh ...	1883	Lorenz method	0·98677	—	106·27
3	Mascart	1884	Induced current	0·98611	0·95374	106·33
4	Rowland	1887	Mean of several methods... ...	0·98644	0·95349	106·32
5	Kohlrausch ...	1887	Damping of magnets	0·98660	0·95338	106·32
6	Glazebrook ...	1882 and 1888	Induced currents	0·98665	0·95352	106·29
7	Wuilleumeier ...	1890	—	0·98686	0·95355	106·31
8	Duncan and Wilkes	1890	Lorenz method	0·98634	0·95341	106·34
9	Jones	1891	Lorenz method	—	—	106·31
			Mean	0·98653		106·31
10	Strecker	1885	An absolute determination of resistance was not made. The value 0·98656 has been used	—	0·95334	106·32
11	Hutchinson ...	1888		—	0·95352	106·30
12	Salvioni	1890		—	0·95332	106·33
12	Salvioni	—		—	0·95354	106·30
			Mean	... ...	0·95354	106·31
13	H. F. Weber ...	1884	Induced current			105·37
14	H. F. Weber ...	—	Rotating coil	Absolute measurements compared with german-silver wire coils issued by Siemens or Strecker		106·16
15	Roiti	1884	Mean effect of induced current			105·89
16	Himstedt	1885	—			105·98
17	Dorn	1889	Damping of a magnet			106·24
18	Wild	1883	Damping of a magnet			106·03
19	Lorenz	1885	Lorenz method			105·93

Part of the apparatus used in the English determinations is before you, and Fig. 2 gives a view of Lord Rayleigh's apparatus. In explanation of the error made by the original Committee it should be stated that Lord Rayleigh had shown that it almost certainly arose from the assumption of a wrong value for the correction due to the self-inductance of the coil.

With a view to securing international agreement, publication of the Report of the Board of Trade Committee was deferred to the autumn, and the resolutions were discussed by the British Association, at Edinburgh, in 1892. Helmholtz, at Thomson's invitation, came over for the meeting, while Lindeck and Kahle, Dr. Guillaume and Professor Carhart also attended; and with a slight alteration in the form of words defining the mercury column, they were agreed to with the full concurrence of all. Sir William, who had become Lord Kelvin early in the year, was present and gave his valuable help.

TABLE VIII.

Value of the Ohm expressed as the Resistance of a Column of Mercury.

Lord Rayleigh ...	1882	Rotating coil	106·31
Lord Rayleigh ...	1883	Lorenz	106·27
Mascart	1884	Induced current	106·33
Rowland	1887	Mean of several methods	106·32
Kohlrausch ...	1887	Damping of magnets	106·32
Glazebrook ...	1882	Induced currents	106·29
Glazebrook ...	1888		
Wuilleumier ...	1888	—	106·31
Jones	1891	Lorenz	106·31
Jones	1892	Lorenz	106·32
Ayrton and Jones ...	1897	Lorenz	106·27
Guillet	1899	Induced currents	106·21
Campbell	1912	Alternating currents	106·27
		Mean	106·29

Next year, 1893, the Board of Trade Committee drew up an amended report to agree verbally with the Edinburgh resolutions, and at the International Conference in Chicago, held in August, 1893, under the presidency of Helmholtz, definitions were accepted for the international units in accordance with the same, while in August, 1894, the English Order in Council defining new denominations of standards for use in electricity under the Weights and Measures Act was approved by the Queen in Council. Resistance coils which had been compared with the British Association standards at Cambridge were set up at the

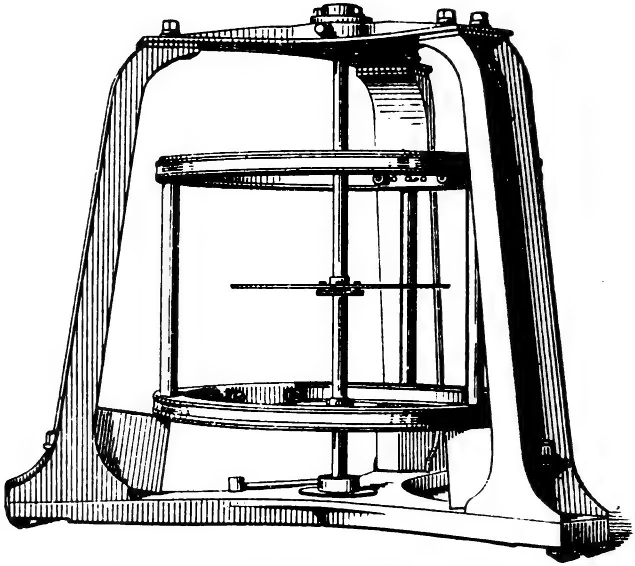


FIG. 2.—Lord Rayleigh's Lorenz Apparatus.

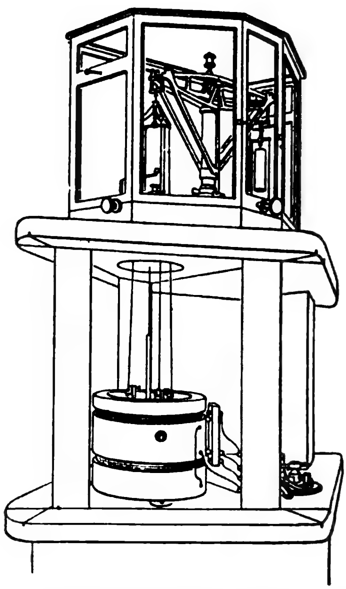


FIG. 3.—Board of Trade Current Balance.

Board of Trade Laboratory as the standard of electrical resistance, and a balance, calibrated by means of the silver voltameter, was constructed as a standard of current. This is shown in Fig. 3.

Leaving for the present the history of the unit of resistance for the last 18 or 20 years, let us turn to that of the other two units—those of current and electromotive force: it can be more brief. Only two of the three units are independent, and for a time it was not clear whether current or electromotive force should be chosen as the second. Weber indeed, in 1851, had selected these as the two fundamental units, defining them, however, in a manner that depended on the strength of the earth's magnetic field, which Gauss had shown in 1833 could be measured absolutely.

The relations between the various quantities occurring in electrical measurements are discussed in Appendix C to the second Report of the

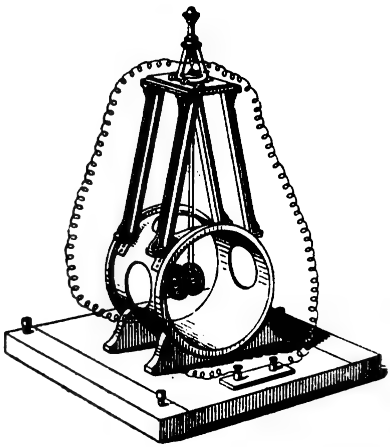


FIG. 4.—Weber's Electrodynamometer.

Committee by Clerk Maxwell and Fleeming Jenkin, and their statement should be carefully studied by every student. Electric currents, they point out, can be measured (1) by their action on a magnetic needle; (2) by their mutual action on each other; and (3) by their chemical effects. The first method is exemplified by the sine or tangent galvanometer, the second by the electrodynamometer devised by Weber (Fig. 4), or the current balance, and the third by the voltameter.

The first method is not suitable for absolute measurement because of the difficulty of determining H sufficiently accurately. The third involves the relation between the current and the amount of chemical action produced in the voltameter, and is therefore not an absolute measure; and we are thus left with the second—the electrodynamometer and the current balance. There are before you the earliest

current balance and one of the earliest tangent galvanometers. They were used by Joule in his celebrated experiment.

This experiment was the well-known one of measuring the mechanical equivalent of heat by electrical means: it is described in the fifth Report of the Committee, Dundee, 1867, Appendix VI. Thomson had shown in 1851 how the results could be obtained. It was necessary, however, to know the resistance of a wire in absolute measure before they could be completed.

The result for the equivalent, assuming the British Association unit to be 10^9 C.G.S. units of resistance, was $42{\cdot}119 \times 10^6$ C.G.S. units, while the value found from the stirring of water was $41{\cdot}586 \times 10^6$ C.G.S. units. The agreement was thought to be very satisfactory by the Committee. They write, after stating that Joule thought the electrical method more accurate than the frictional:—

"Meanwhile the experiments already completed remove all fear of any serious error either in the number hitherto used as Joule's equivalent or in the British Association standard—a fear which hitherto, remembering the very discrepant results obtained by others, has been very naturally entertained even by the Sub-Committee from whose experiments the standard was constructed."

We know now that both Joule's figures were more nearly correct than the work of the Sub-Committee. The slight difference arose from the fact that the British Association unit is not exactly 10^9 C.G.S. units, and the ratio of the two figures just stated gives the value of the British Association units in ohms. This ratio is 0·9873, agreeing very closely with the figure 0·9866 which we have seen was accepted in 1893 as the value.

There is also on the table part of another electrodynamometer, that made for the Committee and referred to in the Reports for 1864 and 1865 and briefly described in that for 1869. Its coils were used by Lord Rayleigh in some experiments to be described later.

As to the volt, no direct means exist for determining it in absolute measure, at least if we exclude electrostatic measurements. Its measurement involves that of a current and a resistance, and the Committee in their second Report, 1863, suggests the use of a Daniell cell when the value of its E.M.F. is known in absolute units. The Daniell cell for this purpose has been replaced by the Clark cell (*Proceedings of the Royal Society of London*, vol. 20, p. 444, 1872) and, more lately, by the Weston cell. Up to 1881 the volt was retained in the second place as the second independent unit defined as 10^8 C.G.S. units of E.M.F., while the ampere was the current produced by a volt acting on an ohm. In 1884 this was altered; the ampere took the second place, defined as 10^{-1} C.G.S. units, while the volt was the E.M.F. required to maintain an ampere in a resistance of 1 ohm.

During the interval, current balances had been set up by Mascart and Rayleigh, who had measured the mass of silver deposited in one second by a current whose value was determined absolutely by the balance. This work was repeated by Kohlrausch and others, and it

was recognized that a current of 1 ampere deposited per second from a solution containing silver about 0·001118 gramme of silver, provided certain conditions as to the voltameter and the solution were complied with. Knowing this figure, it became possible to measure currents or to standardize ammeters by the use of the silver voltameter. Again, using this figure to standardize a current and the known value of a suitable resistance, the E.M.F. of a cell can be measured; and it was found that the Clark cell set up with proper precautions had an E.M.F. of 1·434 volts at 15° C.

TABLE IX.

Electrochemical Equivalent of Silver.

		Mgm. per Sec.
1884	Rayleigh and Sidgwick	1·1179
1884	Kohlrausch	1·1183
1884	Mascart	1·1156
1890	Pellat and Potier	1·1192
1898	Kahle	1·1183
1898	Patterson and Guthe	1·1192
1903	Pellat and Leduc	1·1195
1904	Van Dijk and Kunst	1·1182
1906	Guthe	1·1182
1907	Smith, Mather, and Lowry	$1·1182_7$
1908	Laporte and de la Gorce	$1·1182_9$
1912	Rosa, Dorsey, and Miller	$1·1180_4$

TABLE X.

E.M.F. of Clark Cell at 15° C.

		Volts.
1872	Clark	1·4378
1884	Rayleigh and Sidgwick	1·4345
1896	Kahle	1·4322
1899	Carhart and Guthe	1·4333
1905	Guthe	1·4330
1907	Ayrton, Mather, and Smith	1·4323
	Indirect Determinations of E.M.F. of Clark Cell.	
1884	Von Ettinghausen	$1·433_5$
1885	Rayleigh	1·435
1892	Glazebrook and Skinner	1·434
1904	Trotter	$1·432_9$

Lord Rayleigh's experiments at Cambridge, to which there are many references in Kelvin's life, were largely instrumental in establishing these results, and there are shown to-night parts of his current balance and some of the cells used by him; also cells constructed by

Principal Griffiths for his determination of Joule's equivalent. Fig. 5 gives a view of the balance. These various figures (given in the upper parts of Tables IX and X) then formed the basis of the English Order in Council of 23rd August, 1894, in which the ohm is said to have the value 10^9 in terms of the centimetre and the second of time, and to be represented by the resistance of a certain column of mercury 106·3 centimetres in length. The ampere has the value 10^{-1} in terms of the centimetre, gramme, and second, and is represented by a current depositing 0·001118 of a gramme of silver per second. The volt has the value 10^8 in terms of the centimetre, the gramme, and the second, and is represented by 0·6974 (1000/1434) of the electrical pressure between the poles of a Clark cell. The practical effect of this Order is the same as that of the resolutions of the Chicago Congress of the

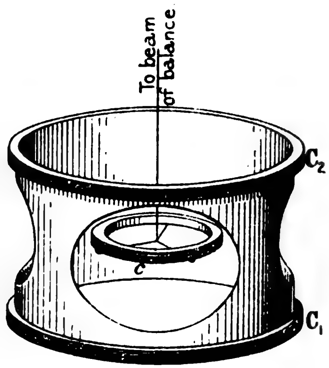


FIG. 5.—System of Lord Rayleigh's Current Balance.

preceding year, which introduced the international ohm, ampere, and volt, and defined them thus:—

The international ohm is said to be based upon the ohm equal to 10^9 C.G.S. units and to be represented by the resistance of a certain mercury column. The international ampere is 10^{-1} C.G.S. units and is represented sufficiently well for practical use by a current depositing 0·001118 gramme of silver per second; while the international volt is represented sufficiently well for practical purposes by 1000/1434 of the E.M.F. of a Clark cell at 15°C.

The difference in phraseology between the resolutions and the Order in Council should be noted. Moreover, according to the former, the international ohm is not 10^9 units, but is based upon this and is the resistance of 106·3 centimetres of mercury, while the international ampere and volt were defined as C.G.S. units and were to be represented by the current depositing a certain mass of silver and a certain fraction of the E.M.F. of a cell respectively.

So matters stood for some years. We pass now to more modern times. The National Physical Laboratory was opened at Teddington in 1902, and the resistance coils and other apparatus of the Committee came under the care of Mr. F. E. Smith. In 1897 Professor Viriamu Jones and Professor Ayrton described a new apparatus constructed for McGill University for determining the ohm by Lorenz's method, and in this apparatus the field coil consists of a single layer of wire wound in the helical groove on a marble cylinder. It followed from these

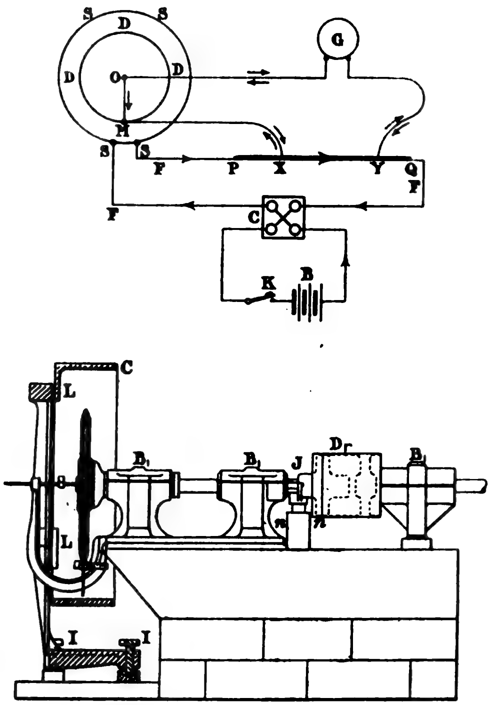


FIG. 6.—Jones's Lorenz Apparatus.

results that the length of the column of mercury having a resistance of 1 ohm is 106·28 cm. The apparatus is shown in Fig. 6.

Discrepancies still existing as to the value of Joule's equivalent when determined by mechanical and electrical means, and amounting to about 1 part in 400, had in 1897 led the Committee to express the view that a determination of the electrochemical equivalent of silver—on which depended the value to be used for the E.M.F. of the cells employed in the electrical method—should be made; and next year, 1898, at the Bristol meeting, Ayrton and Jones described their design for an ampere balance of high accuracy. Jones's illness and death

prevented the realization of this for some time. In 1903 it was arranged that a balance in accordance with their designs should be constructed at the Laboratory, and this was taken in hand. Meanwhile difficulties had been found with regard to the use of the Clark cell. An International Congress was held at St. Louis, in 1904, in connection with the Exhibition, at which attention was called to the fact that there were discrepancies between the laws relating to electrical units in the various countries represented, and urging the appointment of an International Commission to report on the matter. A conference was held at the Reichsanstalt in October, 1905, at which a resolution was passed in favour of holding an International Congress in London, and this took place in October, 1908. Among other things, the Berlin Conference recommended the adoption of a Weston standard cell in place of the Clark cell.

Meanwhile there appeared, in 1907, a series of papers giving the results of work by Ayrton, Mather, Smith, and Lowry, on the current balance, the silver voltameter, and the Weston cell; the results of these were of great value to the Congress, and an account of them follows later.

The Congress for the first time made a clear distinction between two sets of units—the ohm, the ampere, and the volt, and the international ohm, international ampere, and international volt. Let me spend a few moments in explaining the reason for this. The difference had been definitely recognized at Berlin and, to some extent, at Chicago, but the definitions there adopted were ambiguous. Thus the ohm is 10^9 C.G.S. units of resistance, and the international ohm is the resistance at 0° C. of a column of mercury of constant cross-section and having a mass of 14·4521 grammes and a length of 106·300 centimetres. The ampere is 10^{-1} C.G.S. units of current, while the international ampere is the current which when passed through a solution of nitrate of silver in water under certain specified conditions deposits silver at the rate of 0·00111800 of a gramme per second. The volt is 10^8 C.G.S. ~~units of resistance~~, and the international volt is the electrical pressure which when steadily applied to a conductor having a resistance of 1 international ohm produces a current of 1 international ampere. The Congress also decided that the Weston normal cell might be conveniently employed as a standard of electrical pressure, having provisionally, at 20° C., an E.M.F. of 1·0184 international volt.

The reasons for the distinction may be briefly put. It had not been found possible to measure electrical quantities absolutely to the same accuracy as was obtained when comparing two resistances and two currents. Methods of comparing two resistances to an accuracy of some few parts in 100,000, or even of 1,000,000, were in use, and confusion was caused because in attempting to make a concrete standard to represent an ohm uncertainties of 1 or 2 in 10,000 were possible. It was necessary to have a standard more definite than any existing concrete representation of 10^9 C.G.S. units, and all that could

be done was to construct some material standard of resistance having the necessary permanence and capable of being reproduced with the desired exactness. For the sake of securing the tremendous advantages of an absolute system, it was necessary that these concrete standards should approach as nearly as might be to the absolute units. The disadvantage of the continual change in standard required as each more accurate investigation approaches more nearly to the absolute value 10^9 C.G.S. units far outweighs the gain. We know that the ohm and the international ohm differ by a small quantity negligible for nearly every purpose: what that difference is, experiment will determine with ever-increasing accuracy; but the practical standard—the international ohm—remains fixed. The two zeros at the end of the figure 106·300 were added to make this clear. We often need to measure a resistance in terms of a standard to more than four figures; if the standard itself is only defined to four figures, such a comparison is unmeaning.

The change is of the same nature as that which has taken place in the centimetre and gramme. Originally the metre was one ten-millionth of the distance along a meridian arc between the pole and the equator; it is now the distance between two marks on a platinum-iridium bar at Paris. The kilogram was the mass of 1,000 cubic centimetres of water; it is now the mass of a certain lump of platinum.

Lengths could be compared with an accuracy far beyond that possible in measuring a meridional arc, and masses weighed against a standard mass with much higher exactitude than is reached in finding the mass of 1,000 cubic centimetres of water. So, too, with electrical quantities. Standards can now be set up in terms of the international units with an accuracy much in excess of that with which the ohm and ampere were known at the time of the London Congress, thanks in great measure to the co-operation between the National Standardizing Laboratories, secured by a Permanent Committee set up by the Congress. So far as England is concerned, the work of the Conference was confirmed by an Order in Council, dated January, 1910, and recognizing the three international units.

Lord Kelvin died a year before the London Conference was held and did not see the results of his long labours for the absolute system crowned by its international adoption. Its work was the natural outcome of deliberations in which for some forty years he took the leading part, and its success is a lasting memorial to the principles he laid down in the first Report of the Committee.

The Congress settled the definitions. There was more to be done, however, before it could be said that the standards which embodied those definitions in concrete form were uniform throughout the world, and this was left to Lord Rayleigh's Committee.

Meanwhile it remained also to determine as accurately as possible the differences between the ampere and the international ampere, and between the ohm and international ohm. Let us consider these briefly in order.

At the time of the Congress the researches with the Ayrton-Jones balance by Ayrton, Mather, Smith, and Lowry, had just been published. These were followed immediately by work in France by Janet and his colleagues, by Guillet and by Pellat. Professor Haga's work was published in 1910. A very elaborate series of experiments has just been published by Rosa, Dorsey, and Miller, of the Bureau of Standards, who write when describing the work at the National Physical Laboratory: "This work . . . marks the beginning of a new epoch in the history of the absolute measurement of electrical quantities."

Figs. 7 and 8 show the Ayrton-Jones current balance; Fig. 9 the balance of the Laboratoire Central d'Électricité; Fig. 10, Pellat's balance, and Fig. 11, the Bureau of Standards balance. The results with the Ayrton-Jones balance are given in Table XI.

TABLE XI.

Ayrton-Jones Current Balance. Value for E.M.F. of Weston normal cell, 1·01830 at 17° C.

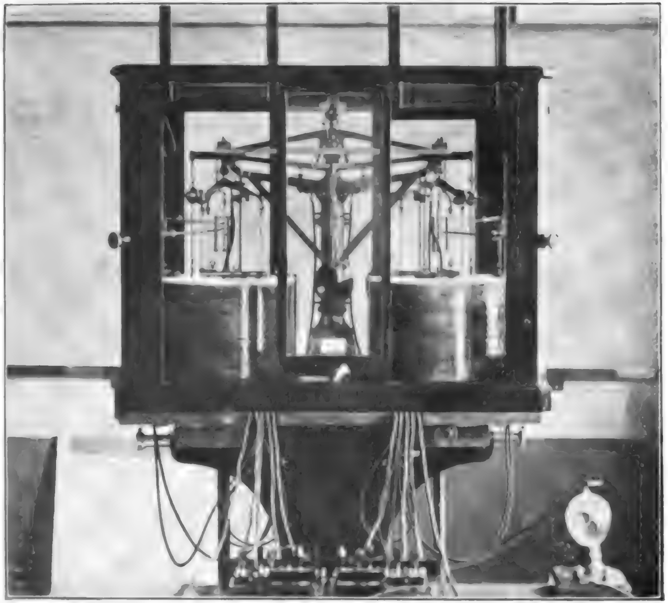
Of the 71 determinations made—

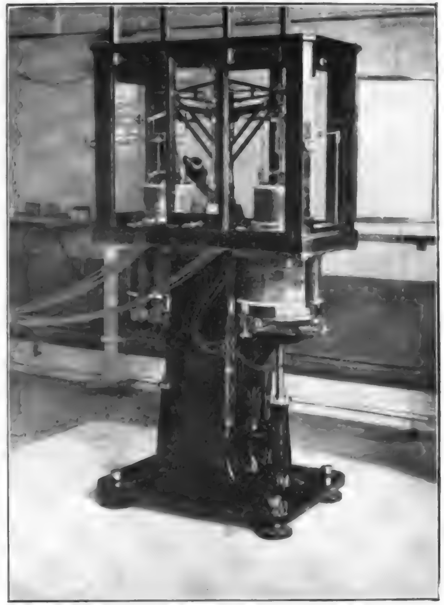
7 are	within	1 part	in 1,000,000	of the mean.
14	"	2 parts	"	"
28	"	5 "	"	"
53	"	10 "	"	"
66	"	15 "	"	"
70	"	20 "	"	"

The difference between the Ayrton-Jones form of balance at the National Physical Laboratory and the Rayleigh form, or, perhaps, rather the Joule form adopted elsewhere, should be noted. In the National Physical Laboratory balance the actual dimensions of the coils can be measured with high accuracy. In the Rayleigh form, as Lord Rayleigh showed, the principal constant depends on the ratio of the mean diameters of the fixed and suspended coils, and can be determined by electrical means. Experiments are now in progress at the Reichsanstalt.

Summing up our knowledge at present, we may claim that the ampere deposits from a solution of nitrate of silver under definite conditions $0{\cdot}0011181_4$ gramme per second, and that, therefore, the international ampere is less than 10^{-1} units by $0{\cdot}01_3$ per cent.

As to the ohm, less has been done. Viriamu Jones at the time of his death was designing new apparatus for its determination by Lorenz's method, and the Drapers' Company had promised a generous grant towards its construction. In 1902 the Company intimated to the Committee of the National Physical Laboratory their wish to place £700 at the disposal of the Laboratory for the construction of the apparatus as a memorial to him. This was gratefully accepted, and the apparatus was designed by Mr. F. E. Smith; and with Sir Andrew Noble's generous help the machine was constructed at Elswick.





FIGS. 7 and 8.—Ayrton-Jones Current Balance at the National Physical Laboratory.

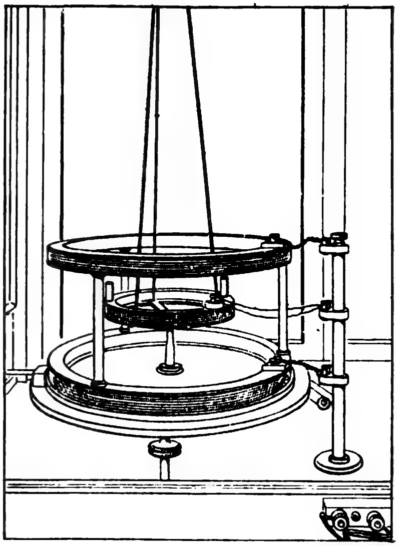
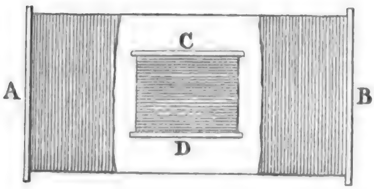


FIG. 9.—Current Balance (Suspended and Fixed Coils) of the Laboratoire Central d'Électricité.



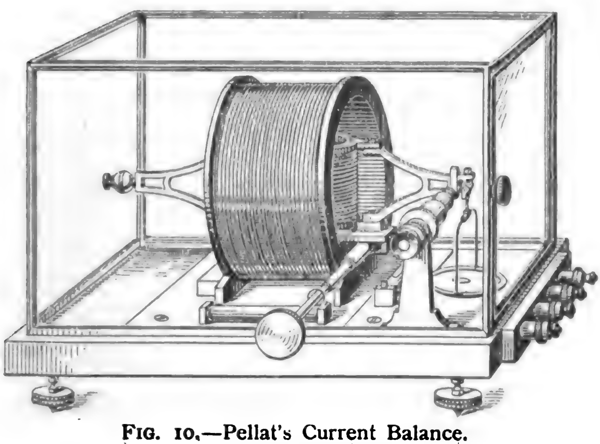


FIG. 10.—Pellat's Current Balance.

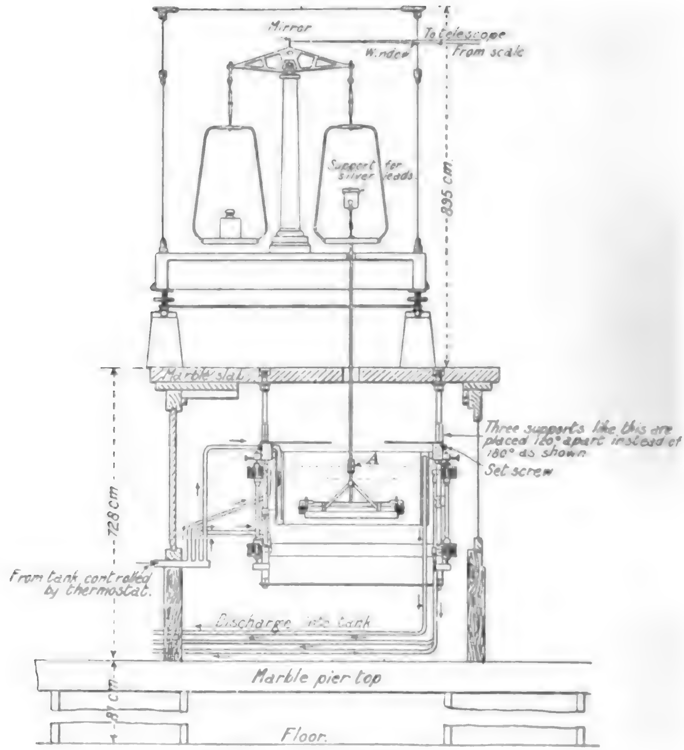


FIG. 11.—Bureau of Standards Current Balance.

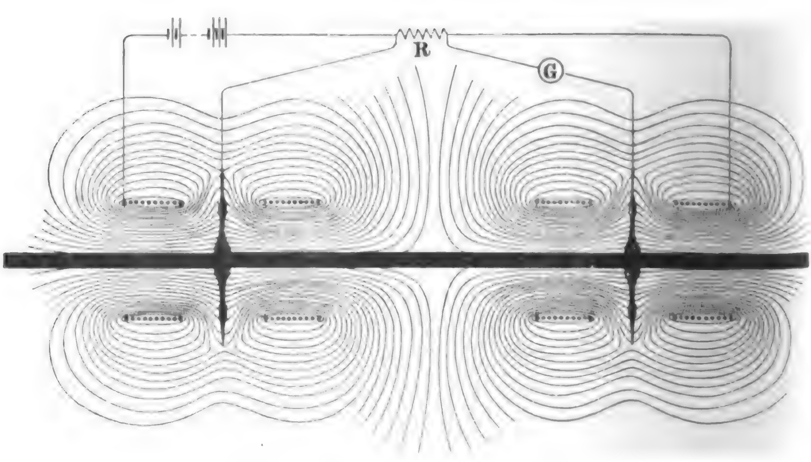
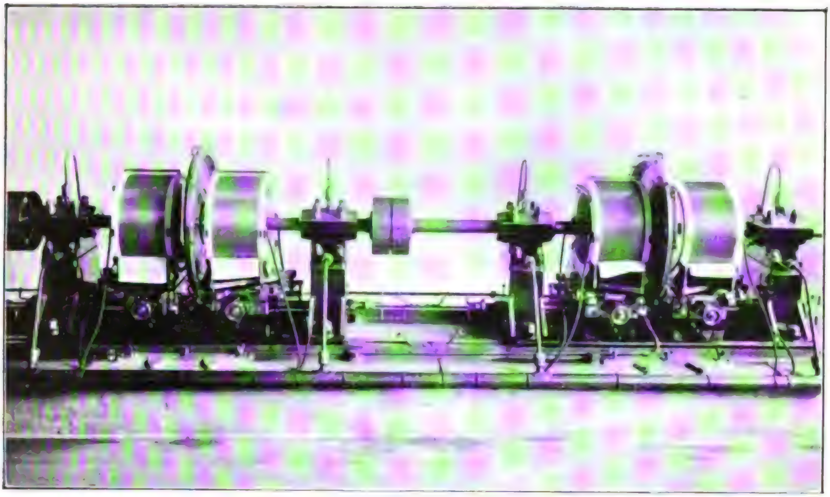
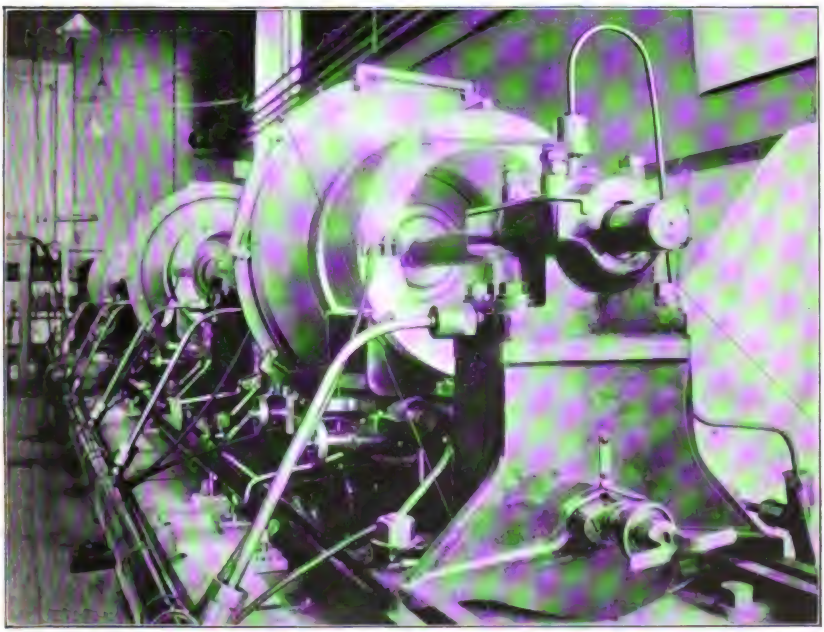


FIG. 12.—Diagram showing Magnetic Field of the Lorenz Apparatus at the National Physical Laboratory.





FIGS. 13 and 14.—Lorenz Apparatus at the National Physical Laboratory.

This machine differs from other apparatus of the kind in having two pairs of fixed coils and two rotating discs. This is shown diagrammatically in Fig. 12. The rubbing contacts, then, by which the E.M.F. generated in the rotating disc is picked off, are thus exactly alike, and the thermal E.M.F.'s generated at the contacts balance very approximately. With brushes rubbing on the disc and on the shaft respectively this is not the case. Again, the dimensions of the disc and coils are so chosen that in the position occupied by the edge of the disc the magnetizing field due to the coils is zero. Thus a slight error in the size of the disc produces a very small change in the number of lines of force which traverse it, and, hence, a very small error in the result. Again, the edge of each disc has been divided by Mr. Smith into ten insulated segments. The shaft is hollowed, and the corresponding segments on the two discs are connected by insulated wires passing through the hollow shaft. A certain E.M.F. (E, say) is produced when the discs rotate between each corresponding pair of segments. The brushes outside are so connected as to put these pressures in series. Thus the total E.M.F. is 10 E, and a resistance can be measured ten times as great as would be possible with the usual arrangement.

Figs. 13 and 14 show the machine.

Mr. Smith has just completed his first series of experiments, with the result that he finds that the length of the mercury column representing the ohm is less than 106·27 cm.

In 1912 Mr. Campbell made a determination by two interesting methods at the National Physical Laboratory. In one of these the coefficient of mutual induction of his standard inductance was compared by a modification of Carey Foster's method with the capacity of a standard condenser; the capacity of his condenser was then found by Maxwell's method in terms of a resistance, and thus the resistance was measured in terms of the mutual inductance and a frequency.

In the other method (Fig. 16) two nearly equal simple harmonic currents in quadrature are employed. One of these, $A \cos \omega t$, induces an electromotive force, $A \omega M \sin \omega t$, in the secondary of a mutual inductance. The other, $B \sin \omega t$, traverses a resistance, R, producing an E.M.F., $R B \sin \omega t$, between its terminals. The resistance R and the secondary of the inductance are in series with a vibration galvanometer tuned to the frequency of the current.

If the galvanometer is not deflected—

$$A \omega M = B . R, \text{ and thus } R = \frac{A}{B} \omega . M.$$

These observations of Mr. Campbell lead to the value 106·27 for the length of the mercury column representing 1 ohm, 10^9 C.G.S. units.

Since the Congress the Standardizing Laboratories of France, Germany, Great Britain, and the United States have co-operated as far as possible in securing identity of standards. This was greatly helped by a visit of representatives of the European Laboratories to Washington

in 1910, rendered possible by the kindness of Dr. Stratton and the munificence of various electrical interests in the States. Tables XII–XVI show how fully success has attended these efforts. A number of mercury tubes have been constructed at the National Physical Labora-

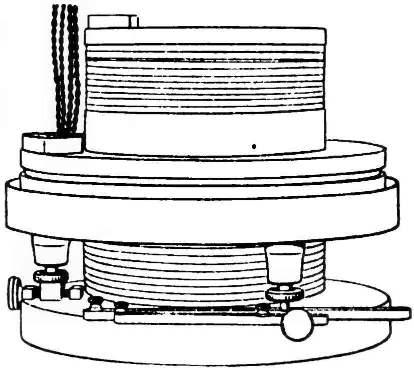


FIG. 15.—Campbell's Mutual Induction Standard.

tory lately, and Table XII gives their values in terms of the means of the whole series. The average difference is about 1 part in 100,000.

In Table XIII we have the values of a series of coils compared with the standards in Washington, Paris, Berlin, and London. Tables XIV

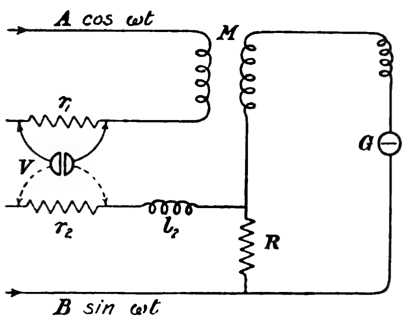


FIG. 16.

and XV give corresponding values for a number of Weston cells, while Table XVI shows the results of the series of combined experiments at Washington on the silver voltameter.

We may claim, I think, that, in spite of the forebodings of the technical men of 1862, the efforts of Kelvin and his colleagues have been successful; and, thanks to him, electricians possess a simple, accurate system of standards unequalled in any other science.

Table XII.

New Mercury Standards of Resistance.

No. of Tube.	Glass.	Length at 0° C. Mm.	Calibre Factor.	Mass of Mercury at 0° C.	Calculated Resistance, int. ohms.	Observed Resistance in Terms of Mean of all.	Diff. obs.—calc. 1 × 10—5.
2	Jena 16'''	1028·879	$1{\cdot}00010_1$	$13{\cdot}5540_1$	$0{\cdot}99986_6$	$0{\cdot}99986_0$	−0·6
6	" "	1058·125	$1{\cdot}00007_7$	$14{\cdot}3339_3$	$0{\cdot}99994_5$	$0{\cdot}99993_7$	−0·8
9	" 59'''	1000·756	$1{\cdot}00011_6$	$12{\cdot}8274_9$	$0{\cdot}99956_7$	$0{\cdot}99957_9$	+1·2
11	" 16'''	929·586	$1{\cdot}00018_1$	$11{\cdot}0679_7$	$0{\cdot}99965_6$	$0{\cdot}99967_0$	+1·4
27	" 59'''	812·187	$1{\cdot}00007_3$	$8{\cdot}4475_0$	$0{\cdot}99977_1$	$0{\cdot}99976_1$	−1·0
S	Verre dur	1188·249	$1{\cdot}00002_6$	$18{\cdot}0776_2$	$0{\cdot}99976_3$	$0{\cdot}99976_9$	+0·3
G	" "	1160·500	$1{\cdot}00012_8$	$17{\cdot}2438_9$	$0{\cdot}99983_8$	$0{\cdot}99985_5$	+1·7
1	Jena 16'''	1138·576	$1{\cdot}00002_9$	$16{\cdot}5965_0$	$0{\cdot}99986_1$	$0{\cdot}99985_8$	−0·3
130	" "	800·722	$1{\cdot}00007_3$	$8{\cdot}2197_8$	$0{\cdot}99967_3$	$0{\cdot}99968_0$	+0·7
137	" "	712·299	$1{\cdot}00000_8$	$6{\cdot}4993_4$	$0{\cdot}99948_0$	$0{\cdot}99946_0$	−2·0
5	Verre dur	1098·653	$1{\cdot}00007_1$	$15{\cdot}4645_7$	$0{\cdot}99916_5$	$0{\cdot}99917_1$	+0·6
11	" "	1024·146	$1{\cdot}00007_0$	$13{\cdot}4291_3$	$0{\cdot}99987_9$	$0{\cdot}99986_5$	−1·4
12	" "	1049·821	$1{\cdot}00010_0$	$14{\cdot}1116_4$	$0{\cdot}99984_5$	$0{\cdot}99985_6$	+1·1
13	" "	1061·576	$1{\cdot}00013_3$	$14{\cdot}4321_4$	$0{\cdot}99967_6$	$0{\cdot}99967_3$	−0·3

Table XIII.

International Comparisons of Resistances, 1912.

Coil No.	Bureau of Standards, June, 1912.	Laboratoire Central, Sept., 1912.	Physikalisch-Technische Reichsanstalt, Oct., 1912.	National Physical Laboratory, Oct., 1912.	Laboratoire Central, Nov., 1912.	Max. Difference.
11	$1{\cdot}00004_9$	$1{\cdot}00004_3$	$1{\cdot}00003_7$	$1{\cdot}00003_7$	$1{\cdot}00004_3$	$0{\cdot}00001_2$
12	5_1	4_5	3_7	3_9	4_2	1_4
3939	10_0	9_4	9_1	9_3	9_4	0_9
3940	10_1	9_4	9_3	9_5	9_7	0_8
Means	$1{\cdot}00007_5$	$1{\cdot}00006_9$	$1{\cdot}00006_5$	$1{\cdot}00006_6$	$1{\cdot}00006_9$	$0{\cdot}00001_1$

TABLE XIV.

E.M.F. of the Weston Normal Cell at 20° C. in terms of the Ampere (10^{-1} C.G.S.), and the International Ohm.

1905	Guthe	1·0185 volts.
1906	Ayrton, Mather, and Smith	82 „
1908	Janet, Laporte, and Jouast	86 „
1908	Lippman and Guillet	82 „
1908	Pellat	84 „
1910	Haga and Boerema	82 „
1912	Rosa and Dorsey	82 „

E.M.F. at 20° C. in terms of the International Ampere and International Ohm.

1906	Ayrton, Mather, and Smith	1·0184 int. volts.
1908	Laporte and de la Gorce...	84 „
1909	Jaeger and v. Steinwehr	84 „
1910	International Committee at Washington	83 „

TABLE XV.

International Comparisons of Weston Cells which travelled between the Various Laboratories. Differences in Microvolts.

Stand. Cell No.	B.S. June and July, 1911.	N.P.L. Aug., 1911.	P.T.R. Sept. and Oct., 1911.	N.P.L. Oct., 1911.	L.C.E. Oct., 1911.	N.P.L. Nov. and Dec., 1911.	B.S. Jan., 1912.
262	−6	—	−70	—	−80	−60	—
267	41	—	0	—	—	—	—
268	37	—	−15	—	—	—	—
51	−58	—	−70	—	−30	—	—
32	−69	—	−115	—	−130	—	—
301	−24	−5	−30	—	−15	—	−40
304	19	23	0	—	0	—	7
309	−36	−27	−45	—	−20	—	−56
310	0	−4	−25	—	−10	—	−44
A1	−13	−12	−15	—	−10	—	−22
43	2	3	−30	—	5	—	0
44	0	—	−15	−7	—	—	−1
19	−27	—	−45	−30	—	—	−28
22	−31	—	−40	−29	—	—	−30
238	−2	—	20	52	—	—	−10
350	−24	—	−20	1	—	—	−24
352	−31	—	−45	−30	—	—	−30
133	—	—	—	—	30	—	34
142	—	—	—	—	30	—	33
1·3	—	—	—	—	—	−6	−5
1·33	—	—	—	—	—	−16	−16
17	—	—	—	—	—	−5	−8

TABLE XVI.

Comparison of Voltameters.

(Four forms of Voltameters and four experimenters—American, French, German, and British.)

Date, 1910	Number of Voltameters in Circuit.	Calculated E.M.F. of Weston Normal Cell at 20° C.	Difference from Mean. 1×10^{-5}.
April 14	4	1·01825	−6
" 15	8	33	−2
" 18	4	27	−4
" 20	8	31	0
" 22	4	29	−2
" 26	8	37	+6
" 28	4	32	+1
" 30	5	34	+3
May 2	7	37	+6
" 3	5	36	+5
" 5	8	35	+4
" 7	8	28	−3
" 12	6	30	−1
" 19	4	26	−5

Mean = 1·01831.

Mr. ALEXANDER SIEMENS: I am delighted to see such a large audience of young men here to-night to hear the history of those electrical units which they are using every day, and which have proved of the utmost value in the practical applications of electricity. Those units have enabled us to compare without difficulty results obtained in different countries, and they have ensured that everybody who is engaged in research measures accurately; and, of course, science is measurement. No doubt Lord Kelvin did more than anybody else to develop the ideas which Professor Weber first set forth when he described his absolute system of units; but I should also like to draw attention to the fact that it was Dr. Werner Siemens who first suggested that mercury was the best substance of which to make the units of resistance. In trying to measure the resistance of cables, he found that a unit was absolutely necessary, and so he introduced a mercury unit of one metre length and one square mm. in section.* Without any doubt the scientific determination of the units and their relation to each other has helped us very much indeed. The practical units of which Dr. Glazebrook has spoken were introduced purely for commercial purposes, because it is impossible repeatedly to alter such units with the progress of exactitude in measurements. Dr. Glazebrook Mr. Siemens.

* *Poggendorf's Annalen der Physik und Chemie*, vol. 110, p. 1, 1860; and *Report of the Thirtieth Meeting of the British Association for the Advancement of Science*, p. 32, 1860.

Mr. Siemens. has given us such a clear statement of the development of these units that the Institution ought to be exceedingly grateful to him; I have therefore much pleasure in moving that a hearty vote of thanks be accorded to Dr. Glazebrook for the Kelvin Lecture that he has just delivered.

Dr. Silvanus Thompson. Dr. SILVANUS P. THOMPSON: I have much pleasure in seconding the vote of thanks which has been proposed by Mr. Siemens. I have no doubt that the president has called upon me to do so because I had the honour of giving the first Kelvin Lecture. While appreciating the privilege of seconding this resolution, I wish, however, that he had called upon another person who has honoured us with his presence to-night—Lord Rayleigh. Lord Rayleigh has played an important part in the making of history in these units from about 1878, and we cannot be oblivious of the fact that we owe a very great debt to him for the vast amount of classical work, substantial, accurate, scientific work, with which his name will ever be associated in this matter of units. I was particularly glad to see Lord Rayleigh's coil spun to illustrate the determination of the ohm. Very few of us, I imagine, had any kind of grasp of the prodigious amount of thought, of patience, time, money, and effort that have been spent since the years 1859 and 1860, when this subject of units became urgent, down to the present time, in the determination and the redetermination of the values of these units and the standards which represent them. To have gained such an excellent view of these pieces of intimate scientific history, and at the hands of one who played no unimportant part in the work—of which he has said all too little to-night, in acting for many years as the Secretary of that British Association Committee—is, I feel, a very great privilege indeed. But the matter does not stop there. Important as all this work has been, important as Dr. Glazebrook's work has been in connection with the units and their history, there is something else, I think, that we must not forget. It is five years and two months since Lord Kelvin died. He was, as we have heard, intimately connected from the very beginning with this work, and he established at Glasgow the first laboratory where accurate electrical measurement was possible in this country; and every university and every college since have deemed it right to do something at any rate toward teaching accurate electrical measurement. But there was an object to be fulfilled beyond that which could be fulfilled either in Lord Kelvin's laboratory at Glasgow or in the Cavendish laboratory at Cambridge, where Maxwell, and after him Lord Rayleigh, and with him Dr. Glazebrook, and down to the present time Sir Joseph Thomson, have worked. There was something still to be desired, and that was the making of a national laboratory where this kind of work could proceed on a larger scale and with a greater certainty of continuity. As we all know, for the last eleven years we have had this National Physical Laboratory under the charge of Dr. Glazebrook. Other nations have their Laboratories. We owe it to the fact that there are these great National Laboratories working in accord at the verification of results that no longer have we any burning question about units, and that the

value of the units has become a thing judged. It is no use to raise fresh issues, but one may mention in passing that the National Physical Laboratory has surpassed itself; for after having persuaded the Conference that met in London in 1908 to add two zeros on to the 106·3 and make it 106·300, they have, through the work of Mr. F. E. Smith, shown how incorrect that is, and that it is 106·27, or something of the sort. They are actually undoing the details which they themselves put in, and are showing that there is something better to be attained. Have we realized—I do not think any of us have—the immense amount of work, energy, time, and patience which is now being spent, under the auspices of the Director, Dr. Glazebrook, and the President of the Committee, Lord Rayleigh, in our National Physical Laboratory? Is it realized that we are spending £32,000 a year, for that is the total of the budget of the National Physical Laboratory? Even so that sum is not enough for the work that is going on there. The progress of science will demand either from the Government or from other sources a large increase in the budget of the National Physical Laboratory. We must be prepared to support the efforts that are being made to extend and amplify the work of the National Physical Laboratory. None of us would undervalue its work; and I am quite sure that, after hearing the Kelvin Lecture from Dr. Glazebrook to-night, none of us will be disposed to say anything else but that Dr. Glazebrook is pre-eminently the right man in the right place for carrying forward the work of the units and standards and all the varied research now in progress in the National Physical Laboratory. I have much pleasure in seconding the motion. **Dr. Silvanus Thompson.**

Lord Rayleigh: Being called upon by the President, I feel I must certainly add my voice to the vote of thanks which has been moved and seconded to Dr. Glazebrook for his lecture. It is some thirty years ago, as will be seen by the tables, since I was actively engaged in determining these units, and at that time the discrepancies amounted to about 2 per cent, I think, in the absolute determinations of the ohm. I believe we got at that time to within about 1 part in 1,000. Mr. Smith would now despise such a result as that, and nothing less than 1 part in 10,000, if not even a higher figure, would satisfy him. I think that the different construction of the coils now used is largely responsible for the very high degree of accuracy now attained. Professor Jones was, I believe, the first to use a single layer of wire, and this not only allows the cylinders to be measured more accurately in the first instance, but their condition can be verified more easily at any subsequent time. There is just one more point I should like to mention. In practical work the Clark cell has now been replaced by the Weston cell, but I think it is rather hard on the memory of Clark that his name should disappear, because after all the Weston cell is a very small modification of the Clark cell—merely the substitution of cadmium for zinc, two closely allied metals—and the points of novelty which Clark introduced are common to both. I believe, also, as a matter of fact the original Weston cell was defined in a form which would not lend itself to the **Lord Rayleigh.**

Lord Rayleigh.

present use, as set up with a non-saturated solution of sulphate of cadmium. I join in the vote of thanks to Dr. Glazebrook which has been moved and seconded.

The resolution of thanks was then carried by acclamation.

Dr. Glazebrook.

Dr. R. T. GLAZEBROOK: I wish to thank the members and guests for the way in which they have received my lecture. I ought to have mentioned that some of the original Clark cells used by Lord Rayleigh are on the table; they will doubtless be of interest to members. I should also like to take this opportunity of thanking various friends who have placed apparatus at my disposal, and in particular my colleague, Mr. F. E. Smith, for the care he has taken in setting up the apparatus and carrying out the experiments.

Proceedings of the Five Hundred and Fifty-first Ordinary Meeting of the Institution of Electrical Engineers, held on Thursday, 6th March, 1913—Mr. William Duddell, F.R.S., President, in the chair.

The minutes of the Ordinary Meeting held on 27th February, 1913, were taken as read, and confirmed.

Donations to the *Building Fund* were announced as having been received from R. H. Burnham, R. A. Dawbarn, J. W. Ewart, F. Gill, A. Hay, J. S. Highfield, H. Hirst, Sir H. B. Jackson, G. C. Lloyd, W. M. Mordey, Professor J. T. Morris, A. Stroh, Sir Joseph Swan, A. Wright, H. W. Young, The Associated Municipal Electrical Engineers of Greater London, and The Incorporated Municipal Electrical Association; and to the *Benevolent Fund* from G. F. Allom, A. B. Anderson, Ivon Braby, M. S. Chambers, R. A. Chattock, W. C. Clinton, V. K. Cornish, B. Davies, A. Denny, J. Devonshire, H. C. Donovan, B. M. Drake, Dr. C. V. Drysdale, W. Duddell, F.R.S., K. Edgcumbe, S. Evershed, F. Gill, Dr. R. T. Glazebrook, C.B., B. B. Granger, F. E. Gripper, C. C. Hawkins, K. Hedges, D. Henriques, J. S. Highfield, H. Hirst, H. W. Kolle, A. E. Levin, C. H. Merz, L. B. Miller, W. M. Mordey, E. Parry, The Hon. Sir C. A. Parsons, K.C.B., W. H. Patchell, A. H. Preece, L. Preece, Sir W. H. Preece, W. R. Rawlings, T. Rich, R. Robertson, S. R. Roget, A. Siemens, The Stearn Electric Lamp Company, Ltd., A. Stroh, A. J. Stubbs, A. P. Trotter, T. C. T. Walrond, H. W. L. Ward, J. G. Wilson-Dickson, and C. H. Wordingham, to whom the thanks of the meeting were duly accorded.

Messrs. G. W. Richardson and E. W. Moss were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

Lionel Edward Caine.
Farley G. Clark.
Harold Ainslie Cox.
William Cross.
Edmund Philip Grove.
Professor Dugald C. Jackson.
Carl Gustaf Stjernberg.

ELECTIONS—*continued.*

As Associate Members.

Alexander MacKay Anderson.
Arthur Lionel Ballard.
Leslie Walter Ballard.
Frank Owen Barralet.
Jesse Haigh Baxter.
Thomas Croasdale Booth.
Frederick Herbert Brandreth.
William Ernest Brandreth.
William James Bransom.
Robert Allan Brown.
Robert Henry Bryans.
Alexander Burnet.
John Haydon Cardew.
Herbert Vooght Cornish.
John Robert M. Elliott.
William Parker Elliott.
Heber Davie Evans.
Wallace Bruce Good.
Thomas Ian Murray Gordon.
Joseph Dean Hathaway.
Ernest Hedley.
Herbert Henry Holmes.
Lennard Arthur Hoyle.
William Cradock Knight.
Robert Watson Law.
William A. MacKenzie.
John Young MacKersie.
George Albert Madden.
David Martin.
Philip Michael Martin.
John Roderic Matthews.
Robert Cecil Milliken.
Arthur Nichols Moor.
Granville Murray.
George Nicolson.
Alfred Regnauld.
John Laurence Smith.
Henry Arthur John Stanton.
Frank Vincent Stephen.
William Tancred.
John Whitfield Turner.

As Associates.

Robert Livingstone McCulloch.
John Fenton Newall.

As Graduates.

Charles Wm. Braithwaite.
Edgar Cecil Channell.
Edward Percy Collett.
Robert Stanley Davis.
Frederick Shah Dinenage.
Thomas Drummond.
Thomas Wilfrid Elsdon.
John Whitford Leach.
Thomas Herbert McNaught.
Oliver Melville.
Raymond James Mitchell.
Thomas Frederick Stent.

As Students.

Edward Castendieck Albrecht.
Sydney Baker.
Benjamin Croft Bayley.
Claude Robert Bicknell.
Noel Barnden Bunt.
Reginald Lancelot Castle.
Arthur Stephenson Carr.
John Colin Clarke.
Luis Felipe Clement.
William Thomas Critcher.
William Gordon Cross.
Thomaz Custance C. de Moura.
Antonio Victor dos Santos.
Herbert Gregson.
John O. Griffiths.
Brenchley Ernest G. Mittell.
Basil Sheldon Orme.
Norman Pemberton.
John Harrison Salisbury.
Albert Schamasch.
Maxwell Smith.
George Owen Tipping.

TRANSFERS.

From the class of Associate Members to that of Members :—

Edward Calvert.
Archibald Sidney Campbell.
Claud Carew-Gibson.
John Hayton Carrick.
William Leonard Carter.
Alfred George Cooper.
Haldane Gwilt Cotsworth.
James Herbert Edwards.
Harry Stephenson Ellis.
Thomas Frank Courtier Forster.
John Gray, B.Sc.
Frederick Simmons Grogan.
Eustace Edward Gunter.
Rudolph John Kaula.
Walter Frederick Long.
John Henderson Mackail.
Robert Stafford McLeod.
Robert Burgess Mitchell.
Frank Westlake Parkinson.
Eustace Ridley.
Frank Henry Whysall.
Charles Barnard Wigg.
John Harry Wild.
John Harcourt Williams.

From the class of Associates to that of Members :—

Albert Thomas Bartlett, B.A.
Ernest Arthur Reynolds.

From the class of Associates to that of Associate Members :—

Percival Charles Austwick.
Alfred Brown Blakey.
Herbert Edward Britton.
James Cecil J. Johnston.
William Mead.
Thomas Pearson.
Herbert William Stovold.

From the class of Students to that of Associate Members :—

Edward Victor Buchanan.
Theodore C. Christianson.
Hugh Clark, B.Sc.
Edwin Arthur Corbin.
James Gaunt Craven.
Frank Murphy.
Richard Jones O'Brien-Owen.
Alan Campbell Towers.
Ernest Ansley Watson.

From the class of Students to that of Graduates :—

George John Baldock.
John Wilson Beck.
Harry Butler.
Alfred George Cross.
Jinsun Kitsen Hwoo.
Edward Arthur Richards.
Alfred Smellie, Junr.

A paper by Mr. S. L. Pearce and Mr. H. A. Ratcliff, Members, entitled "Recent Developments in the Street Lighting of Manchester" (see page 596), was read and discussed, and the meeting adjourned at 10 p.m.

RECENT DEVELOPMENTS IN THE STREET LIGHTING OF MANCHESTER.

By S. L. PEARCE and H. A. RATCLIFF, Members.

(*Paper first received 16th January, and in final form 3rd February,* 1913; *read before* THE INSTITUTION *6th March, before the* MANCHESTER LOCAL SECTION *25th February, before the* BIRMINGHAM LOCAL SECTION *12th March, and before the* SCOTTISH LOCAL SECTION *18th March,* 1913.)

SYNOPSIS.

1. Introductory remarks defining the nature and scope of the paper.
2. Reference to the early history of street lighting by electricity in Manchester.
3. The street lighting authority for the city.
4. The competition between the rival illuminants.
5. Description of the Princess-street high-pressure gas installation.
6. Description of the Portland-street arc lamp installation.
7. Criticism of the first results obtained in Portland-street.
8. The subsequent alterations that were made to the lamps.
9. Experts called in to report on the two rival systems.
10. Full particulars of the photometric work undertaken.
11. Probable limits of accuracy.
12. General comparisons between Portland-street and Princess-street. Advantages and disadvantages of each system.
13. Confirmation of the Portland-street results as furnished by the reports of the experts.
14. Relative costs of the two systems.
15. The lighting in Mosley-street, St. Peter's-square, Albert-square, and Piccadilly.
16. Notes on the physiological problems involved, with special reference to the flicker photometer and the question of colour differences.
17. Various types of globes, diffusers, and reflectors, with their advantages and disadvantages.
18. Conclusions to be drawn from the tests.
19. Appendices.

INTRODUCTION.

This paper is principally a record of the work undertaken, the results obtained, and the numerous tests and experiments upon which the satisfactory completion of the work depended.

Much of the detailed information relates to the lighting of Portland-street, Manchester, by means of flame arc lamps; and as the success or

otherwise of the scheme adopted can only be judged by comparisons, data relating to earlier systems of arc lighting and contemporary systems of high-pressure gas lighting have also been included. Such data mainly consist of statements of relative costs, and curves and diagrams showing graphically the results of comparative photometric tests.

Early History of Street Lighting in Manchester by Electricity.

The first installation of arc lighting in Manchester was carried out in the early part of 1897. A number of 500-watt "open-type" arc lamps of the short-hour double-carbon type, suspended about 22 ft. above the ground, were erected in Albert-square, on Piccadilly-esplanade, and in St. Ann's-square.

On the 27th October, 1897, the City Council approved a Report of the Electricity Committee recommending that all tramway routes be lighted by arc lamps; and in the following year application was made to the Local Government Board, and sanction subsequently received, for the borrowing of £75,000 for street-lighting purposes. It is outside the scope of this paper to go into the reasons why the work has not been carried out, notwithstanding a substantial expenditure on underground mains for the purpose. The fact may be recorded, however, and is of interest in view of the very meagre amount of public lighting by electricity in this city, that at one period it was the distinct desire of the Corporation that the main thoroughfares should be so illuminated.

In 1904 the original "open-type" arcs were superseded by 600-watt and 900-watt lamps of the single-enclosure type, suspended some 18 ft. 6 in. from the ground, and burning 100 hours without re-trimming.

At or about that time a limited number of intensified gas lamps had been placed on the streets of Manchester, and a report was presented by one of the authors giving some comparative results of the two systems of illumination. The report was subsequently reprinted in the *Electrician* of 4th November, 1904, and led to a good deal of discussion in the gas and electrical journals of that date.

For the purpose of exact comparison, the test results obtained in 1904 have been converted to the corresponding values for illumination on a horizontal plane, 3 ft. 3 in. above the ground. Notwithstanding the steepness of the illumination curves, the 1904 lamps were in every way superior to the then rival system of intensified gas lighting. The comparative results obtained showed that the cost of a minimum horizontal illumination of 0·25 foot-candle per lamp per hour was 0·57d. for the 600-watt arcs, and 1·21d. for the intensified gas. The running cost per 1,000 c.p.-hours was similarly 0·75d. for the arcs, and 1·08d. for the intensified gas. These figures are mentioned here as being of interest in comparison with the results obtained at a more recent date.

In 1905 the Urban District Council of Gorton (not being then incorporated with the city) entered into a contract with the Electricity Department of the Manchester Corporation for the lighting of Hyde-

road, Wellington-road, and Cross-street, and this was carried out with the same type of single-enclosure lamp as used in the city, but of only 450-watt capacity.

Although sharing in the public lighting to an almost negligible extent, the Electricity Department was desirous of keeping up to date with their street illumination. The year 1906 marked the beginning of the supersession of the single-enclosure type of lamp by the magazine flame arcs, a commencement being made on the Piccadilly-esplanade. With the exception of the lamps in St. Anne's-square and Gorton, the change-over was completed by the end of 1911, and the result has been a very substantial improvement in the general illumination of those thoroughfares above referred to.

The Street-lighting Authority for the City.

Having regard, however, to the size of the city, it will not be disputed that the amount of street lighting, totalling only 114 lamps (inclusive of 42 lamps in the Gorton district), is ridiculously small. This state of affairs has been outside the control of the Electricity Department for the simple reason that up to a very recent date the Gas Committee of the Corporation has been the street-lighting authority for the city of Manchester.

On 2nd October, 1912, the City Council brought about a change by placing the control of the lighting of the streets under the authority of a Street Lighting Committee, consisting of five members of the Gas Committee, five members of the Electricity Committee, and five members appointed by the Nomination Committee, being members of neither Gas nor Electricity Committees. It is earnestly hoped, especially in view of the recent trials, that at least impartial consideration shall be accorded in the future to the rival illuminants.

Up to 31st March, 1912, it was generally understood that the price charged for gas for public street lighting was the same as to the ordinary householder in the city, viz. 2s. 3d. per 1,000 cub. ft. A reduction was made at and from the last-named date to 1s. 6d. per 1,000 cub. ft.

The Competition between the Rival Illuminants.

Arising out of a proposal to extend the high-pressure gas lighting system down Mosley-street, the City Council on 1st February, 1911, definitely authorized a scheme of competition between the two rival illuminants. Mosley-street was allocated to the Gas Department and Portland-street to the Electricity Department. No restrictions were placed on either department; each was to do its best.

The Mosley-street installation was completed first. After the Portland-street arc lamps had been put into commission, it was immediately apparent to the most casual observer that there was no possible comparison between the illuminating effect produced and the contemporary illumination produced by the high-pressure gas lamps in Mosley-street

and this notwithstanding the fact that at that time the Portland-street effect was regarded as anything but satisfactory.

The pronounced inequality in the illumination was presumably recognized by the Gas Department, and a further extension of the high-pressure gas lighting on a much more elaborate scale was at once proceeded with.

This paper is more particularly concerned with the comparative results of Princess-street and Portland-street, which may be regarded as typical examples of modern street lighting by high-pressure gas lamps and high-candle-power flame arcs respectively.

The Gas Installation in Princess-street.

Four Keith high-pressure lamps were suspended in Princess-street at the same height above the roadway as the arc lamps, namely, 27 ft. 6 in. The distance between the lamps varied from 95 ft. 6 in. to 118 ft. 9 in., but 106 ft. 6 in. may be taken as approximately the average.

Each lamp contained three inverted burners, and clear globes were used. At normal pressure each burner was rated at 1,500 c.p., or a total of 4,500 c.p. for the complete lamp; but, as will be seen from the test results, the maximum candle-power obtained was only about half this figure (see Fig. 21).

As originally installed, the lamps were fitted with traversing and lowering gears; but these were apparently not successful, as the lamps were at a later date fixed permanently in position. The flexible gas supply tubing was also replaced by rigid galvanized gas barrel.

Princess-street is 60 ft. wide, and as the lamps were on an average only 106 ft. 6 in. apart, the resulting illumination was very good, and far superior to any previous example of high-pressure gas lighting in Manchester.

Presumably in order to improve still further the maximum illuminating effect, but certainly not the uniform distribution of the light, the lamps have been lowered about a foot.

The Arc Lamps in Portland-street.

The central suspension system was chosen for the lighting of Portland-street, and certain predetermined "units" of light were erected at such calculated distances apart as to give the maximum illumination for the least capital expenditure.

In addition to low initial costs the central lighting system has the following advantages, which appear to outweigh certain known disadvantages:—

(*a*) The distributing mains can all be kept to one side of the street.

(*b*) No separate lighting standards are required on the street pavements, with consequent advantage to pedestrian traffic.

(c) A more even illumination is obtained; in other words, the ratio of maximum to minimum illumination is less than with side lighting for a given amount of electrical power employed.

The traffic in Portland-street is of a very dense character all day long, and more especially between the hours 4 p.m.—6·30 p.m., when the shipping warehouses are working at high pressure. It was therefore deemed advisable to aim for a high standard of minimum illumination, viz. something of the order of 0·5 foot-candle.

Owing to the short time available it was not possible to make photometric tests on the lamps selected before they were actually installed, consequently all calculations of illuminating effect were based on data furnished by the lamp-makers. The minimum illumination at any point on a horizontal plane at ground level, under the conditions mentioned below, was expected to be not less than 0·44 foot-candle. This figure was not obtained with the lamps as at first installed, but has since been exceeded.

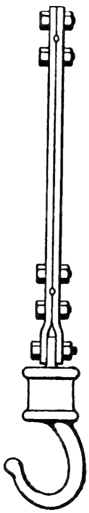


FIG. 1.

The length of Portland-street is 1,751 ft., and its width 66 ft. Sixteen 550-watt lamps, working 4 in series on the 200-volt mains, have been erected. Owing to the positions at which certain important side streets intersect the main street, the distance between lamps varies from 114 ft. to 124 ft.

Eight of the 16 lamps are run on an all-night circuit, and the remaining 8 are switched off at 11 p.m. The lamps are so arranged that, when all 16 are burning, the lighting is balanced across the 3-wire distributing mains; but after 11 p.m. the remaining 8 lamps are connected to one side of the system only.

The switching "on" and "off" is automatically controlled by time switches, provision being made to avoid interference with the latter when any lighting outside normal hours is required.

With six exceptions, which called for the use of the tramway side poles with extended arms, the arc lamps were suspended 28 ft. above the level of the street by means of two wrought-iron straps, from two steel wires of $\frac{5}{16}$ in. diameter placed 14 in. apart, one above the other in a vertical plane (see Fig. 1). Under these conditions the swinging of the span wires is largely counteracted.

The lamps are fixtures, in so far as no provision has been made for lowering or drawing them to the side of the street. All trimming has therefore to be done from a tower wagon. This decision was come to after carefully considering the extra expense and complication involved in arranging for lowering gear, and also with due regard to the type of lamp selected, the hours of burning, and the local conditions.

There are two lines of tramway metals laid in Portland-street, one

of which, however, is seldom used, and little or no dislocation of the traffic occurs even when the tower wagon has to be called out in the daytime. So far no difficulty has been encountered in trimming or even changing the lamps during the busiest portion of the day. As a matter of fact, arrangements have been made to enable the necessary trimming to be done during the early hours of the morning and when there is no traffic on the streets.

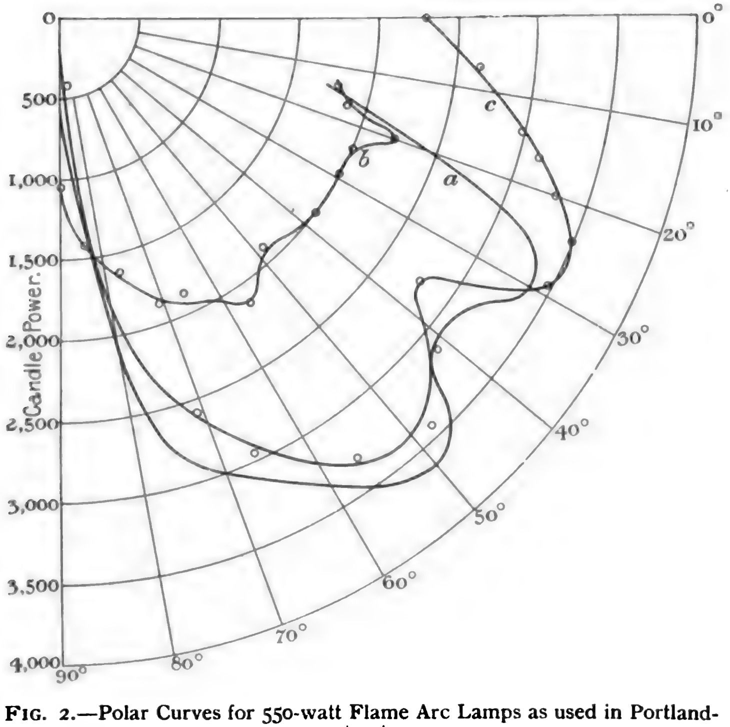


Fig. 2.—Polar Curves for 550-watt Flame Arc Lamps as used in Portland-street.

(*a*) With original inner and outer globes and spinnings. Outer globes clear.
(*b*) With original inner and outer globes and spinnings. Outer globes opalescent.
(*c*) With clear inner and outer globes of the type now used. Tested with "new type" carbons.

The cable connections to the lamps are as follows :—

From the street level to a height of some 8 ft., galvanized steel tubing is run up the building walls. The service branch cables drawn through the tubing then enter a connecting-box, and from the latter a heavily sheathed 7/18 S.W.G. twin vulcanized india-rubber cable is fastened to the upper part of the walls with raw hide cleats, and is then carried across the span wires to the lamps by means of the usual pigskin suspenders.

The lamps themselves are of the magazine type, and the actual hours of burning average 70; but it is usual to allow only 65.

First Results obtained in Portland-street.—The first results obtained were not considered altogether satisfactory, the shadows under the lamps, thrown by the ash-trays, were most pronounced, as was also

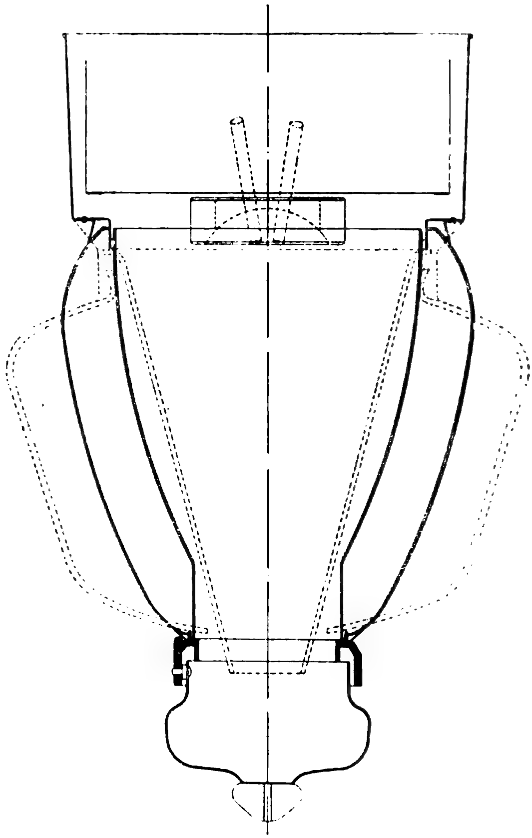


FIG. 3.—Sketch showing Inner and Outer Globes now used. The Original Globes and Spinnings are shown dotted in their relative positions.

the series of concentric rings on the surface of the roadway. As fitted with clear inner and opalescent outer globes, the lamps gave a maximum candle-power on the 20 degree ray of 2,250. This was substantially lower than the result anticipated.

Photometric tests showed that the polar curve of the lamps with the particular inner and outer globes used did not meet the necessary requirements, and the resulting distribution of the light was very unequal. The shadows and concentric rings were practically elimi-

nated by the use of slightly opalescent outer globes, but the efficiency of the lamps was impaired to a very appreciable extent, and the distribution of light was rather worse than before, the change from the dark zone midway between lamps to the bright zone adjacent to the lamps being very pronounced.

It is immediately apparent, from an inspection of the polar curves (Fig. 2), that neither the clear nor the opalescent outer globes were suited to the particular conditions obtaining in the lamps and the desired distribution of light.

Subsequent Alterations to the Lamps.—Certain modifications were therefore made to the lamps. To begin with, the height of the lamps was lowered from 28 ft. to 27 ft. 6 in., with a view to masking certain shadows of the trolley-wires thrown on the ground.

The most important alteration, however, was the outcome of a suggestion made to the authors by the superintendent of the Street Lighting Department, that whilst the lamps were undoubtedly giving their rated candle-power, yet, owing to the particular design of the outer opalescent globes and spinnings, practically all the rays from the horizontal to the 18-degree angle below the horizontal were entirely blotted out.

The construction of the spinnings supporting the globes, and the shape of the outer globes, were therefore considered to be largely responsible for the unsatisfactory distribution, and it was decided to modify the construction of the spinnings in such a way as to result practically in a lowering of the arc a distance of $\frac{3}{8}$ in. and also to use another type of outer globe in which there was less interference with the direct transmission of the light rays emanating in the neighbourhood of 20 degrees below the horizontal (Fig. 3).

The alterations resulted in a substantial increase in the candle-power emitted between the angles of 10 degrees and 20 degrees, the maximum ray attaining 3,600 candle-power at an angle of 23 degrees.

One objectionable feature remained, however, viz. the deep shadow vertically beneath the lamp. In the latter portion of this paper are described the series of experiments which were conducted to get rid of this, and which ultimately resulted successfully.

Reports on the Two Systems.

In the early part of 1912 the Manchester Corporation called in Mr. Jacques Abady and Mr. Haydn Harrison to report on the two systems of street illumination as carried out in Princess-street and Portland-street respectively. The results were to be expressed on an "equal basis of cost and illumination."

The joint and separate reports of these gentlemen have already obtained a wide publicity in the technical Press.

The Photometric Work Undertaken.

In order that the comparison between the various schemes of lighting might be both definite and reliable, and also to enable the

best results to be obtained from the arc lamps, a very considerable amount of photometric testing was necessary. This work was in progress for over twelve months, and many thousands of readings were taken during that period.

All the testing was done in the streets at night with the lamps burning under normal conditions, and consequently the results obtained are directly applicable to the requirements of practical work, and in that respect may no doubt differ to a considerable extent from similar tests conducted in laboratory photometer rooms. It is perhaps advisable to emphasize the fact that all tests were taken in the streets under working conditions, as otherwise the value of the results obtained might require discounting to a very appreciable extent, particularly in the case of figures relating to dioptric globes.

All measurements of the intensity of the illumination due to any particular ray, and therefore of the corresponding candle-power, were made approximately in the position, both as regards angle and distance from the lamp, at which the particular ray was effective in producing actual illumination. Consequently any errors due to the inapplicability of the inverse square law were practically eliminated.

The photometer used was a modification of an instrument constructed by Messrs. Alexander Wright & Co. It may be briefly described as an oblong rectangular wooden box divided into two compartments, one of which contains the sub-standard of light, and the other the actual photometer head or comparison device.

Between the two compartments there is an arrangement of adjustable shutters operated by a micrometer screw; the extent of the shutter opening, as measured by the graduations on the micrometer screw-head, constituting a measure of the illumination received on the photometer head. The range of the illumination can be considerably extended by means of a lens which acts as a condenser and is interposed between the standard lamp and the photometer.

The photometer head is of the well-known Simmance and Abady flicker type, consisting of a flicker disk driven by a clockwork motor, with the necessary sighting telescope, focussing lenses, and scale of angles.

The compartment containing the photometer is also provided with a set of small doors which can be opened at any desired angle, thus enabling the photometer to be focussed on a source of light at any angle between 0 and 90 degrees below the horizontal. This photometer therefore measures the actual light flux density of the incident ray, from which values for candle-power and the intensity of illumination on vertical or horizontal planes can readily be calculated.

The complete apparatus is mounted on a special cart, by means of which it can be wheeled about as required. When set up for taking observations, the cart is raised on three legs so that the wheels are clear of the ground; the stand supporting the photometer is arranged to permit of levelling in any direction, and in addition, a movement of roughly 30 degrees about a vertical axis is also possible,

Owing to numerous structural alterations, and for no other reasons, it was not conveniently possible to arrange the photometer for taking readings at a less distance than 5 ft. from the ground.

The suggested international standard height for photometric measurements of outdoor illumination is 3 ft. 3 in. (*i.e.* approximately 1 metre) above the ground level. Many of the measurements actually obtained on a plane at a height of 5 ft. have therefore for purposes of comparison been reduced to their corresponding values on a horizontal plane 3 ft. 3 in. above the ground.

The choice of a suitable height for photometric measurements is not apparently a very easy matter. Except for the fact that a metre is a very convenient unit of length there is apparently no particular advantage attaching to the use of a plane at this height for photometric measurements, and in some respects it is a very inconvenient height.

Measurements of the illumination on a plane normal to any particular ray or of the horizontal component of the illumination, *i.e.* the illumination on a vertical plane, would probably be more in accordance with ordinary conditions if taken on a plane not less than 4 ft. 6 in. above the ground level. In cases where the minimum horizontal illumination is taken as a criterion of the value of a system of lighting there is much to be said in favour of taking measurements of the actual illumination on the ground level. In practice this would of course be done by means of calculations based on data actually obtained at a more convenient level.

It should be clearly understood that what is actually meant by the term "horizontal illumination" is the vertical component of the light flux density on any plane normal to the incident ray, and it is advantageous to obtain this value by direct calculation, without any reference to the particular nature of the ground surface.

The actual illumination on the horizontal plane may frequently with advantage be defined in terms of the values calculated for the ground surface, for in that way a much more uniform curve of illumination is obtained. Further, the lay observer invariably looks on the ground surface when judging the value of the horizontal illumination in a street. It will also generally be found that the illumination on a vertical plane is usually of more importance at a height of 4 to 5 ft. above the ground surface.

The method of obtaining the data from which the numerous curves illustrating this paper were drawn, was briefly as follows :—

The photometer was first set up immediately under the lamp to be tested, or as near as possible to the base of the post. The photometer head was then focussed upon the lamp, and the angle between the normal incident ray and the horizontal was observed. The intensity of the illumination, or light flux density, on a surface normal to the incident ray was then measured by direct comparison with the illumination from the sub-standard lamp.

The photometer was then moved a short distance away from its initial position, usually about 2 ft. 6 in., and the observations again

taken. This was repeated by gradually extending steps until a distance of about 90 ft. from the lamp was attained.

In special cases, where possible, the lamp was also lowered in order that measurements might be taken at small angles to the horizontal, and also actually on the horizontal. Before making any extensive street tests a considerable number of observations were made on a lamp specially suspended for experimental purposes, and it was found that with the original opalescent globes the candle-power at the various angles, and consequently the shape of the polar curves, was not altered to any important extent by alterations of the angle between the plane

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FIG. 4.—Luminometer Test-card.

of the carbons and the vertical plane cutting the lamp under test and the photometer. Subsequent tests with the clear outer globes gave variations differing by as much as 10 per cent from a mean value; but as all curves have been drawn from the average results of numerous tests made at different angles to the planes of the carbons, these variations have been ignored.

Two 6-volt Osram lamps were used alternatively in the photometer as sub-standards of luminous intensity, thus providing a check on the measurements. The lamps were supplied from a 20-ampere-hour accumulator, and before conducting the actual tests the battery was charged and discharged to such an extent that the lamps were always

burning when the discharge curve of voltage was practically horizontal. Periodical checks were made in the photometer room between the Osram lamps and the laboratory standard.

In addition to photometer measurements, a few observations were also taken with a luminometer. This device simply consisted of a rectangular wooden box into which were fitted two rectangular tubes set at an angle of approximately 45 degrees to each other. The box was painted dead black inside, and underneath one of the openings a test-card similar to Fig. 4 was fixed. Careful observations were then made of the actual distance from the various lamps at which the different sets of type were just visible. The reason for the somewhat eccentric typography is therefore obvious. It was of course necessary to take precautions to avoid illumination from other sources than the lamp under observation.

The results of the numerous photometric tests are given graphically in this paper, and take the form of polar curves of candle-power, curves showing the horizontal component of the illumination on a vertical plane at a height of 3 ft. 3 in. above the ground level, and curves showing the vertical component of the illumination on horizontal planes 3 ft. 3 in. above the ground, and on the surface of the ground.

Probable Limits of Accuracy.—The results of street photometric tests are very frequently questioned, and gas authorities invariably dispute their accuracy. It is therefore advisable to consider briefly the limits of accuracy desirable and obtainable in practice.

The gauge or conception of the intensity of illumination by the human eye follows a logarithmic law, or, in other words, it is only the percentage variation in the intensity of illumination which is noticeable. Consequently small variations are of no importance, and it is even very doubtful whether it would be possible without the aid of photometric apparatus to detect a difference in the intensity of illumination in a street within a range of, say, 10 per cent. A consistently maintained degree of accuracy within a limit of 5 per cent is therefore all that is necessary for ordinary street work, and in fact such a limit is recognized as sufficiently close by most of the authorities on photometric work.

Great care was taken in the calibration and subsequent checking of the photometer used for the work recorded in this paper, and as all the curves obtained were plotted from the "means" of numerous readings taken with every necessary precaution, it may be reasonably assumed that the limit of possible error did not exceed 5 per cent, and in many cases was probably less.

It is very questionable whether it would be possible to guarantee any street photometric measurements as correct within a limit of error of less than 3 per cent, but in any case the complications and extra work involved would be quite beyond the possibilities of tests conducted on anything approaching a commercial scale.

General Comparisons between the Lighting in Portland-street and Princess-street.

Any comparison of the Portland-street and Princess-street lighting, other than on an "equal basis of cost and illumination," requires to be very unbiased, since there is much to be said in favour of both systems.

The gas lamps give a much steadier light than the arc lamps, although the difference is not very noticeable to a casual observer, or even to a keen observer; but it is very noticeable when making measurements with a flicker photometer, and the severe eyestrain incidental to the use of such a photometer is in consequence considerably reduced.

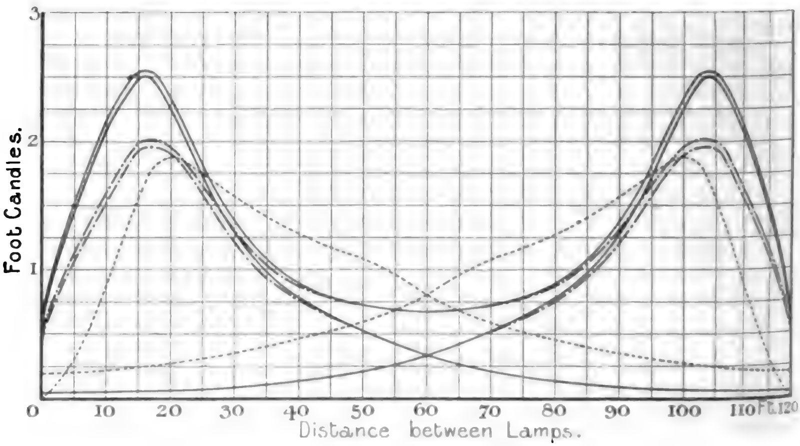


Fig. 5.—Curves showing the results of the Photometric Tests in Portland-street obtained with the first experimentally Obscured Globes.

Full-line curves represent illumination on a horizontal plane 3 ft. 3 in. above the ground level; dotted curves, the illumination on a vertical plane; chain-line curves, the illumination on the ground. The effect of the shadows under the lamp is clearly noticeable.

Unfortunately, although the gas-lamps give a steadier light, their candle-power varies very considerably from day to day. The candle-power of a particular lamp may fall at least 50 per cent before the mantles are renewed, unless it is arranged to change only one mantle at a time, thus spreading the complete change over a fairly long period.

The candle-power of the arc lamps may vary quite appreciably within a few minutes, but provided that the same make of carbons is used the average candle-power at any particular angle will not change to any extent from day to day if the line voltage is reasonably constant.

A very important feature directly affecting the comfort of the general public is the absence or otherwise of glare. Glare is purely a physiological effect, and is apparently dependent upon the intrinsic brilliancy of the light source and the angle of incidence of the light rays received by the eye. Obviously, therefore, the larger the surface of the light

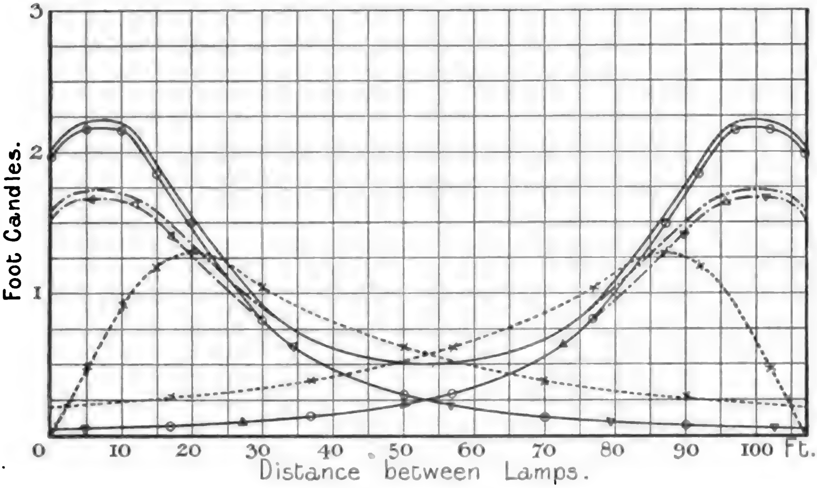


Fig. 6.—Curves showing the results of the Photometric Tests on the Princess-street 3-burner High-pressure Gas Lamps.

Full-line curves represent illumination on a horizontal plane 3 ft. 3 in. above the ground level; dotted curves, the illumination on a vertical plane; chain curves, the illumination on the ground.

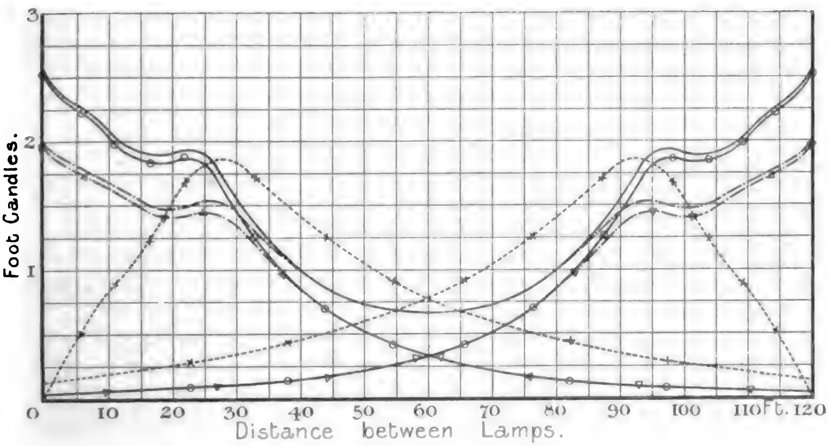


Fig. 7.—Curves showing the results of the Photometric Tests on the Portland-street 550-watt Lamps, fitted with the latest type of Outer Globe having a Graded Frosting.

Full-line curves represent illumination on a horizontal plane 3 ft. 3 in. above the ground level; dotted curves, the illumination on a vertical plane; chain curves, the illumination on the ground. The elimination of the shadows under the lamps should be noted.

source in proportion to the total amount of light emitted, the lower the intrinsic brilliancy and the less the effect of glare. In this respect, the 3-burner high-pressure gas lamps have an advantage over the flame arc lamps with clear inner and outer globes. The use of clear outer globes with flame arc lamps is in fact hardly advisable from the point of view of scientific lighting, since any form of lighting which is productive of eyestrain is essentially unscientific, and is certainly unsatisfactory.

Two noticeable features in Princess-street are the comparative softness of the shadows of objects cast on the ground and the absence of a pronounced dark horizontal zone in line with the reflectors. The first result is undoubtedly due in a great measure to the fairly large triple light source, which has the effect of shading off the edges of shadows; and the absence of a dark zone in line with the reflectors is no doubt due partly to the fact that the source of light is well below the reflector, and partly to the reflection from the inner surface of the large globes.

The shadows cast by the flame arc lamps when burning with clear inner and outer globes are very intense, and there is no appreciable shading of the edges, consequently it is possible to confuse shadows with actual objects. The smallness of the light source, or in other words the high intrinsic brilliancy, and the fact that the arc is well up under the reflector is no doubt the cause of the objectionable horizontal dark zone noticeable in cases where reflectors are used.

The Portland-street light has a much warmer and more cheerful effect than the comparatively cold light in Princess-street.

The actual relative values of the two schemes of lighting are clearly shown by the various tables and curves giving the results of photometric tests, and the contour curves showing the portions of the street area within which the illumination on a horizontal plane is equal to or greater than 0·5 foot-candle.

The curves (Figs. 5, 6, 7) show the horizontal and vertical components of the illumination on planes 3 ft. 3 in. above the ground, and on the ground level. Table III gives a summary of the actual results obtained. The figures in Table II were obtained with a luminometer, and clearly show that as regards the intensity of the illumination at a considerable distance from the lamps the flame arcs give better results than the gas lamps.

Purely from the point of view of illuminating effect there is much to be said in favour of both systems; but the electric lighting system possesses all the practical advantages, a few of the more important of which are:—

(*a*) Lower cost.
(*b*) Simplicity of switching operations, and possibility of dispensing with lamplighters.
(*c*) Flexibility and ease of erection.
(*d*) Lamps not affected by vibration when suspended from traction poles.

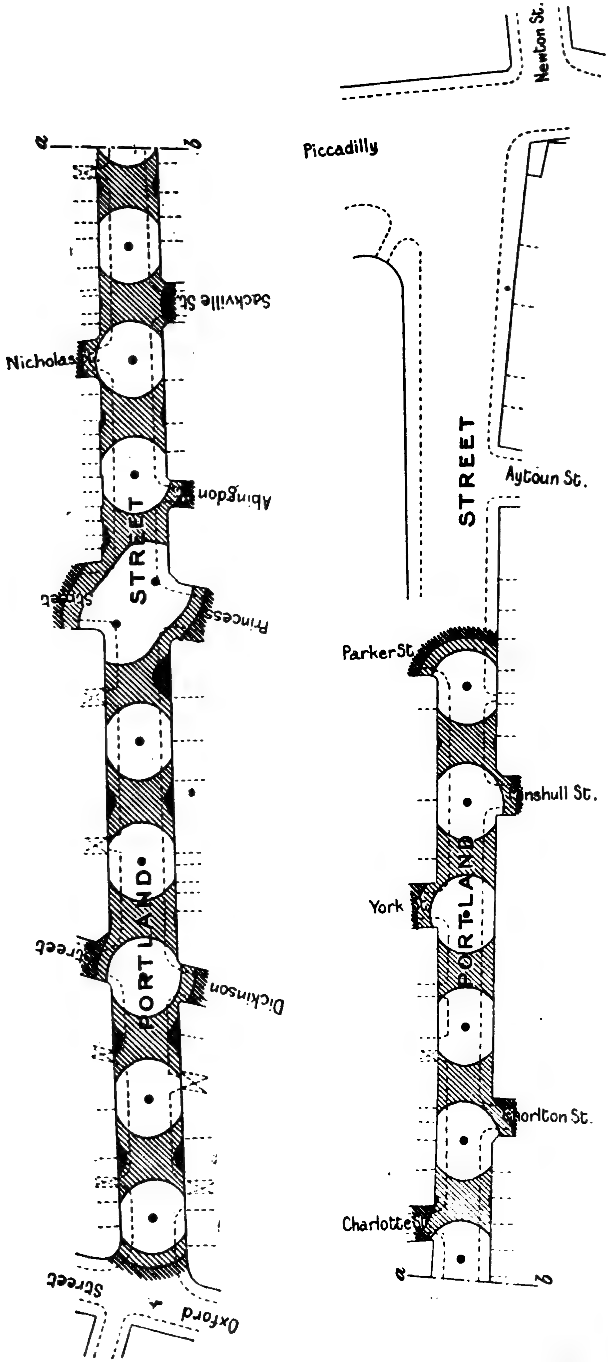


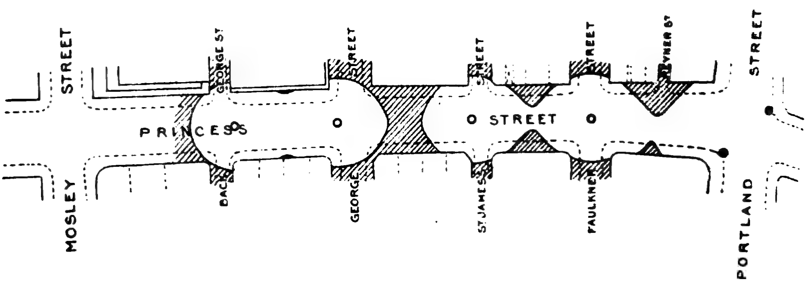
FIG. 8.—Contour Curve for Portland-street, showing the results obtained with the latest Experimentally Obscured Globes.

Illumination within the dark shaded area on a horizontal plane 3 ft. 3 in. above the ground level is less than 0·5 foot-candle; the illumination within the light shaded area is less than 1 foot-candle and not less than 0·5 foot-candle; the illumination within the area shown white is equal to, or greater than, 1 foot-candle.

(*e*) Possibility of reliable check on running costs (*i.e.* current consumption and carbons).
(*f*) Negligible leakage.
(*g*) Absence of globe breakages due to heating, etc.

All the above advantages are absent in the case of the high-pressure gas system, and in contrast may be mentioned the disadvantages incidental to its use :—

(*a*) Extensive and highly dangerous leakage of high-pressure gas.
(*b*) The detrimental effect of a foggy or heavily smoke-laden atmosphere on the mantles, resulting in a serious diminution of candle-power just at a time when it is most required.
(*c*) Partial and occasionally complete failure in frosty weather.



o = Keith, 3-burner, High-pressure Gas Lamp.

FIG. 9.—Contour Curves for Princess-street Lighting.

Shaded portions represent the areas within which the horizontal illumination on a plane 5 ft. above the ground is less than 0·5 foot-candle ; the illumination within the area shown white is equal to, or greater than, 0·5 foot-candle.

CONFIRMATION OF THE PORTLAND-STREET RESULTS AS FURNISHED BY THE REPORTS OF THE EXPERTS.

Reference has been made to the results of the first photometric tests in Portland-street, and consequently the value of the subsequent experimental work is clearly indicated by the successful results now obtained.

Portland-street is a most unsatisfactory street to illuminate, owing to the nature of the buildings and the absence of any appreciable amount of reflection ; nevertheless, the present lighting is probably as good an example as will be found elsewhere of uniform street lighting with a high average intensity of illumination on the horizontal plane, and a reasonable absence of glare.

The minimum intensity of the illumination on a horizontal plane 3 ft. 3 in. above the ground is, with unimportant exceptions, 0·5 foot-candle, and the ratio of maximum to minimum illumination in the centre of the road is 3·75. The ratio of the maximum to the minimum illumination on the horizontal plane may conveniently be referred to as the "variation factor."

When carefully analysed it will be noticed that the experts' reports very closely confirm the values of candle-power and illumination claimed by the Corporation Electricity Department as a result of the tests made by their own staff.

Mr. Abady refers to the meaning of "illumination" and the difficulties incidental to its measurement. Throughout this paper, illumination has been regarded as the equivalent of impressed light flux density multiplied by the cosine of the angle of incidence. Light has also been assumed to be a vector quantity. This assumption is not strictly correct under all conditions, but it does not affect the results in question.

Apart from actual values, the proportionality of Mr. Abady's figures for the illumination of Portland-street and Princess-street is in fairly complete agreement both with the Electricity Department's and Mr. Harrison's results; and his values for the candle-powers of the Princess-street lamps provide important confirmation not only of the Electricity Department's tests, but also of the fact that the lamps were not giving anything like the candle-power claimed by the manufacturers.

This result is both interesting and important, for it has always been the experience of the authors that gas lamps give substantially lower candle-powers than the values claimed by the manufacturers.

This is evident from the figures given in Table I.

TABLE I.

Description of Lamp.	Year of Test.	Rated Candle-power.	Actual Maximum Candle-power. (Average Results.)
Keith, 3-burner high-pressure gas	1912	4,500	2,300
Keith, 2-burner high-pressure gas	1912	3,000	1,630
Keith, single-burner high-pressure gas	1912	1,500	725
Welsbach-Kern, twin-burner	1907	1,200	655
Suggs "Belgravia"	1907	1,800	1,025
Lucas "Thermopile"	1907	1,250	765
Intensified gas, Sackville-street	1904	1,000	525

RELATIVE COSTS OF THE TWO SYSTEMS.

In Appendix A is set forth the method of arriving at the cost of the current under the conditions that obtain in Portland-street.

The totals for "fixed" and "running" costs respectively are arrived at in the manner shown, and the question of how these should be applied to the special case of a street-lighting load may give rise to some difference of opinion.

There are three possible methods :—

1. To divide the actual totals of the "fixed" and "running" charges respectively by the total maximum demand on the undertaking and the total units sold for all purposes.
2. After arriving at the totals for the "fixed" and "running" charges, to deduct therefrom the traction costs (ascertained in the manner laid down in the recent Model Report of the Municipal Tramways Association and the Incorporated Municipal Electrical Association); and then to divide the remainders by the respective totals for the maximum demand and the number of units sold for lighting and power.
3. As in (2), but to differentiate further between lighting and power supplies.

The authors consider that the course outlined under (3) is the correct one; the resultant figures are £6·133 per kilowatt of demand plus 0·232d. per unit metered. Applying these values as shown in Appendix B, the cost of the current for the 11 o'clock and for the all-night lamps respectively come to 0·97d. and 0·6d. per unit.

Appendix C contains a statement of all costs involved in lighting Portland-street.

The cost of uncompressed gas per 1,000 cub. ft. (as deduced from the published accounts of the Gas Department) is stated by the gas authorities to be 12·69d., and the corresponding figure for compressed gas is 13·89d.

Appendix D is a statement showing the cost per lamp per annum of lighting Princess-street, as given by Mr. Abady in his report. Nothing is included in these figures for interest and depreciation on capital spent on the installation.

For the purpose of ready comparison, the following particulars of the lighting in Princess-street and Portland-street are extracted from Mr. Hadyn Harrison's report. In order to make the figures strictly comparable, the Portland-street lamps are assumed to be all switched out at 11 p.m.

	Princess-street. High-pressure Gas.	Portland-street. Arcs.
Candle-power of lamps	1,750	2,970
Number of lamps to the mile ...	49·34	43·6
Running costs per lamp per hour ...	1·5d.	0·7d.
Capital cost per mile of street ...	£2,537	£1,569
Running cost per 1,000 c.p.-hours ...	0·857d.	0·236d.
Cost per annum per mile, equal illumination	£675	£254
Minimum illumination, basis of comparison	0·39 ft.-candle	0·5 ft.-candle
Cost per mile of street per annum (up to 11 o'clock) at above illumination	£617	£254

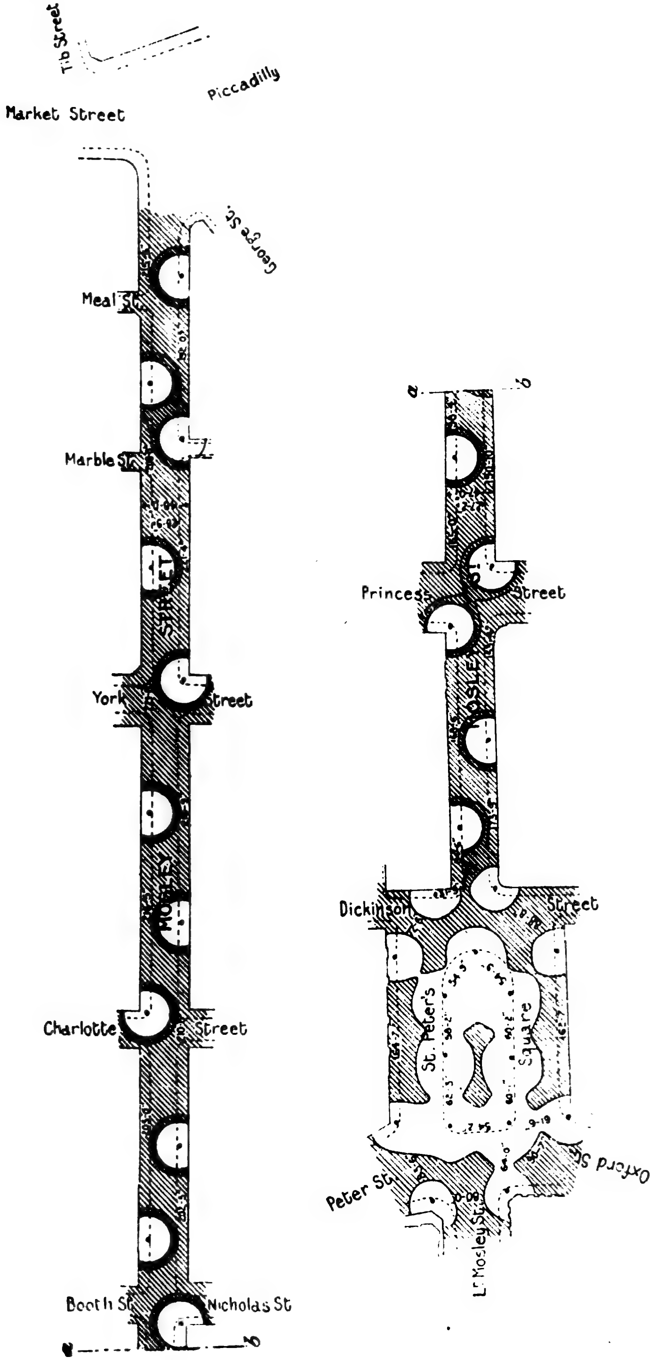


FIG. 10.—Contour Curves of Illumination in Mosley-street and St. Peter's-square.

The illumination within the shaded areas on a horizontal plane 5 ft. above the ground is less than 0·5 foot-candle, and the illumination within the whole of the area shown white is equal to, or greater than, 0·5 foot-candle.

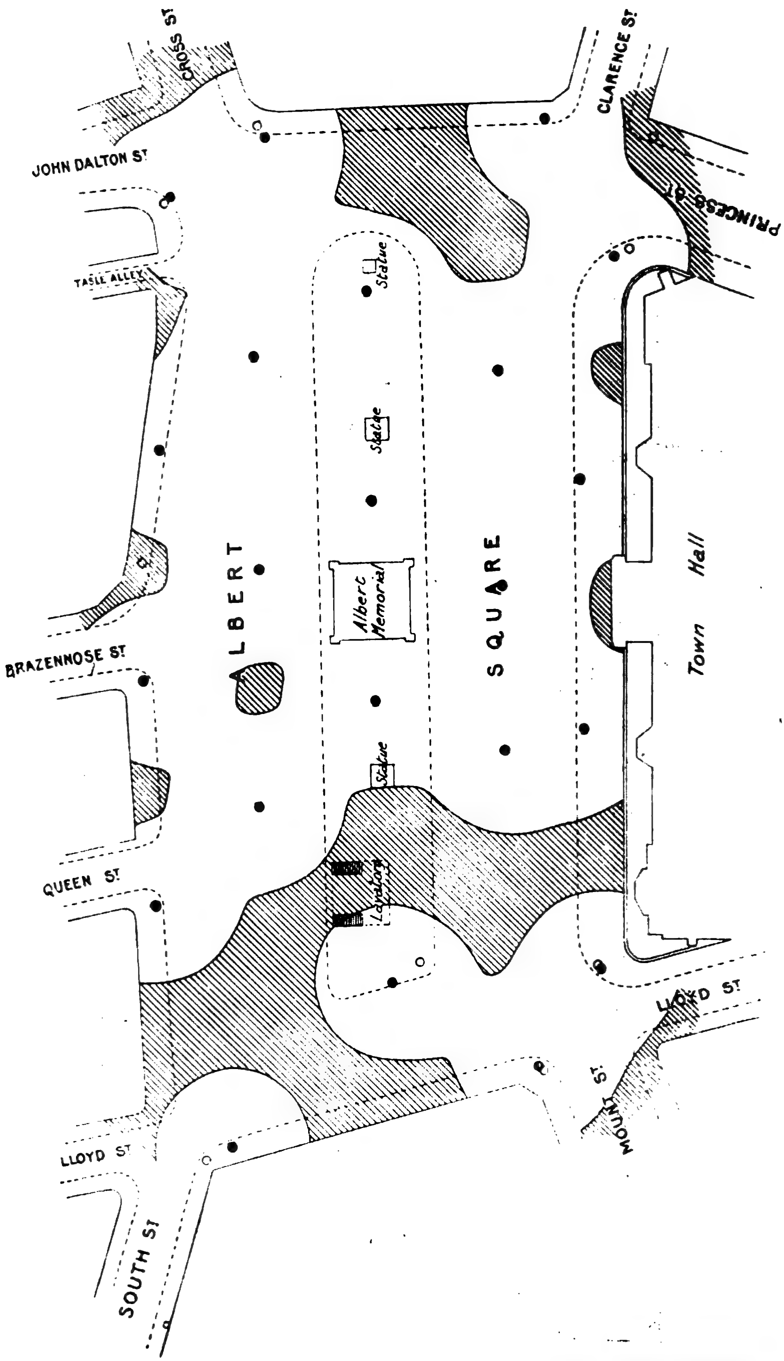


FIG. 11.—Contour Curves for Illumination in Albert-square.

The illumination within the shaded areas on a horizontal plane 5 ft. above the ground is less than 0·5 foot-candle, and the illumination within the whole of the area shown white is equal to, or greater than, 0·5 foot-candle.

The Lighting in Mosley-street, St. Peter's-square, Albert-square, and Piccadilly.

It has already been stated that the high-pressure gas lighting in Mosley-street is a very poor display compared with the arc lighting. It is also a very poor display of high-pressure gas lighting, as will be clearly seen from the contour curve (Fig. 10).

An illumination on the horizontal plane 5 ft. above the ground level equal to 0·5 foot-candle is only obtained up to a distance of 22 ft. from the base of the lamp-posts, and the minimum midway between the posts is only 0·07 foot-candle, the "variation factor" being as high as 43.

St. Peter's-square is distinctly a better example of high-pressure gas lighting, and compares favourably with the flame arc lighting in Albert-square. The illumination on a horizontal plane 5 ft. above ground level is maintained at a sufficiently high value, and is equal to 0·5 foot-candle in all parts where there is any great amount of traffic. This is also clearly shown by the contour curves (Fig. 10). The central dark portion is over a grass plot, and is therefore of no consequence.

The general nature and extent of the present illumination in Albert-square and Piccadilly are clearly indicated by the contour curves (Figs. 11 and 12).

The 500-watt lamps in Albert-square are fitted with slightly opalescent globes, and are mounted on poles at a height of 22 ft. 8 in. above the ground. The illumination is fairly uniform, with the exception of the two dark patches shown. It is intended, however, to eliminate these entirely, and also to improve the light distribution generally by a suitable modification of the globes.

The average horizontal illumination in Piccadilly is well over 1 foot-candle, and in no part where there is any appreciable amount of traffic is it less than 0·5 foot-candle. The roadway is lighted by means of twelve 500- and four 550-watt lamps fitted with clear inner and outer globes, and fixed on the tramway standards at a height of about 28 ft. above the ground. In addition, there are also ten 360-watt lamps on the esplanade. These lamps have opalescent bowl-shaped globes. They are suspended from swan-necked pole-brackets at a height of 18 ft. 6 in.

The Physiological Problem—Colour Differences.

Reference has been made to the limits of attainable accuracy in connection with the photometric measurements; but such limits are applicable only in a physical sense, and it is therefore probable that they may require to be further extended in order to accommodate the possible discrepancies arising from the physiological factors involved. The flicker photometer used enabled a balance to be obtained between lights varying appreciably in colour; but it cannot for that reason be assumed that the intensities of the illumination on the two sides of the photometer disk were therefore equal, for the physiological con-

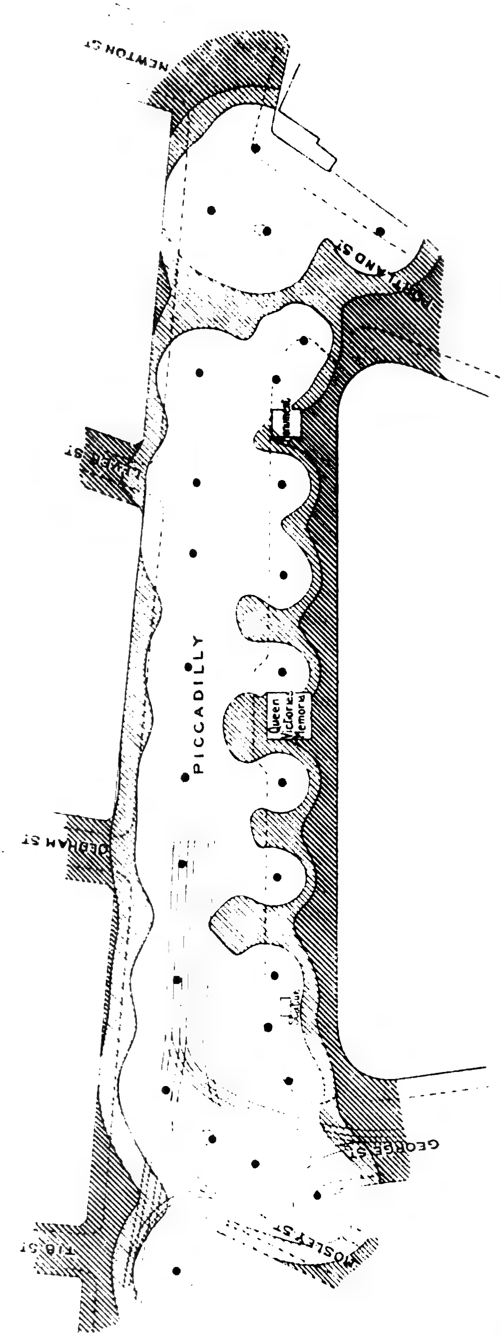


FIG. 12.—Contour Curves for Illumination of Piccadilly.

The dark shaded portions show the areas within which the illumination on a horizontal plane 5 ft. above the ground level is less than 0·5 foot-candle. The light shaded portion is the area within which it is more than 0·5 and less than 1 foot-candle; and within the white area the illumination is equal to, or greater than, 1 foot-candle.

ceptions involved are not definable. Much recent research has not so far resulted in any very definite pronouncement on the subject of colour photometry.

In the case of the tests in question, the light from the Osram lamps used as sub-standards differed in colour to an apparently similar extent from the light of both the flame arc lamps and the high-pressure gas lamps. As this difference was not abnormal, the physiological effects, if existent, could not have been very pronounced. The sub-standard lamp, however, is only the equivalent of the counterweight used in connection with the method of double weighing, and consequently the comparison between the arc and the gas-lamps would still be affected by any incidental physiological phenomena; but it is probable that such discrepancies are not of appreciable importance.

At the same time it must be admitted that there is a difference in colour between the light from the flame arc lamps and that from the

TABLE II.

Sizes of Type as per Fig. 4.	Maximum Distance at which distinctly readable.	
	High-pressure Gas.	Flame Arcs.
Large	339·0 ft.	371·5 ft.
Medium	265·0 „	313·0 „
Small	212·5 „	242·0 „

high-pressure gas lamps, and it is therefore of interest to consider the probable effect of such difference.

If the well-known "Purkinje" effect may be taken as a guide, the arc lamps should give a superior illumination in their immediate vicinity, and the gas lamps a superior effect at a distance. The point of equality is probably in the neighbourhood of an intensity of normal illumination equal to about 1 foot-candle.

Visual observations confirmed the excellent carrying power of the light from the gas lamps, and it was therefore decided to obtain definite comparisons by means of the luminometer. These tests showed that the flame arc lamps were quite the equal of the high-pressure gas lamps, so far as the resulting illumination at a distance was concerned; but this was probably to a certain extent due to the higher candle-power of the arc lamps at the angles concerned. These tests and observations (Table II) refer to clear weather; there is no question as to the superior penetrating power of the rays from the arc lamps in foggy weather.

All figures are the average of not less than six sets of observations taken on different nights.

Globes, Diffusers, and Reflectors.

A very considerable amount of testing and experimental work has been undertaken in order to discover, if possible, the most satisfactory

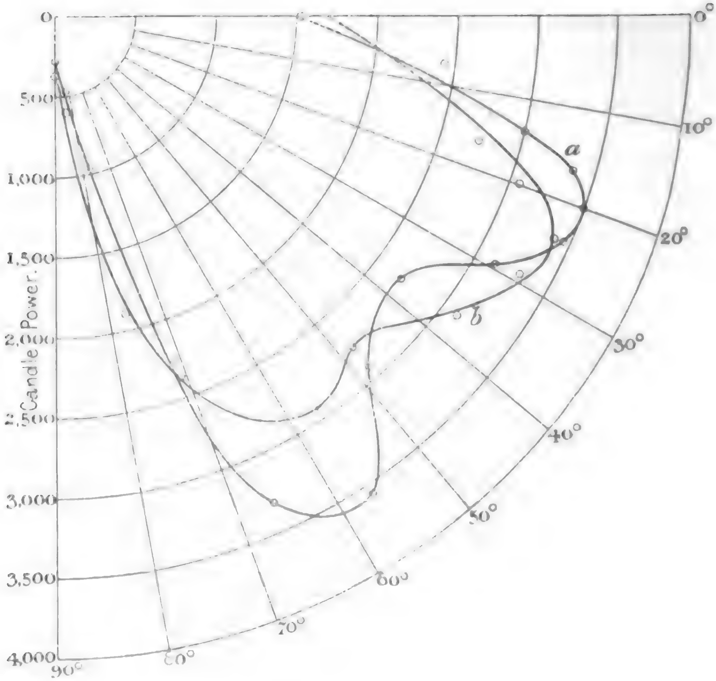


Fig. 13.—Polar Curve for 550-watt Flame Arc Lamp as used in Piccadilly.

(a) With clear outer globes.
(b) With slightly opalescent outer globes.

type of outer and inner globe for use under the conditions obtaining in Portland-street, and to comply with the following requirements:—

- Reasonably uniform illumination.
- Absence of glare.
- Moderate cost of globes.
- Light distribution not affected by slight alteration in the position of the arc.
- Maximum candle-power approximately between the 20- and 25-degree rays below the horizontal.

A large number of tests were made, and much of the information obtained is given graphically in the accompanying polar and illumination curves.

As previously mentioned, the light distribution with the original

globes was anything but satisfactory, for in addition to most pronounced shadows under the lamps the general illumination was very unequal. The glare was also very objectionable, and must have caused a considerable amount of personal discomfort to passengers on the open tops of tramcars.

The first attempt to overcome these defects was by the use of opalescent outer globes of the same shape as the clear globes. The result was a distinct diminution of the shadows under the lamps, and

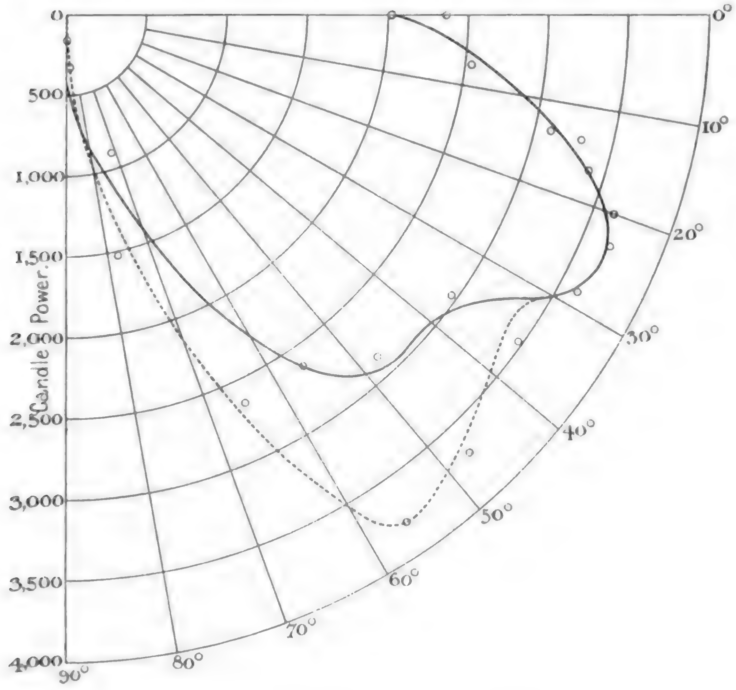


FIG. 14.—Polar Curve for 550-watt Flame Arc Lamp as used in Portland-street. The dotted portion shows the effect of the original experimental obscuration. Tested with "old type" carbons.

the complete disappearance of the concentric rings; but unfortunately the light distribution was very considerably reduced, and the dark gaps midway between the lamps were very noticeable.

Various types of dioptric and inner diffusing globes were then tried, but without appreciable success. This lack of success was no doubt due in a great measure to the position of the arc and the shape of the original outer globes. It was not until the type of outer globe now in use had been adopted that any headway was made with the various attempts to improve the light distribution (see Fig. 3).

Fig. 14 shows the polar curve for the 11-ampere lamp fitted with

the clear inner and outer globes. It must be admitted that this curve possesses many excellent features, but at the same time it can be modified with considerable advantage according to the class of work for which the lamp is intended to be used.

For street-lighting work, the upper portion of the curve between 15 and 25 degrees from the horizontal is of most importance, but the

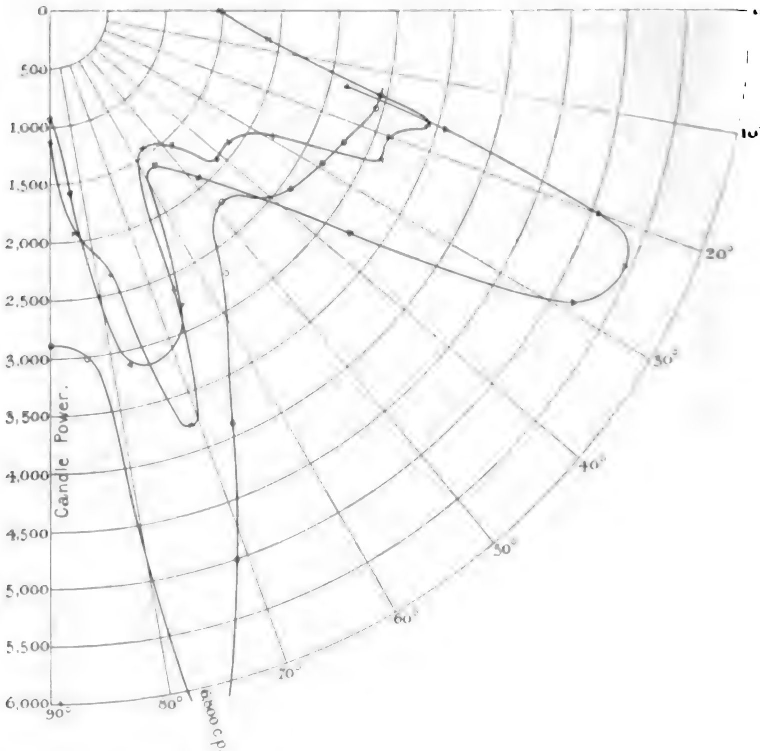


FIG. 15.—Polar Curves for 550-watt Flame Arc Lamp fitted with Clear Outer Globe of the shape now in use, and with Dioptric Inner Globe. The different curves show the effect of varying the relative positions of arc and inner globe.

portion between the 40-degree and 65-degree rays could be reduced with advantage. The absence of appreciable candle-power between 80 degrees and 90 degrees is a result of the rather pronounced shadows cast by the ash-trays.

The first attempt to reduce the distribution of light between the 40-degree and 65-degree rays, and at the same time to improve the distribution in the neighbourhood of 20 degrees below the horizontal, was by the use of dioptric globes. The results were not entirely successful, as the curves (Fig. 15) will show. One is obviously the result of

incorrect focussing, and the others clearly indicate the intense splash of light which is always obtained between the 60-degree and 80-degree rays, as a result of which the rest of the illumination looks poor by comparison, although in reality not at all bad.

These tests seem to demonstrate very clearly a rather objectionable feature of dioptric globes, namely, the strongly defined optical centre, in consequence of which the light distribution is very considerably distorted if there is any appreciable change in the arc position.

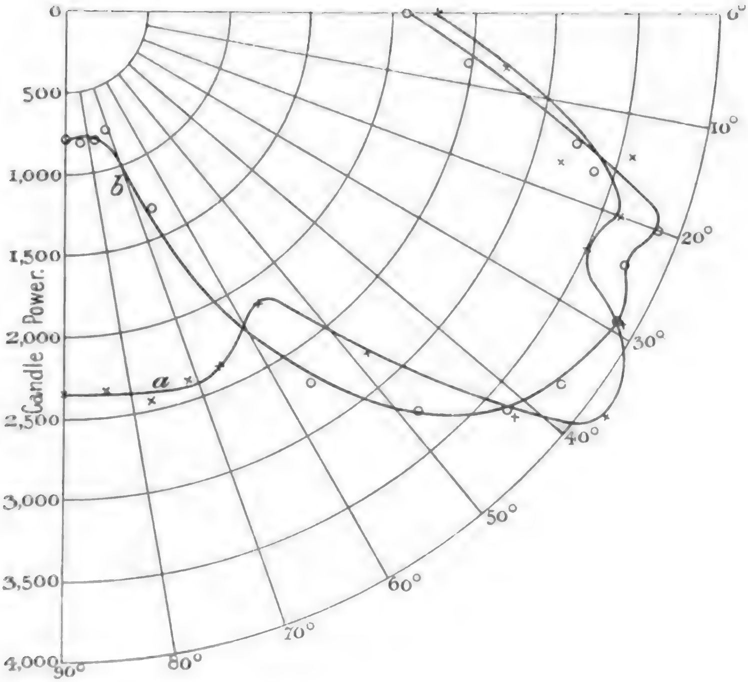


FIG. 16.—Polar Curves of 550-watt Flame Arc Lamp.

(*a*) With clear outer globe having a dense frosting on the lower portion, extending up to about 48 degrees from the horizontal.

(*b*) With clear outer globe having a graded flashing on the lower portion, the flashing being densest at the bottom of the globe, and gradually tapering off to about 40 degrees below the horizontal.

It is quite possible that the splash of light between the 60-degree and 80-degree rays might be still further reduced, but all attempts at so doing by relative changes in the position of the globe and the arc were unsuccessful. It is therefore very probable that the open lower end of the dioptric globe is largely responsible for the very marked dip in the polar curve.

Attempts were then made to modify the shape of the polar curve by suitable obscuration of the outer globes, and it was owing to the

success of the early attempts that further investigations were conducted upon these lines. The necessary obscuration was at first obtained by the application of whitening on the inside of the lower portion of the outer globe, and the results obtained were rather remarkable. Unfortunately, owing to the nature of the obscuration, it was a very difficult matter to do two or more globes exactly alike, and consequently reliable test figures were only obtained with considerable difficulty.

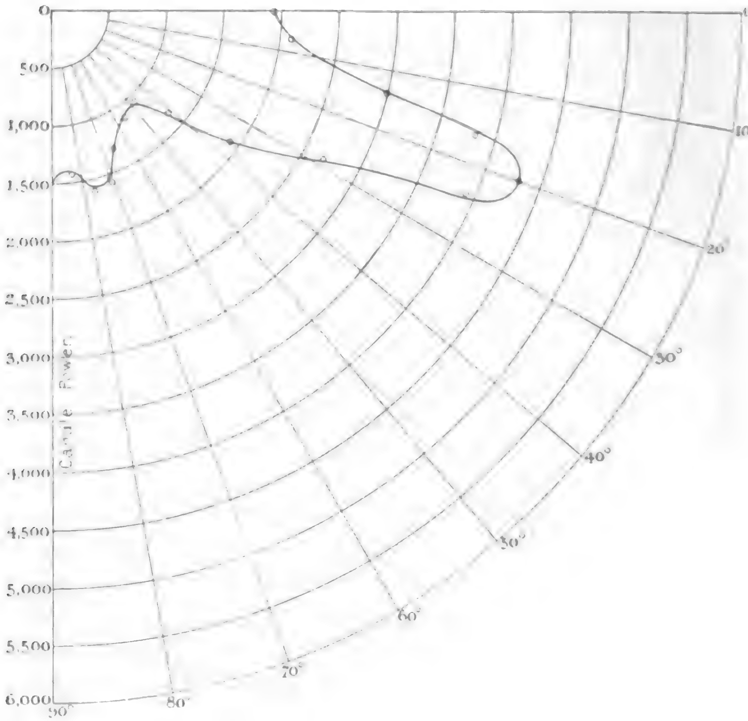


FIG. 17.—Polar Curve for 550-watt Lamp with Dioptric Inner Globe and the same Outer Globe as for Fig. 16 (*a*).

Owing to the comparatively opaque and very materialistic nature of the obscuration, the effect was merely to cut down the light between the 40-degree and 80-degree rays, there being otherwise very little diffusion or alteration in the shape of the polar curve, although slight traces of reflection were apparent which had the effect of improving somewhat the candle-power nearer the horizontal. The shadows under the lamps were not greatly reduced. There was a very great improvement in the curve of illumination on the horizontal plane, and the glare was appreciably reduced.

Globes obscured in this manner have actually been in use in Portland-street for over six months, and no doubt the slight difference between the test results obtained by the Corporation's own staff and the independent experts are traceable to the variable nature of this obscuration, and also to changes in the type of carbons used.

Attempts were then made to obtain from the globe-makers suitably obscured globes, but the greatest difficulty was experienced in obtaining exactly what was required. Apparently the makers were able to

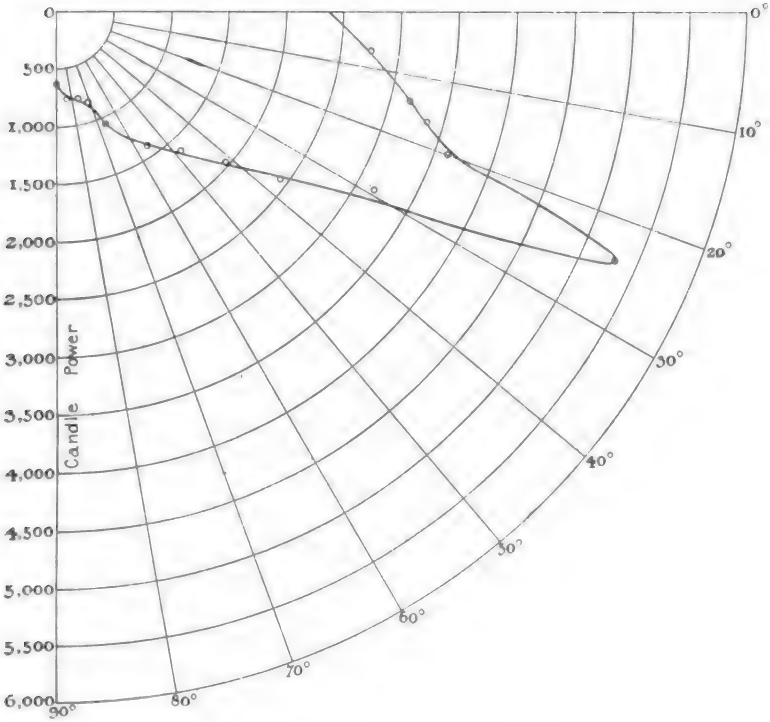


FIG. 18.—Polar Curve for 550-watt Lamp with Dioptric Inner Globe and the same Outer Globe as for Fig. 16 (*b*).

supply globes completely opalescent, sand-blasted, or acid-etched, and even globes so treated on only a portion of their surface; but no manufacturer would undertake to give a graded effect, at any rate on a diffracting globe, although a flashed opalescent globe with a graded effect on the lower portion was eventually obtained.

The experimental etching of globes was therefore undertaken. The process employed was very simple. The globes were clamped on to a special lead stand and filled to the desired height with the etching acid. The grading effect was then obtained by allowing the acid to

run out of the globe through an adjustable orifice in the lead stand, and any desired degree of frosting could be obtained by regulating the rate of flow.

Figs. 16, 17, and 18 show a variety of polar curves obtained with different combinations of globes, and it is obvious that Nos. 17 and 18 are very suitable for giving a fairly uniform illumination on a horizontal plane.

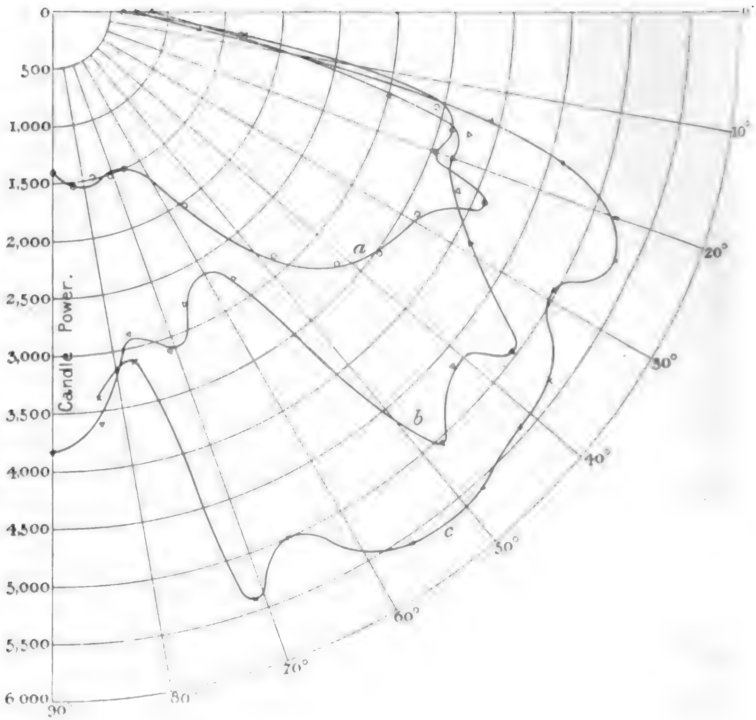


FIG. 19.—Polar Curves for 550-watt Lamp.

(*a*) With clear inner globe, partially flashed outer globe, and enamelled iron reflector.
(*b*) With clear inner globe, partially frosted outer globe, and enamelled iron reflector.
(*c*) With clear inner and outer globes, and enamelled iron reflector.

Experiments were also made in equipping the lamps with enamelled iron reflectors of various shapes, but it can hardly be said that the results were very successful, at any rate from the point of view of the distribution of the light. The effect is clearly indicated in the polar curves (Fig. 19). The most effective combination is that producing the polar curve (*a*) shown in Fig. 19. In this case the reflector was used in conjunction with an outer globe having the lower portion partially flashed.

Reflectors were really tried in order to remove the shadows under the lamps cast by the ash-trays; but for this purpose they were only partially successful, and the resulting distortion of the polar curve was very unsatisfactory.

One objection to the use of reflectors is the dark shadows cast above an angle of about 80 degrees to the vertical, as a result of which the blackness of the buildings and the darkness above the

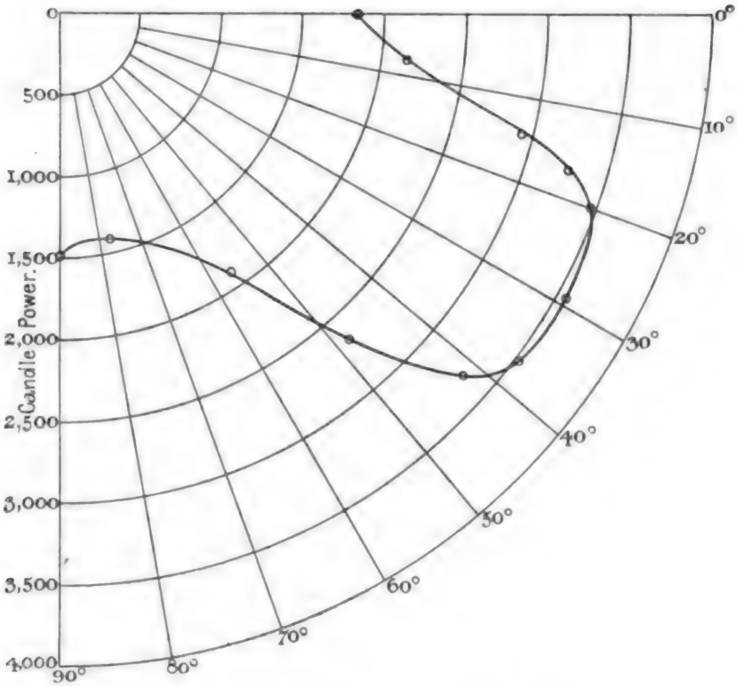


FIG. 20.—Polar Curve for 550-watt Lamp, fitted with Clear Inner and Outer Globes, as shown in Fig. 3 (full lines), and having a Graded Frosting on the Lower Portion of the Outer Globe. The frosting is densest at the bottom of the globe, and disappears at an angle of about 40 degrees below the horizontal.

lamps is very much intensified. This is particularly noticeable in foggy weather.

The "man in the street" probably bases his opinion of the respective merits of rival schemes of lighting upon the relative intensity and extent of the illumination. Consequently there is no doubt that it is a distinct advantage to illuminate as large an area of buildings as possible, provided always that it can be done without a reduction of the light flux in a more useful direction. If the elimination of shadows can therefore be effected by more legitimate means,

there is no advantage to be gained by the use of reflectors. It is very evident, from the results, that the most satisfactory curves of light distribution are obtained either with suitably frosted outer globes, or with dioptric inner globes used in conjunction with partially opalescent outer globes. There is very little to choose between the two methods on the score of light distribution. The dioptric globes undoubtedly have a slight advantage in this respect, but when it

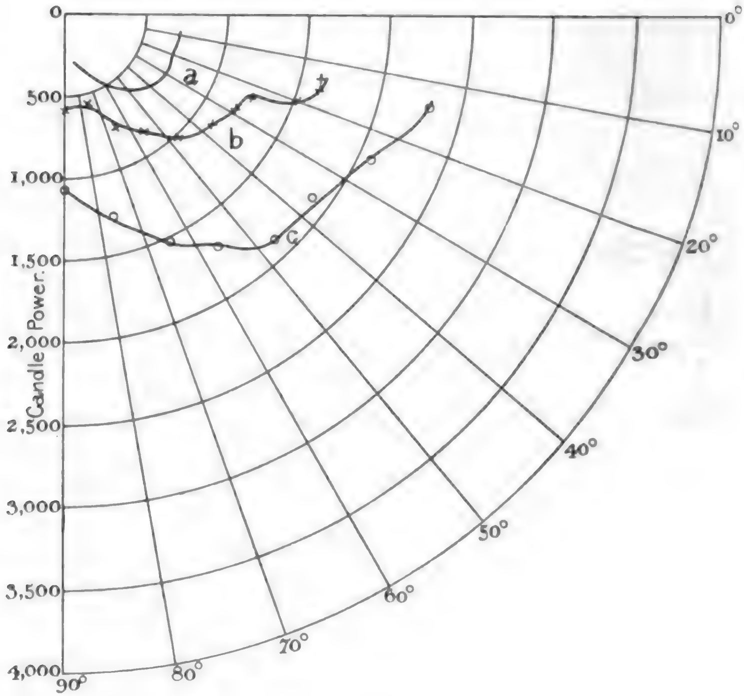


FIG. 21.—Polar Curves for Keith High-pressure Gas Lamps.
(*a*) Single-burner. (*b*) Twin-burner. (*c*) Three-burner.

comes to the consideration of general advantages and disadvantages, there is much to be said in favour of the partially frosted outer globes.

The advantages are :—

(*a*) Comparatively inexpensive to produce.

(*b*) The degree of frosting may be easily varied or graded as required.

(*c*) They have no optical centre, and consequently slight relative displacement of globe and arc is of no serious consequence. (This must not be confused with relative displacement of arc and economizer.)

Table III.

Description of Lamp.	Where Situated.	Height above ground level.	Maximum Candle-power. Average Results.	Illuminations on Horizontal Plane in Foot-candles. Maximum at Centre of Road.	Minimum at Centre of Road.	Minimum at Building-line.	Height of Measuring Plane.	Variation Factor. Centre of Road.	Variation Factor. Maximum.
550-watt flame arcs, original globes. Outer globe opalescent	Portland-street	27 ft. 6 in.	2,250	3·14	0·370	0·280	5 ft.	8·50	11·20
550-watt flame arcs. New type inner and outer globes. Experimental obscuration ...	Portland-street	27 ft. 6 in.	3,660	2·53	0·675	0·500	3 ft. 3 in.	3·75	5·06
550-watt flame arcs. Final graded frosting	Portland-street	27 ft. 6 in.	3,560	{ 2·50 2·00	0·670 0·670	0·500 0·500	3 ft. 3 in. Ground level	3·75 3·00	5·00 4·00
500-watt flame arcs. Opalescent outer globes	Albert-square	22 ft. 8 in.	1,910	4·94	0·150	—	5 ft.	32·90	—
550-watt flame arcs. Clear inner and outer globes...	Piccadilly	28 ft.	3,580	5·76	0·750	0·500	5 ft.	7·58	11·90
Keith 3-burner high-pressure gas	Princess-street	26 ft. 6 in.	2,300	2·23	0·575	0·400	3 ft. 3 in.	3·88	5·58
Keith single-burner, high-pressure gas	St. Peter's-square	17 ft.	725	(a) 2·45	0·180	0·120	5 ft.	13·6	20·4
Keith single-burner, high-pressure gas	Mosley-street	15 ft. 6 in.	725	(a) 3·00	0·070	0·055	5 ft.	43·00	—

(a) These values are the maxima, obtained approximately 5 ft. from the posts and not at centre of road.

(*d*) Intrinsic brilliancy of the light source is considerably reduced with a corresponding reduction of the glare effect, provided that the lamps are fixed sufficiently high.

(*e*) Shadows under the lamps are entirely eliminated, and the sharpness of the shadows cast by objects is toned down very considerably.

Compared with these advantages dioptric globes have the following disadvantages:—

(*f*) They are rather expensive.

(*g*) They have a very definite optical centre, and slight alteration in the relative position of arc and globe produces large changes in the light distribution.

(*h*) A completely uniform distribution of light does not appear to be obtainable without the use of special outer globes.

Conclusions from the Tests.

The numerous tests referred to in this paper have at least vindicated the lighting of city streets by means of flame arc lamps, not only on the dual basis of equal cost and illumination, but also on the ground of light distribution.

Unfortunately, owing to many unavoidable difficulties, the experiments with various forms of frosted globes have not yet been quite completed; nevertheless, the results so far have been very satisfactory, and further slight modifications will no doubt enable any reasonable alteration in the light distribution to be obtained. An obvious improvement in the case of the Piccadilly lighting will be the substitution of globes having a suitably graded frosting for the existing clear outer globes.

As there is a substantial difference between the cost of the flame arcs and the high-pressure gas lamps for the same minimum illumination, it would be possible to improve the arc lighting, if considered desirable, by reducing the distances between lamps in the case of future extensions. If fixed at the present height, and 100 ft. apart, the minimum illumination would be not less than 0·75 foot-candle.

No attempt has been made to obtain perfectly uniform illumination, since it is very doubtful whether such a scheme would be desirable, even if possible. A variation factor of 3·75 is not excessive, if the change is fairly gradual; and in this respect the partially frosted globes give results quite as favourable as the dioptric ones.

A well-known authority on street lighting has expressed the opinion that perfectly uniform lighting is flat and uninteresting. From a purely psychological point of view this is no doubt quite correct, but there is also probably a more definite physiological explanation. The visual conception of illumination is largely a matter of contrast, in connection with which fatigue of the eye plays a very important part.

It is, therefore, quite probable that the hollows in the illumination provide the rest necessary to enable the eye to appreciate the peaks, with the result that the average impression produced is superior to the corresponding effect due to a perfectly uniform system of lighting of equal average intensity.

APPENDIX A.

Manchester Corporation Electricity Department.

Statement of Cost of Supplies during the Year ended 31st March, 1912.

	Total.	Division. Running Costs.	Division. Fixed Costs.
	£	£	£
Generation costs ... ... Distribution costs ... ... Rents, rates, and taxes ... ... Management expenses ... ... Interest charges ... ... Depreciation account ... ... Reserve funds ... ... Rate aid	447,498	87,431	360,067
Deduct: Cost of traction supplies taken in accordance with the Model Form drawn up by the I.M.E.A. and M.T.A.	109,389	25,078	84,311
	£338,109	£62,353	£275,756

(*a*) Running costs : £62,353.

Number of units sold (other than traction), 64,466,224 = 0·232d. per unit.

(*b*) Fixed costs : £275,756, divided on following assumptions :—

(i) That the costs are due to the maximum demands of consumers.

(ii) That these (known only in total of 31,475 kw. from station records) are estimated to be made up thus :—

	kw.
70 per cent of lighting connections, 24,031 kw. =	16,821
The balance as power	14,654
	31,475

(iii) $\frac{16,821}{31,475}$ of £275,756 — Lighting = £147,371

(iv) $\frac{14,654}{31,475}$ of £275,756 — Power = 128,385

£275,756

(*c*) Lighting "fixed costs" spread over total kilowatts connected:—

$$\frac{£147,371}{24,031 \text{ kw.}} = £6{\cdot}133 \text{ per kw. connected.}$$

APPENDIX B.

Statement showing Cost of Current per Unit.

Cost of current = £6·133 per kw. connected, plus 0·232d. per unit metered.

(*a*) The all-night lamps run on a 45 per cent load-factor, or 4,015 actual burning hours per annum.

(*b*) The 11 o'clock lamps run on a 23 per cent load-factor, or 2,017 actual burning hours per annum.

Cost of current per unit in the case of (*a*) = 0·6d.

" " " (*b*) = 0·97d.

APPENDIX C.

Statement showing Cost per Annum of Lighting Portland-street.

Lamp watts = 550.
Carbon costs = 0·2d. per lamp per hour.
Trimming and maintenance charges = 0·05d. per lamp per hour.
Current consumption per annum (all-night lamps = 4,000 × 55/100 = 2,200 units + 11 o'clock lamps = 2,000 × 55/100 = 1,100 units) = 3,300 units.

	Cost per lamp per annum. £	s.	d.
All-night Lamps.			
Current, 2,200 × 0·6d.	5	10	0
Carbons, 4,000 × 0·2d.	3	6	8
Trimming and maintenance, 4,000 × 0·05d.	0	16	8
	£9	13	4
11 o'clock Lamps.			
Current, 1,100 × 0·974	4	9	0
Carbons, 2,000 × 0·2d.	1	13	4
Trimming and maintenance, 2,000 × 0·05d.	0	8	4
	£6	10	8

Summary.

	£	s	d.
Cost of 8 "all-night" lamps	77	6	8
„ 8 "11 o'clock" lamps	52	5	4
	£129	12	0

Capital expenditure on installation, £564.			
Capital charges per annum ($8\frac{1}{2}$ per cent on £564) ...	48	0	0
Annual acknowledgments for building attachments and use of tramway-poles	1	4	0
Annual cost of painting poles	3	6	6
Total	£182	2	6

Averaged over 16 lamps, the cost per lamp per annum equals £11 7s. 8d.

Capital spent on installation of the 16 arc lamps in Portland-street.

	£	s.	d.
Wages of men	88	18	5
Material, viz.—			
Arc lamps, poles, wire, carbons, hangers, etc.	342	0	4
Meters	4	17	6
Paid to Tramways Committee for erecting side poles, wall rosettes, and span wires	128	6	7
	£564	2	10
	or, say, £564.		

APPENDIX D.

STATEMENT PREPARED BY MR. ABADY SHOWING THE COST PER GAS LAMP (11 O'CLOCK) PER ANNUM.

Cost of gas uncompressed	12·69d. per 1,000 cubic feet
Cost of compression	1·2d. „ „ „
Cost of gas compressed	13·89d. „ „ „
Mantles	1s. $1\frac{1}{4}$d. each.

Wages—Proportionate part of time of one man (whole time at 28s.), and one man for 6 hours a week at 25s.: £1 7s. 2d.

Each lamp consumes 80 cub. ft. of gas per hour for 2,000 hours per annum.

One mantle lasts about 400 hours, therefore each 3-burner lamp takes 15 mantles per annum.

Lamps attended to and maintained at a wages cost stated above.

	Cost per lamp per annum. £	s.	d.
80 cub. ft. by 2,000 hours = 160,000 cub. ft. at 13·89d. per 1,000	9	5	2
Lighting, extinguishing, cleaning, and maintenance ...	1	7	2
15 mantles at 1s. 1½d. each	0	16	10½
	£11	9	2½

Equivalent to 1·37d. per hour lighted.

Nothing is included in the above for interest and depreciation on capital spent on installation, or for globes.

Capital spent on Installation of the Four Gas Lamps in Princess-street.

	£	s.	d.
Wages	12	6	6
Lamps	49	17	6
Poles...	33	0	0
Suspension gear and rosettes	26	16	10
Barrel and fittings	2	1	2
Tramway account for work done	70	9	4
	£194	11	4

Nothing is included in the above for high-pressure mains or compressor plant.

DISCUSSION.

Mr. Seabrook. Mr. A. H. SEABROOK: We have been very much disturbed in London during the last twelve or eighteen months on account of the progress made by our competitors in regard to street lighting. If we had had placed before us earlier a paper of this kind, with its valuable tests and records, I think the success attained by our competitors would not have been anything like so great as has been the case. There is not very much to criticize as far as the paper itself is concerned, because it is entirely a record, and the figures given are so conclusive and the tests obviously so impartial that there does not seem to be any possible doubt as to the accuracy of the deductions. I am entirely in agreement with the method of centrally-suspended lamps advocated by the authors. We have no room on the footway, what with tramway poles and other obstructions; and it is therefore really necessary that arc lamps should, where possible, be suspended from the adjoining premises. I do not quite understand the absence of lowering gear, and the authors do not appear to attach much importance to being able to lower the lamps without the use of tower wagons; I think it highly desirable, however, not only because of the saving in the cost of trimming, but also on account of the ease with which the lamps can be attended to during the dense street traffic in the evening. During the last two years we have changed the Oxford-street lamps

from tower-wagon trimming to lowering gear, and the cost of the lowering gear was practically wiped out in about eighteen months by the saving of labour, apart from the fact of being able to give proper attention to faults. The figure of 0·2d. mentioned by the authors for the price of carbons is interesting; until a year ago that was our cost for flame arc lamp carbons for Oxford-street. We then went in for a cheaper type of carbon, costing 0·16d., but the result was quite unsatisfactory and shows that economy in the case of street lighting is very often a false economy. This year we are going back to the dearer carbons at 0·2d. I am quite certain that no greater mistake can be made than to attempt economies of this kind, whether in carbons or anything else, in connection with street lighting, which affords one of the greatest advertising possibilities for the electric supply undertaking, and should be taken advantage of by giving the best light possible. I will ask the authors in their reply to give some information about the globes which appear to have been very successful after a lot of trouble and experiments: I should very much like to try some of these globes in our own district. I am rather diffident as to touching on the question of the calculated cost of current for street lighting on page 631, because I do not know whether 0·6d. per unit is intended to include profit, or whether it is the actual cost of supply for street lighting. In any case it is a very good covering figure, owing to the method by which it is obtained, particularly the method of arriving at the percentage of maximum load on the feeders for which the lighting load is responsible. Of the 31,475 kw. for lighting and power, the lighting load is assumed to be 16,821 kw.—obtained by taking 70 per cent of the lighting connections, which are 24,031 kw. That 70 per cent is a very high figure; 50 per cent or 45 per cent is nearer the mark. As I say, however, I should not like to place too much stress on that point, because I do not know whether the authors' figure is supposed to cover profit. It is interesting in a way, however, because although the figure of 0·9d. looks very low for half-night lighting and 0·6d. for all-night lighting, by taking the lighting maximum load on a more accurate basis (as I think) it means that anyone who is very hard pressed by gas for street lighting has, on the authors' basis, a very considerable margin for a still further cutting of price in case of necessity. I suppose there is a large amount of residence lighting in Manchester, a great proportion of which load comes on an hour and a half to two hours after the peak. If the authors reduce that figure of 16,800 kw. to something like 12,000 kw., it brings the cost down from £6 to possibly £4 or £5. Mr. Seabrook.

Mr. HAYDN T. HARRISON: Although such work as that described in the paper has been done before—for instance, in London by Mr. F. Bailey and his assistants—the results have not hitherto been published to the same extent. The point which interests me, and I suppose other members also, is that, in the particular case dealt with, high-pressure gas was successfully shown to be "out of court." We know that in the joint report to the Manchester Corporation submitted by Mr. Harrison.

Mr. Harrison.

Mr. Abady and myself, Mr. Abady was able to say that the high-pressure gas lamps were not up to the standard possible; that is to say, a better showing could have been made for high-pressure gas. I think it only fair to him to say that a better showing has been made (for instance, in Westminster) than was done at Manchester at the time referred to. The reason is that in Manchester a lower gas pressure is used than in London, and if the pressure had been raised to the same value as that in London the subsoil of Manchester would have been saturated with gas. The difference is represented by 27 candle-power per cubic foot of gas as against nearly 50 candle-power. The reason why the higher pressure and efficiency are obtainable in London is because here the same care and precautions have been taken by the gas authorities in dealing with the street-lighting problem that Mr. Pearce and Mr. Ratcliff took in providing street lighting at Manchester by means of electric lamps. It is essential to let the arc-lamp manufacturers know exactly the light distribution required. Many makers are quite content to send out lamps which work satisfactorily from a mechanical feed-point of view, and which give a very efficient light at certain angles unsuitable for street illumination. To install such lamps in an indiscriminate way does immense harm to the progress of electricity for street lighting. I have a case in mind where a circular was distributed by one of the many associations connected with the electrical industry, and in this circular it was affirmed that anything from 2,500 up to 5,000 candle-power could be obtained with the flame arc lamp. But when an example is required, it is necessary to admit that only from 1,300 to 1,700 useful candle-power is obtained. Fortunately this is a point which the Institution has carefully considered, and which will soon be dealt with in the forthcoming standard specification for street lighting. Many members may have seen the figures recently published by Dr. Bloch—one of the greatest authorities in Germany on this particular class of work. Dr. Bloch states in his book that with high-pressure gas lighting it is possible at the present time to obtain 25·5 to 28·5 candle-power per cubic foot in a horizontal direction, 18 to 21·5 mean spherical candle-power per cubic foot, and 16 to 18 mean hemispherical candle-power per cubic foot. Many of us are told that we have to compete with 50 candle-power per cubic foot for high-pressure gas, but Dr. Bloch and the authors of this paper have shown that that is not usually the case. The effective useful candle-power for street lighting does not generally exceed 30 candle-power per cubic foot. Mr. Abady and myself, using different types of photometers, found a mean result of 27 candle-power per cubic foot, which closely agrees with those in Dr. Bloch's work in Germany that has been published since our report. Thus his independent figures not only corroborate our own, but show that photometric measurements even in the street are not by any means unreliable. With regard to the minimum horizontal illumination mentioned, the authors give a minimum of 0·5 foot-candle. That is a high minimum, and it cannot be held to apply except in towns of such importance as Manchester. The buildings in that city are practically

black, and there is nothing in any way to assist the street lighting. At one particular point in the two streets referred to in the paper there is a house which is a little lighter in colour than the others; originally it was white, when I was there it was grey, and now probably it is black. At the time of the test everybody who looked at the lamp adjacent to that house asserted that it was the best lamp of the lot; and so it appeared to be. That shows that if we have to depend on the opinions of others, as so many engineers have to do, we shall be told by those inspecting the work something similar in effect, namely, that "the best lamp is nearest to the white house." I ought to say, once and for all, that it is essential to have recourse to photometry. There are occasions when we are told that photometry does not matter; but if the lamp nearest the white house were removed to the other end of the road, those who had previously affirmed its superiority would then say it was the worst lamp. As regards intrinsic brilliancy, when comparing high-pressure gas lamps and arc lamps, many people say that the effect of an arc lamp on the eyes is not so pleasant as the effect of the high-pressure gas lamp. Our competitors claim that this ill-effect is due to the intrinsic brilliancy of the arc lamp. I have calculated it roughly by estimating the candle-power per square inch or per square centimetre of the light source. In the case of the high-pressure gas this will be something like 3,000 candle-power for 9 square inches. This is certainly a much higher intrinsic brilliancy than in the average arc lamp, having a globe which absorbs about 22 to 25 per cent of light; in which case electric lighting would have the advantage from the point of view of intrinsic brilliancy. The cost appears at Manchester to be greatly in favour of electric light. I notice that on page 614 of the paper the authors give figures which are made strictly comparable by assuming that the Portland-street lamps are all switched out at 11 p.m.; in connection with that, I would ask whether anyone can say why the lighting of the streets should be reduced at 11 p.m.? If it be a question of traffic, this in London certainly exists until midnight; and the traffic then diminishing, people say the amount of light may be reduced. Policemen in London state that robberies, etc., are mainly committed between the hours of 3 and 5 a.m., when the lighting is reduced. In Manchester the lamps are turned out at 11 p.m., and, presumably, the police can do their work after that hour with about one-eighth of the ordinary illumination. I hope we shall one day be able to show the municipal authorities that the practice of thus reducing the light is unwise. In which case longer hours of lighting will further reduce the cost of electric lighting. Mr. Harrison.

Professor A. SCHWARTZ: One of the first difficulties met with in designing a photometer for street purposes is the very large range of illumination which the instrument has to measure. On the one hand, it has to deal with an illumination of, say, 6 foot-candles, and, on the other hand, it has perhaps to read down to an illumination of one-tenth of a foot-candle. In order to reduce the light entering the photometer I understand the authors have employed a slit, the jaws of which Professor Schwartz.

Professor Schwartz.

are connected by a micrometer screw. I should like to ask them whether they have made any tests to find out whether there is any backlash present. With a large aperture in the slit, a small amount (such as one-hundredth of an inch) of backlash would cause little or no error; but when working with a narrow slit, the error would be considerable. Last year, two of my former students, Messrs. Gwyther and Taylor, designed and made a photometer for street work, in which they got over this difficulty of the slit and the micrometer screw by means of a disc containing four or five circular apertures. These apertures were covered with photographic films of different degrees of density. The films were carefully calibrated, and were placed in turn in front of the orifice of the photometer, thus reducing the amount of light which came in, while the range of illumination with which they were able to deal varied from $\frac{1}{100}$ foot-candle up to 12 foot-candles. The authors have referred to deviations from the law of inverse squares; the inverse square law applies strictly only when the source of light is a point, and the matter is of some importance in the calibration of the photometer, especially when a standard such as the Fleming-Ediswan with a long filament is used for calibration purposes. In the case of a source of light in the form of a straight filament, if it is desired to keep within an error of 2 per cent, the distance from the photometer head to the standard must not be less than six times the length of the filament. This deviation from the law of inverse squares also applies, of course, to the use of the sub-standard, in the photometer itself. Another possible source of error within the photometer, which I think might well be investigated, is the amount of reflection obtained with a dead black paint. I am sure the authors will agree with me that the old Royal Infirmary of Manchester is remarkable in the way of blackness, and yet good photographs of that building are extant showing that a considerable amount of reflection must have taken place from its surface. I think that a certain amount of reflection also results from the dull black paint inside the photometer; Mr. Weston Taylor's experiments indicate the presence of a distinct disturbance increasing as the distance of the sub-standard from the photometer head was increased. On page 605 of the paper reference is made to a proposed international standard height for photometric observations, viz. a plane one metre above the street surface, to which illumination measurements should be referred. I agree with the authors in thinking that there is no real reason why this height should have been selected. It is certainly a very inconvenient height for making measurements with certain types of photometers, and I further agree with the authors' view that a height of 1½ metres (about 4 ft. 10½ in.) would be a much more convenient plane at which to take measurements, as it represents approximately the average height of the eye. If any reduction by calculation is made, it should be to the ground level. Messrs. Taylor and Gwyther's observations in Portland-street and Princess-street were taken on a plane 5 ft. above the street surface. Their results are slightly higher than those of the authors, both for gas lamps and arc lamps, but they

confirm the general relation between the two illuminants found by the authors, also the great variation in the candle-power of the high-pressure gas lamps. In Mosley-street, for instance, the high-pressure gas lamps gave maximum illuminations of 0·5, 2·55, 3·4, 4·55, and 6·5 foot-candles from four lamps respectively. The authors have referred to the question of glare. In average cases glare is caused by light between the angles of 60 deg. and 80 deg. with the vertical. I think the authors are to be congratulated on their diffusing arrangements and the absence of glare, which is very cheaply purchased by the loss of a little light. They have told us that the curves and tables given in the paper are the result of a large number of experiments, and are average or mean values. I think it would be useful if we could come to some agreement as to the method of expressing the average value for the illumination of a street. We might, for instance, get the mean polar curve of half a dozen lamps and then assume two lamps whose height and distance apart were the average height and distance apart of all the lamps in the street. We could then work out from the polar curves the illumination curves for these two lamps, which would form characteristic curves for the illumination in that street. Professor Schwartz.

Mr. F. BAILEY: In an enterprising city of the size of Manchester, it seems to me rather extraordinary that there is sufficient smoke left to incrustate the buildings with black, instead of factory chimneys having been replaced by electric motors. I think that the time has arrived when the Corporation of Manchester should seriously consider the initiation of street cleaning, and they should start their improvements by cleaning the buildings. They could then obtain the 0·5 foot-candle referred to in the paper with the expenditure of less electrical energy than at present. I sympathize with the authors, but I cannot help feeling slightly critical. The point about which I have difficulty is this: Why should all these disadvantages and obstacles that have been encountered occur in the streets? Knowing what they had to deal with, would it not have been better to have tested the lamps privately, and then to have erected them? The paper rather gives the impression that there is something unknown about arc lamps and flame arc lighting, and that there are dangers, difficulties, and disadvantages, which it requires expert assistance to overcome. This, however, is not really the case. I disagree with the suggestion that we have not published our results, but of course we have not published long histories of disappointments and failures, as this course would have been unsatisfactory to everyone. It seems a pity that these erroneous impressions about public lighting should get about. With regard to grading the opalescence of globes, what we are all trying to avoid is interference in any way with the skin of a globe, so as to preserve it as long as possible in the state in which we receive it from the makers. Without proper attention it is difficult to keep a globe in its original condition for more than a few days, and to prevent the vapours from attacking the glass. We do not want to start by spoiling our globes by exposing them to the additional risks of Mr. Bailey.

Mr. Bailey. dirt and vapours. The authors give a tabulated statement setting forth the advantages and disadvantages of dioptric globes. We have had some experience with these globes, and while I do not know what type of globe the authors experimented with, or the methods they have adopted, the results shown in some of the curves are very curious. It seems to me that something must have been wrong either with the type of dioptric globe or with the method of application. I should be sorry to think that this type, whatever it is, is likely to be used indefinitely in this particular form. The dioptric globe, naturally, requires very careful handling, and its chief functions have not been mentioned in the paper. The dioptric globe, such as I should like to see, is that which is the outcome of the work of Mr. Trotter (who, by the way, does not often get the credit for his suggestions and initiations). Mr. Trotter's work is the foundation of the design worked out by Mr. O. L. Peard, to whom we are so much indebted for our practical success. It is a very difficult matter to try and combine the best theoretical shape with the practical form in which it can be made. We had a number of interviews with the glass manufacturers when endeavouring to apply a theoretical design which could be worked out in practice, and as they did not care to undertake the expense and risk of making the necessary mould, we had to set to work on a new problem and make the mould ourselves. With skilful treatment at the makers' hands the globes are a practical success; they are clean, free from blow-holes or bubbles, and stand heat remarkably well. The essential feature of the design is to focus the rays of the arc in a particular direction, but there are other important functions to bear in mind. Thus, we had to consider the angle at which the lamp could be seen from a distance, and to arrange for the ash from the carbons to be deposited in a receptacle and not left to collect on the glass itself, as this would not only have obscured the light, but would probably have led to the glass being damaged by subsequent cleaning. The most important function of a properly designed globe is to secure a steady arc. It is difficult, in fact, to believe the remark of the authors that the gas lamp is steadier than the arc lamp. It seems incomprehensible, in 1913, to admit that an arc lamp cannot be made to burn as steadily as a gas lamp: I strongly recommend the authors to reconsider this matter, and see whether by the adoption of dioptric globes, in the proper manner, this difficulty cannot be overcome. With regard to tower wagons and the central-suspension system, I have had a good many years' experience of the latter, and it has been my lot to deal with a difficult case where a number of arc lamps are permanently fixed without lowering gear. The trimmer did not find his work by any means easy. There can be no question that the practical advantages of a lowering gear are very great. We have, during the last seven or eight years, carried out a considerable amount of testing, and have given the makers the benefits of any tests we have made. Not only so, but we have done our best to make the process of lighting the lamps a simple operation.

Mr. K. EDGCUMBE: I think that all who share the opinion that horizontal illumination will eventually be accepted as the basis for comparing results in street lighting will welcome the way in which the authors of this paper seem to take it as a matter of course that the figure for the horizontal illumination is the one to be given. Professor Schwarz has raised the question of the metre height. There is rather more in this question than merely taking a convenient unit of length, as the authors suggest. A year or two ago the Germans, through the Verband Deutscher Elektrotechniker, authorized the adoption of the metre as the height for horizontal illumination measurements in the street, which has led to the suggested adoption of the metre in this country. For a long time 3 or 4 ft. has been taken as the most convenient height, and it therefore seems reasonable to adopt 3 ft. 3 in., so that the result of measurements in the two countries may be really comparable. Fortunately the lux, which is the German unit of illumination, is nearly $\frac{1}{10}$ of a British foot-candle. If only we could induce the Germans to accept the "candle-power" now adopted in this country, France, and America, the two units would be still nearer, as the difference would then only be $7\frac{1}{2}$ per cent. I am sorry that the authors do not say more definitely what they mean by "horizontal illumination." They give a definition on page 605 of the paper to the effect that "horizontal illumination is the vertical component of the light flux density on any plane normal to the incident ray." This is perfectly correct, but it seems involved, and the authors rather spoil it, to my mind, by their next remark, namely, ". . . it is advantageous to obtain this value by direct calculation, without any reference to the particular nature of the ground surface." I am sure the authors do not mean to suggest that if the ground is light in colour the "horizontal illumination" will be higher, or vice versa; the illumination in foot-candles has nothing to do with the "ground surface." It is true that the *surface brightness* would be greater, but that is due to what Mr. Paterson calls the "co-efficient of reflection," and depends upon the nature of the surface. I do not wish to labour the point, but it has so often been contended that the illumination is affected by the nature of the surface that it seems desirable, in a classic paper of this kind, to have the matter explained clearly. I note that the authors suggest that the horizontal illumination should be calculated from candle-power measurements. That, I suppose, is partly due to the fact that the photometer they were using was a candle-power photometer pure and simple, and they had, therefore, no alternative; but I think that this does away with one argument in favour of the horizontal illumination basis compared with the candle-power basis. One of the main advantages is that since it is illumination and not candle-power which is being purchased, it is better to leave the contractor as free a hand as possible as to how he obtains his result. I think that the remark of the authors, on page 612 of the paper, as to the unsatisfactory nature of Portland-street from the illumination point of view, distinctly shows that we should not merely measure the candle-power and calculate what we think the horizontal illumination

Mr. Edgcumbe.

Mr. Edgcumbe.

ought to be, but that we should actually measure it. It seems to me that the figures given in the paper do not represent the actual illumination in Portland-street, but rather what that horizontal illumination might be supposed to be if the whole installation were removed to some impossible situation where there was no possible reflection from houses, trees, etc. As, however, the installation is in a street the reflection from the houses, if it exists, should, it seems to me, be taken into account, and actual measured values of horizontal illumination given in place of calculated values. The authors do not appear to be exactly enthusiastic about the flicker photometer; they may mean to be, but, if so, I cannot share that enthusiasm. For street lighting I have never been in favour of the flicker photometer. They tell us about the physiological troubles and the severe eye-strain, but do not allude to the well-known difficulties which arise when using the flicker photometer for low illuminations. The flicker photometer, moreover, corresponds very much to the telephone receiver when used with a Wheatstone bridge; one can hear the telephone buzzing, but there is no indication as to the direction in which the adjustment has to be made. With the flicker photometer there is a similar uncertainty, and that is a serious disadvantage. The equality of brightness photometer, on the other hand, corresponds with the central zero galvanometers in this respect.

Some years ago I suggested, in one of the technical journals, that it might be interesting to calculate a figure for the average illumination in foot-candles, as obtained from curves such as those in Figs. 6 and 7, and see how that worked out in £ s. d. per yard of street. It is extremely difficult by any other method, as far as I can see, to get a comparative figure for the two installations described in the paper. The arc lamp illumination may be either better, worse, or equal to the gas. We know what the two cost, but it is difficult to arrive at a really comparable figure. Referring to the curves of horizontal illumination on page 609 of the paper, if we take the average illumination over the length between two lamps, and multiply it by the distance between them, we should have a figure of so many "foot-candle yards," say. In fact the idea is much the same as that involved in calculating horse-power by multiplying the length of stroke by the mean pressure. Such a figure, compared with the cost per lamp per hour, or per 1,000 hours, would give a constant, from which the value of the lighting installation could be deduced, and a direct comparison made. Roughly, I make out from the curves in Figs. 6 and 7 that each gas lamp gives about 47 "foot-candle yards" and each electric light about 62 "foot-candle yards." From the figures in the appendixes I gather that the cost per lamp of the gas lighting is about £5 15s. for 1,000 hours, and of the electric lighting £3 5s. for 1,000 hours. This would come to 2s. 6d. in the case of gas, and 1s. in the case of electricity, per "foot-candle yard" for each 1,000 hours of lighting. This proposal, of course, only deals with the illumination along the centre of the street, but so long as the breadths of the two streets are not widely different the comparison

seems perfectly fair, and this is borne out by comparing Figs. 8 and 9. Mr. Edgcumbe.

Professor J. T. MORRIS: The authors state that the detrimental effect of a foggy or heavily smoke-laden atmosphere on the mantles results in a serious diminution of candle-power just at a time when it is most required. It would be of interest to know to what this effect is due, and what is its magnitude. As to the efficiency of the high-pressure gas lamps installed in Manchester, it is to be regretted that these lamps were certainly not working at their highest efficiency. Many people have stated, among them Mr. Haydn Harrison, as a result of tests made, that high-pressure gas lamps do not, as a rule, give the guaranteed candle-power. I am specially interested in this matter, having myself also tested some of these high-pressure gas lamps, and having obtained figures which are practically corroborated by the tests given in the paper. As the results of some of my tests have been called in question, I am glad to have the independent support afforded by these tests. Table I is particularly interesting, for here we have a lamp rated at 4,500 c.p. giving a maximum of only 2,300 c.p., and so on all the way down the columns, the actual maximum being only about half the rated candle-power. It is exceedingly important to our gas competitors to see that the lamps they supply are installed in such a way that the candle-power obtained from the lamps is that at which they are rated. As far as I can see it seems much more difficult to get the rated candle-power from gas lamps than from arc lamps; but that is a matter for the high-pressure gas experts. What I want to see is fair, open competition, and no doubt we all desire the same. On page 619 of the paper there occurs a statement, distinct from that to which I have previously referred, about relative penetration in a foggy atmosphere. I take it that the authors are convinced that the electric flame arc is superior in its penetrating power to the high-pressure gas lamp in foggy weather. Have they measurements by which they can corroborate that? If so, they would be most valuable. The statement has often been made that high-pressure gas has a much greater penetrative power in fogs than the electric arc, and I have known cases where high-pressure gas has been installed merely because that statement has been believed. Figures supporting the authors' contention would therefore be most valuable. Professor Morris.

I have one or two questions with regard to photometry. In Table II the authors give the size of type which can be read distinctly at given distances; in connection with this I should like to ask if the test card (Fig. 4) was placed horizontally or at right angles to the beam from the lamp. Also, could the authors give figures of illumination which would compare with those distances, so that the following point could be tested? Whether the ease with which these types are read is simply a question of illumination, or whether there is something intrinsic in one or other of the sources of light which causes a difference? I have expressed, in the form of a short table, the percentage cost of the various items which go to make up the total expenditure in each lamp (gas or

Professor Morris.

electric), with the idea of showing in which direction it would be most profitable to look for improvements :—

	High-pressure Gas.	Electric Flame Arc.
	Per cent.	Per cent.
Gas or electrical energy	80	60–65
Trimming and maintenance ...	13	7–8
Mantles or carbons	7*	28–30

Mr. Dow.

Mr. J. S. Dow: One of the most interesting points in the paper, to my mind, is the effort which has been made to improve the distribution curve of the arc by graded globes. I am pleased to see that the authors lay stress on the avoidance of glare; but it rather seems to me that the method adopted of obscuring these globes is not quite what is needed to get rid of the worst variety of glare. The experience of most people is that the rays which cause most trouble from a street lamp are those that come at small angles to the horizontal from fairly distant lamps falling within the direct range of vision. Yet the authors seem to have used the heaviest obscuring on the *lower* parts of the globe, thus softening mainly the rays that are comparatively innocuous. It must be admitted that the rays slightly below the horizontal are just those that one requires to be very intense in order to secure uniform illumination. One is thus in a dilemma—if one seeks to reduce glare one is apt to diminish the illumination midway between the lamps. The authors raise the question of even illumination. People sometimes argue that daylight is uneven; but daylight is so very much brighter than artificial light that we can have enormous variations in illumination without being aware of them. But when we are badly in need of more light, as in street lighting, and when each point in the street requires illumination to an equal degree, uniform illumination is the proper thing at which to aim. The next question is how to get the ideal curve of illumination to secure uniformity, and yet avoid glare. The curve given by the authors which comes nearest to the ideal for uniformity would seem to be that shown in Fig. 18. On the other hand, on examining the curves in Fig. 19 it is noticeable that curve *a* (which is recommended) appears to be the best for giving even illumination; but that type of curve is only obtained by absorbing a great deal of the light given off at other angles. The other curves, *b*, *c*, may not give the right distribution for street lighting; but this is because they give so much more light than *a*. It was formerly argued that lamps with upright carbons gave a better shape of curve

* Unfortunately this percentage for the mantles is too low because the wear and tear of mantles is known to be much greater if the lamps are giving their full rated candle-power.

for street lighting than flame arcs; but they did so merely because the lower carbon got in the way and obstructed a great deal of light. What one should do is to get the ideal curve, not by merely absorbing the light in certain directions, but by redirecting it. I question whether by any form of purely diffusing globe one could achieve much in this respect. Translucent diffusing methods rarely direct the light to any great extent; it is usually found that they tend to smooth out irregularities and to make the curve spherical. The ideal curve might perhaps be got with comparative ease in the case of a metal filament lamp, because all the light which otherwise would pass upwards can be utilized. One investigator—Sweet—in the United States did obtain a close approximation to the ideal curve, coupled with very good conditions as regards avoidance of glare. He used a combination of prismatic glass reflector and opal globe, but I understand that his method was not practical in other respects. In the flame arc the source is largely "bottled up" in the economizer, and much of the light is inevitably directed downwards. Turning to photometric questions, one would recommend adhering to the 3 ft. 3 in. horizontal plane for illumination measurements, because it is general on the Continent, and recognized by the Committee of the Verband Deutscher Elektrotechniker; moreover, this measurement is the one most employed in indoor work, because the metre is not far from the height at which illumination is mainly required—about the height of a table in most cases. The authors have also raised the old bogey about the "Purkinje effect." From work recently described by Ives in the *Philosophical Magazine*, it appears that this problem is in a fair way to be settled. But it may be suggested that, at the degree of illumination the authors mentioned, these physiological colour-effects need not be feared. I have found that even with red and green light it is necessary to get a lower illumination than one-tenth of a foot-candle before any very serious difference becomes apparent, while with ordinary illuminants the effect would be still less marked. But the method of removing test type from a lamp until the type just cannot be read is perhaps more open to objection from this standpoint than any other. This system, and also so-called "extinction" methods, are apt to lead to very weak illuminations being employed, and at such illuminations the eye is in an entirely abnormal state. Considerable confusion has been caused by the publication of experiments in which the effect of this circumstance was not realized. Mr. Dow.

Mr. A. J. CRIDGE: Reference is made in the paper to the physiological effect of light. It has occurred to me that elderly people are possibly more sensitive to light of short wave-length, and so prefer the slightly greenish light of incandescent gas, while younger people like the yellow light of the flame arc, which is of longer wave-length. I should like to ask the authors if their experience bears this out, for I remember that when the question of the street lighting of Sheffield was being settled, the Chief Constable, who was responsible to the Watch Committee for the lighting of the streets, and who was, of course, a Mr. Cridge.

Mr. Cridge. considerably older man than myself, was decidedly in favour of gas. I preferred the flame arc lamp.

Mr. Trotter. Mr. A. P. TROTTER (*communicated*): This paper is a record of the result of the instruction of the City Council of Manchester to its Gas and Electricity Committees to take two streets and to do their best. The first effort of the Gas Committee in Mosley-street was so much less brilliant than the electric lighting in Portland-street that a short part of Princess-street abutting directly on Portland-street was chosen for a more strenuous production. A critic can stand at the junction of these streets and compare the various features presented by the two illuminants. This I have done on several occasions, and on the last with Messrs. Pearce and Ratcliffe's paper in my hand. In doing their best the two rivals seem to have had in view copious illumination, uniformity of distribution, and economy. If these were their objects they have both succeeded admirably. The difference between the illumination of the two streets and between the uniformity of distribution is imperceptible without a photometer, and the price is in favour of electricity. Where an illumination (on a horizontal plane 3 ft. 3 in. from the ground) of half a foot-candle with a ratio of maximum to minimum of $3\frac{3}{4}$ is wanted, this is an excellent way of doing it. The Corporation are well able to judge for themselves to what extent this degree of lighting is suitable for Manchester. Until this experiment was made, Manchester was, and in most of its other streets still is, the worst lighted of our great cities. There are probably three reasons. First, the gas supply and the street lighting were in the hands of the Gas Committee. It is hardly credible that they should have exacted from the ratepayers the same price for gas for public street lighting as for ordinary householders. The second is that there was no competitive inducement for any improvement. The third may be an excuse as well as a reason. It is said that all the traffic in Manchester goes down one street, and that grass grows in the rest. The distribution of vehicular traffic in that city is certainly surprising to a stranger in the daytime, but the scarcity of any traffic at all, except tramways, after 7 p.m. is even more remarkable. I am not acquainted with the traffic in these streets before 6.30 p.m.

Other street-lighting authorities who may examine the results described in this paper should not assume that a capital expenditure of £1,500 per mile of street, or £250 per mile per annum, is necessary for streets having the traffic of Princess-street, or even of Portland-street after 7 p.m. It is to be hoped that local authorities will begin to rival each other in street lighting as they did in tramway development. But the zeal with which each councillor claimed tramways for his ward, whether it wanted it or not, and the jealousy with which one town went in for larger and more gaudy bogie-cars than their neighbours, was not altogether good for the tramway industry. The grading of the lighting to suit the requirements of the traffic in different streets, and the hours during which that traffic exists, are necessary factors in economy, unless a spectacular display is desirable for the purpose of

making a town look attractive and gay. He would be a bold man who would try that with Manchester. It is right that we electrical engineers should try to increase the amount of light in our streets, and our gas friends will be not unwilling to co-operate with us; but it is not to the true interest of electrical engineering to encourage extravagance. Local authorities and their engineers and surveyors must be educated in illumination as a branch of engineering. It is perfectly useless to tell such people that light is a vector quantity and that illumination is the equivalent of impressed light flux density multiplied by the cosine of the angle of incidence. It is difficult enough to make them understand that a surface facing a light of one candle-power, and at one foot from it, receives a unit illumination of one foot-candle. It is very desirable that municipal engineers should realize that illumination can be easily, quickly, and accurately measured. The complicated, tedious, cumbersome, and fatiguing photometer (which I have seen) used by the authors, to say nothing of the laborious calculations to be made afterwards, is enough to discourage measurements altogether. Those who have made such calculations recognize the heavy work which the authors have expended on this paper. The whole of the illumination measurements might have been made with one of four or five direct-reading instruments either at 3 ft. 3 in. or actually on the ground with no calculations whatever. In street lighting one need not worry about the Purkinje effect; it does not begin appreciably to alter the relative brightness of red and green until the illumination has been reduced to about 0·025 foot-candle. But red and green lights are not used for street work, and the Purkinje effect may be left to laboratory investigations. Mr. Trotter.

The authors say that their tests demonstrate a rather objectionable feature of dioptric globes, namely, the strongly defined optical centre. If any maker of dioptric shades claims that his shades have no optical centre, and his claim is found to be untenable, some objection might be raised. But a dioptric shade generally means one which by the use of calculated planes and curves so distributes the rays of light that a certain predetermined effect is produced. It is necessary to assume an initial distribution within the shade, and this implies a focus or optical centre. The difficulty is that the inventors are generally helpless, while makers or their agents merely want to sell the goods and do not insist on the proper setting of the shades, and purchasers do not take the trouble to find out if there is any necessity for setting and, if so, how to set. To say that a dioptric shade has a strongly defined optical centre is to admit that it is, and to give it the credit for being, intelligently designed and made for some purpose or other. I have seen offered for street lighting dioptric shades in which no intelligence seems to have been used in the design, and I have seen well-designed shades so unintelligently used as to defeat the intended effect altogether. With a fixed focus arc there should be no difficulty in setting the shade as the inventor or designer intended. In 1879 I thought that uniform illumination was desirable in street

Mr. Trotter. lighting. I produced a dioptric shade intended to illuminate uniformly a circle having a diameter seven times the height of the lamp. Fixed focus arcs were unknown, and my lantern was 2 ft. 6 in. in diameter. The intention of the large size was to reduce the effect of the alteration in the position of the arc. I have long ago changed my opinion about the value of uniform illumination for street work. I consider a street is more usefully lighted when the ratio of maximum to minimum illumination is 15, or even greater. When half the lights are switched off in Portland-street after 11 p.m. the street is excellently lighted. The graded frosting of the outer globes in Portland-street no doubt contributes largely to the uniformity of the lighting which has been achieved. It is a simple and effective device, but it is like regulating the speed of a steam engine by a brake instead of by the steam admission. It is a dioptric method of the crudest kind, and even this requires some setting; it could not be applied on a small globe to an old-fashioned arc lamp. After examining these excellent examples of lighting on several occasions, I find that, so far as the eye can judge, the uniformity of illumination in Princess-street is as good as that in Portland-street, and is secured without any partial obscuration. There are two good features in the gas-lighted Princess-street which are well known to electrical engineers, but which have not been adopted in Portland-street. The gas lamps are surrounded by large clear globes. The large size may be necessary on account of the intense heat, but they look well. The small globes in Portland-street have a meagre appearance compared with these. Small globes are good enough for railway goods yards, docks, etc., where appearance is of little consequence, but a few shillings are well spent on large globes for first-class street lighting, especially when hung so high. While the maximum candle-powers are about the same, the glare, or the distress to the eye in looking straight at the arcs, is greater in my opinion than with the gas lamps. This was not to be expected; for though the mantles themselves, which have a high intrinsic brightness, are visible through the clear glass, the globes of the arcs seem to be of a practically uniform brightness, and of a larger area than that of the group of gas mantles. The lesser glare of the gas seems to be due to the use of a white reflector. Glare was the subject of a paper read before the Illuminating Engineering Society by Dr. J. H. Parsons in 1910, and was followed by a long discussion on two evenings, and by communications from many writers on the Continent. The definitions of glare were bewildering in diversity, and the explanations were complicated, but one practical fact was brought out by Mr. W. R. Cooper and by Mr. Haydn Harrison. "A lamp against a black background gives a greater sense of glare than the same lamp against a white wall." The small arc-lamp globes in Portland-street are seen against a dark sky or the even blacker buildings. If large white reflectors were used the painful glare would be reduced. The reflectors would be of little use in the sense of reflectors, for very little light is sent upwards. No mathematics are wanted in designing them. The two conditions are

that they should be illuminated, and that they should present as large a surface as possible to the eye. If slightly conical, with the base downwards, they would be more brightly illuminated, but would cast shadows on the buildings and would not present so much surface to the eye. With the base upwards more surface would be visible, but the illumination would be less. A flat disc of white enamelled iron would probably be as good as anything; the only limit to the diameter would be the tendency to swing in the wind. The lighting in the important thoroughfare of Piccadilly is excellent, and that in Albert-square is worthy of its situation and enables the drivers on the taxi-cab stands to read their newspapers with ease. Mr. Trotter.

Mr. W. R. Cooper (*communicated*): I should like to support the authors in questioning the desirability of measuring the horizontal illumination of streets on a plane 3 ft. 3 in. above the ground level. The only reason for adopting this particular height seems to be that it is practically equivalent to one metre; but I know of no reason why one metre should be particularly favoured. The authors also take a height of 5 ft., which is rather inconsistent. A pedestrian in a street requires a certain horizontal illumination *at the street surface* so that he may avoid obstructions. Illumination is also required at higher levels, for seeing door numbers or vehicular traffic, but it is the vertical illumination that is then useful. It would be much more rational, therefore, to reduce all measurements of horizontal illumination to the street surface. The measurement of vertical illumination may give useful information, but this suffers from the inherent defect that generally it can only be measured so as to include light from one direction. Another point to be borne in mind is that if a given height, such as 3 ft. 3 in., is taken, this has a greater influence upon results with short poles than with high poles. At a considerable distance from a pole the horizontal illumination varies approximately as the height of the pole; consequently the apparent minimum is reduced as the height of the plane of reference is increased. This reduction will have a greater effect with short than with high poles, and will render the results not truly comparable. For example, the difference between taking mid-illumination with 20-ft. poles spaced 150 ft. apart, (1) at street level and (2) on a plane 5 ft. above the ground, assuming that the candle-power is the same in the two directions involved, is a diminution of 20 per cent by the use of the plane. On the other hand, immediately below the lamps the reference plane gives an increase of nearly 80 per cent. Thus the ratio of maximum to minimum illumination becomes much exaggerated, and this to a variable extent depending upon the height of the plane and the height of the poles. I do not quite understand why no difference is apparent at the mid-point of the authors' diagrams. For indoor measurements there is something in favour of a height of, say, 2 ft. 6 in., the height of an ordinary table. Mr. Cooper.

From the point of view of using clear globes, the shape of the economizer shown in Fig. 3 is not good. I should like to know if any attempt was made to alter this. Presumably its position in relation to

Mr. Cooper. the arc was not changed. It is noticeable that the illumination curves in Fig. 7 are very smooth. As there was apparently a good deal of variation of illumination it would be interesting to know how long was required to obtain an average result. I scarcely think the Purkinje effect can cause any error in the illumination measured. It does not seem to play a part unless the illumination is as low as $\frac{1}{10}$ foot-candle, and Mr. J. S. Dow has given figures indicating an even lower limit. Table II gives a comparison of penetrating power by a reading test. It is not clear, however, what criterion has been taken. The first essential would be that the lamps should be of equal candle-power (or corrections introduced), and that they should obey the inverse square law. Perhaps the authors could give some further information in regard to the basis here adopted.

Mr. Baker. Mr. C. A. BAKER (*communicated*): In regard to the central suspension which the authors advocate, it was found in 1887, in connection with the street lighting of Milan, that equivalent illumination could be obtained with 30 per cent fewer lamps when a central position was secured than with the usual arrangement of lamps fixed along the edge of the footways. The public electric lighting of Milan was all carried out on this system at that date, Turin and other cities following the lead. As to the use of inner globes, some kind of diffuser is essential; with small arc lamps no doubt the diffusion should be done as close to the arc as possible; with more powerful lamps it is perhaps equally permissible to use a clear inner globe and an outer diffusing globe. The dioptric patterns are usually rather expensive to buy and difficult to keep clean. I cannot help thinking that the height adopted, viz. 27 ft. 6 in., is still too great, and that a better effect might be obtained by reducing this height with some further modification of the over-reflector. I have found that metal filament lamps offer so many advantages for street lighting that it has been practicable to replace a large number of arc lamps with them. As further advances are made in improving metal lamps and reducing their cost, I am of opinion that future street lighting will be influenced by the increased use of metal filament lamps rather than of arc lamps; for any new scheme of public lighting, except for very wide thoroughfares, the merits of the metal lamp should be fully weighed before a system of arc lighting is decided upon.

DISCUSSION BEFORE THE MANCHESTER LOCAL SECTION ON 25TH FEBRUARY, 1913.

Dr Rosenberg. Dr. E. ROSENBERG: After seeing Portland-street and Princess-street there can be no doubt that the electric lighting is far preferable to the high-pressure gas lighting shown there; and it is to be hoped that electricity will in consequence be much more extensively used for street lighting in Manchester. I believe, however, that due to the competition between the rival illuminants both the electrical engineer and the gas engineer have tried far too much to provide a "brilliant"

light. It is not only in the streets of Manchester, but also in London and all over the world where competition exists between electricity and gas, that the aim of the street-lighting authorities seems to be merely to bring before our eyes street lamps giving thousands of candle-power. The lighting which one generally sees, not only in the streets but also in houses and workshops, gives one the impression that the knowledge of the principles of illumination is still very restricted; and therefore a competition for street lighting based on the brilliancy of the lamps is not setting a good example. Our grandparents knew that glare was bad for the eyes, and that things on the table could be seen better at night when a shade was put over a candle. If the photometer had been known at that time perhaps they would have been scared, just as illuminating engineers are to-day, at the great loss of radiation if the rays from the candle were shut off in one direction. I think we use the photometer too much. It does not show what our eyes see. The authors state on page 607 that the conception of the intensity of illumination by the human eye follows a logarithmic law. Thus, if a certain illumination appears to us, say, as unity, an illumination 1,000 times stronger perhaps gives an impression of only being 10 times stronger. If, therefore, the thousandfold strength of rays are directed into our eyes, and we see at the same time another object having perhaps an illumination 10 times unity, our eyes with their narrow pupils do not see this as 10 times, but perhaps only as one-hundredth of normal illumination, which means that it appears absolutely dark. The only consolation electrical engineers have is that the glare of the gas lamps is still worse than that of the electric lamps, because gas engineers do not seem to be endeavouring in the least to diminish the glare. This competition reminds me of a letter of James Watt, reporting on one of his early steam engines, wherein he said— Dr. Rosenberg.

> "The velocity, violence, magnitude, and horrible noise of the engine gave universal satisfaction to all beholders, believers or not. I have once or twice trimmed the engine to end the stroke gracefully and to make less noise, but Mr. Wilson cannot sleep without it seems quite furious, so I have left it to the engine men; and, by the by, the noise seems to convey great ideas of its power to the ignorant, who seem to be no more taken with modest merit in an engine than in a man."

I believe we have also to learn this modest manner in street lighting. The knowledge of illumination undoubtedly exists. We can perhaps once in a thousand times see a room or a shop window which is well lighted and which gives us a comfortable impression because the lamps are not visible to our eyes and the things we wish to see are. I venture to hope that in future competition between the rival illuminants the comfort of the pedestrian will be more considered than is the case at present.

Alderman W. WALKER: The statements of cost given in the paper are, from our point of view, superlatively safe. For instance, the cost Alderman Walker.

Alderman Walker.

of supplies for the year 1911–12 is given in Appendix A as £447,498, whereas in the Abstract of Accounts of the Electricity Department I find the actual amount is £407,998; and if £24,500, the sum given in aid of rates, be added there is still a margin of £15,000. In Manchester only 0·23 of a unit per head of population is used for public lighting, whilst at the other extreme we have Edinburgh with 6 units per head of population. Somewhere between these two figures is a consumption which we ought to get. On page 610 the authors enumerate the practical advantages of electric lighting by arc lamps, and to their list I would add "reliability." In the report of the two experts on the installation under review Mr. Abady mentioned as one advantage of the gas lamps "freedom from involuntary extinctions." Unfortunately for him the report contains the date and the duration of every failure noted during the two months when the tests were being carried out; the failures are as follows:—

Gas Lamps.

16th June,	No. 2	Lamp out	20	minutes.
18th "	No. 2	"	60	"
23rd "	No. 3	"	all night.	
21st July	No. 4	"	"	

Arc Lamps.

14th June,	No. 4	Lamp out	80	minutes.
19th July,	No. 5	"	20	"

Mr. Abady also says "for the purpose of comparison all the arc lamps were observed." This means that 16 arc lamps, 8 of which burnt all night, are compared with 4 gas lamps which are extinguished at 11 p.m. The 4 gas lamps showed 4 failures, 2 of which were total, against only 2 failures of the arc lamps; yet Mr. Abady calmly asserts that one advantage of gas lighting is freedom from involuntary failure. A result of these competitive installations will be to make electric supply authorities assert themselves in this matter of street lighting. We in Manchester have been told that, because the makers of gas lamps publish wonderful results, obtained at a place unknown, at a date unrecorded, and under conditions not specified, that the matter was settled for all time; and electrical engineers have been relegated to a perpetual back seat. The installation under review was the Gas Committee's own choice, both as regards location and terms; they have been well beaten, and as a result a large share of the street lighting must come to us.

The penetration of fog by the rival illuminants was a point about which considerable doubt existed. I know that prior to the tests one of the gas journals called attention to this supposed defect of the arc lamps, and stated that during a fog darkness would exist in the streets where the arc lamps were installed. We have found that under the weather conditions named, Portland-street, with its lamps a greater distance apart than the gas lamps in Princess-street, is much

better illuminated than the latter street. Personal observation will verify this statement. The authors ask us to agree that uniform lighting is flat and uninteresting. If they mean uniformly graded lighting, I do not agree with them, as I object to areas of different light densities within the same radius. Personally the polar curve shown in Fig. 18 is the one I prefer. It is, in my opinion, better for securing the desired effect than the one given by the combination ultimately adopted. It shows very clearly that an even gradation of light can be obtained with that particular combination of globes. I would ask the authors to tell us if they have taken into their calculations of cost anything for the low-pressure gas used in the by-passes of the high-pressure lamps; this ought to be done. I believe in Westminster it was about 4s. per lamp per annum. The cost of the installation per mile of street is one which will cause street-lighting authorities to weigh carefully the advantages of arc lamps. I wish there could have been included in these tests a short length of secondary street with metal filament lamps. I am sure that at an early date there will be in this city a good example of this style of lighting for comparison with low-pressure gas lighting; and with the result of such a test added to that in the paper, the latter will be referred to as a standard on street illumination. Alderman Walker.

Mr. H. R. BURNETT: Only those who have had experience of photometric work, and more especially of street photometry, can fully appreciate the enormous difficulties which crop up in connection with this work, and I feel sure that this paper will be referred to for a long time to come as a standard work on this subject. There are only one or two points which I should like to criticize. With regard to the costs; in Appendix C we have the annual costs, and I should be glad if the authors would give us a little more information on these. For example, trimming and maintenance are given as costing 16s. 8d. per annum for 4,000-hour arc lamps. This is certainly a remarkably low figure. Assuming that the lamps burn for 65 hours per trim, it works out at 3·25d. per trim. This strikes me as being a low figure to cover wages of men alone, but in addition there must be other repairs and maintenance charges, and I should like to know over how long a period this figure was obtained. On page 614 the comparative costs of the gas lighting and arc lighting are given. The cost per mile of street per annum is given as £254 for the arc lighting, which figure is apparently based upon £6 10s. 8d. per lamp per annum (Appendix C). It must be remembered, however, that this does not include any capital charges, and is only for lamps burning until 11 p.m. If the capital charges are added, the cost of all-night lighting calculated upon the authors' figures, which are of course much lower than would be obtained in most towns, amounts to £500 per annum per mile of street. This leads to the important questions: What towns can afford to spend this amount of money on lighting even the more important streets? Have not the authors gone too far in fixing 0·5 foot-candle as the minimum illumination? Is it not more than is really necessary, Mr. Burnett.

Mr. Burnett.

and more than we can afford? It must be remembered that if we light our main streets to this extent, we must raise the standard of illumination of all side-streets running into them. In connection with the capital charges, I notice that only 8½ per cent is allowed for interest and depreciation. This appears to me to be quite inadequate, but there is doubtless some explanation. The point Alderman Walker has raised in connection with secondary street lighting is important, and I hope Mr. Pearce will soon carry out some experiments in the lighting of such streets and present the results in a similar manner to that in which he has given those for arc lighting. There is a certain minimum illumination at which metal filament lighting becomes cheaper than flame arc lighting, and from calculations which I have made, based upon the costs given in the paper, I am of opinion that for minimum illuminations of less than 0·1 foot-candle, metal filament lighting must be resorted to.

Mr. Angold.

Mr. A. Angold: The authors have given a very good explanation of how they arrived at the best globes for the purpose, but they do not tell us very clearly about the design of the globes for what they call defracting purposes, but which I call reflecting purposes. I do not think raising the fittings ⅜ in. produced all the improvement. The change from one globe to another had a lot to do with it. I should like to explain how it is that the new globes boost the light up at the angles of 15 deg. and 20 deg. The glass, although clear, has a considerable reflecting power. Now if the glass can be so shaped that the inside of the globe acts like a mirror in a searchlight (and the curves of the globes, both inner and outer, are designed on these lines), a beam is reflected by the inner globe in this particular instance along the 20-deg. line, and another beam from the outer globe along the 15-deg. line. Comparing this with the old inner globe it will be seen that each ray of light striking the flat surface of the inner globe must go off at a corresponding angle, and the reflected light is not concentrated in any way. Reflections on the inner surface of the gas-lamp globes are referred to, but the authors neglect to mention it in connection with the new arc-lamp globes. In regard to the glare from the clear globes shown in full lines in Fig. 3, I do not think this should be very noticeable in looking at the globe anywhere between 35 deg. and the horizontal, because of the fact that at those angles so much light is coming from the surfaces of the globes. On looking at the arc lamps in Portland-street or Piccadilly it will be seen that there is a cone of light from top to bottom of the globes between 30 deg. and say 10 deg. After all, 35 deg. is somewhere about the average height of the sun at midday. To look up at an angle of 35 deg. means quite a noticeable raising of the eye, and "the man in the street" does not often lift his eye to that angle. The graded frosting referred to is a very good idea. I do not know whether success has now been arrived at in getting these globes made with a graded flashing, but I do not think that the frosting or etching will be permanently satisfactory, because all frosted globes tend to take up dirt, and the frosting spoils the effect

of the reflection I have mentioned. I should like to know what type of globe has been finally adopted. In regard to the splash of light from the dioptric globes at about 80 deg., the authors suggest that it is from the hole in the bottom of the globe, but as a matter of fact a light from an arc of the power discussed would not be strong enough to give 6,800 c.p., especially as it has to pass through an outer globe. I think it is due to some reflections inside the prisms. Mr. Angold.

Dr. T. ZETTEL: Arc lamp manufacturers are not usually in a position to carry out street-lighting tests under practical conditions, and if Corporations like Manchester take over this work for the manufacturers it must be in the interests both of manufacturers and users. No doubt similar tests have been carried out by other Corporations, but in no previous instance have all the results been published. It is only recently that the design of arc-lamp globes has received serious attention or that an endeavour has been made to design them scientifically; in order to further developments of this kind the authors' tests and results are highly encouraging. I should like to ask whether any special attention has been paid to the state of the globes when the tests were taken—whether they were dirty or cleaned before the tests, or whether they were taken under ordinary operating conditions. Experiments have shown that a great difference is found in candle-power figures taken with clean globes and with globes which have been in service for two or three days. I should also like to know whether the experiments with gas lamps have been treated in the same way. Variations have been found due to the position of the photometer relative to the carbons, and it would be very interesting if the authors could give us results with different relative positions of the photometer to the carbons. No particulars are given concerning the type of reflector used; such information, however, even if furnished at a later date, would be very interesting to everybody engaged in street lighting. Dr. Zettel.

Mr. E. M. HOLLINGSWORTH: Some time ago we abandoned arc lamps in favour of metal filament lamps, and I must say that with the latter we have obtained a more even illumination. I was therefore interested to hear from Alderman Walker that there is a possibility of having at an early date a paper by the authors on street lighting with metal filament lamps. Such a paper would be of still more interest than the present one on arc lighting. From my experience with metal filament lighting, we can hold our own in competition with gas lighting at ordinary pressures; but I am afraid, from the figures given in the paper, that the metal filament lamp will not compare as favourably as the arc lamp when in competition with high-pressure gas. The manner in which the authors have arrived at the cost of current for street lighting is of interest to power-station engineers. I should like to know how the charge of 0·6d. per unit compares with the total cost of production. Our charge for current for street lighting is 0·62d. per unit, and this is in competition with gas at 1s. per 1,000 cubic feet. Mr. Hollingsworth.

Mr. H. T. Wilkinson.

Mr. H. T. WILKINSON: On page 599 it is mentioned that the overhead system of suspension is used. I do not think that this system is used in England as much as it should be—it is used on the Continent much more freely. In one instance on the Continent I saw arc lamps suspended right across a square. That was rather an extreme case, but I do not think it was quite as large a square as Albert Square—there were no poles in it whatever. Of course in Portland-street it is obviously the right thing to do, because it is a comparatively narrow street. Lights on side poles, especially if staggered, cause a moving object to cast shadows varying in intensity and moving around the object in its progress from lamp to lamp. In these days of fast vehicular traffic, it is more and more necessary to suspend the lamps centrally over the roadway itself. Mr. Pearce says that Portland-street is a very unsatisfactory street to illuminate. Anyone who knows Portland-street must agree, and I should like to ask why this street was chosen, as I cannot understand why such an unsuitable street should be chosen for a test. I should imagine that the choice was made by the Gas Department. I do not agree with the authors in the last paragraph of the paper. Perfectly uniform lighting may be flat and uninteresting, but we are not likely to get anywhere near that state of artistic imperfection; and I think that the most even illumination possible should be aimed at for the reduction of shadows, and to increase the apparent efficiency by enabling the pupil of the eye to expand and remain so all the time. The question of glare is also important, but not easily overcome if maximum illumination is to be obtained at least cost. Dr. Rosenberg was very severe, but he did not suggest any practical solution of the problem. The height of the lamps in Portland-street overcomes this, unless one wilfully looks at the arc itself at a distance, and then the distance itself reduces the effect.

Mr. Koppel.

Mr. A. A. KOPPEL: The arc lamps in Portland-street are too brilliant; they might be useful for advertising purposes but unsuitable for street lighting. I agree with Dr. Rosenberg that glare should be avoided, and that a scientific basis of lighting is important for the public well-being. Yellow-flame arc lamps should not be used for street lighting. For equal distribution of light, and for reducing the dark shadows complained of by the authors, three 200-watt metal filament lamps in a single fitting would be more suitable. The experiment would be worth trying of having units of 3,000 to 4,000 c.p. at heights of 100 ft. from the ground with special reflectors and suitably spaced. This system could of course not be employed in narrow thoroughfares unless the buildings were whitewashed, and where metal filament lamps as suggested could be adopted.

Mr. Cunliffe.

Mr. R. G. CUNLIFFE: Fig. 8 and similar figures show by shaded areas the light distribution on the road surface. Does the photometer show abrupt changes as indicated in these figures? Visual examination often indicates almost identical results. Observers standing at the intersection of Portland-street and Princess-street and observing first the electric and then the gas lamps are unable to detect any great

difference in the effects produced by the lamps on the eye. Visual tests are therefore not sufficiently delicate to distinguish between the lamps as regards intensity of illumination. Dr. Rosenberg referred to the effects of glare, but those who have to drive in the above streets express great appreciation of the centre lighting as carried out by the authors and as compared with side lighting, in which case the lamps invariably throw a distracting glare in the eyes of drivers of vehicles. The average street gas lamp is placed at a great disadvantage owing to its comparative proximity to the eye as compared with the suspended centre lights. In Piccadilly there are three rows of street lamps, and the high-power gas lamps on the island platforms are placed too low, as their excessively glaring mantles are within only a few feet of the eyes of passengers on the upper decks of tramcars. This portion of the city should present a good opportunity of lighting by suspended lamps. The penetrating power of the light from different lamps can be tested by observing the distance at which the light becomes invisible to the eye. Tested by this method in foggy weather the arc lamps are found to be far more penetrating than the gas lamps. One effect of central lighting by suspended lamps of high illuminating power is the inevitable speeding up of street traffic due thereto. The value of good lighting cannot be overestimated in the interest of the drivers of high-power and high-speed vehicles, and I attribute much of the congestion at night-time to the slow and cautious driving due to inability of the drivers to obtain a clear view of the roadway for a fair distance ahead of their vehicles. Mr. Cunliffe.

Mr. A. WILKINSON: I am of the opinion that the new type of globe has a fixed focal point, and that care must be used in fitting them to the spinnings. If in drawing up the net wires one side is pulled tighter than the opposite side, the globe will be canted out of the centre line. This will alter the angle at which the light rays strike the inner surface of the globe, the resulting effect being to deflect the rays on one side upwards and on the opposite side downwards. The shape of the polar curve is thus altered. The 10-deg. to 20-deg. rays are direct and will not be affected by the canting. The 40-deg. to 80-deg. rays, however, are entirely reflected, and it is important that these should not be distorted. As this is a most important point, I think some consideration should be given to it by globe manufacturers and designers. I have also found that globes with necks are preferable to those without. In regard to the use of reflectors, I do not think that the use of those mentioned by the authors would be successful, owing first to the arching effect, and also to the further difficulty that with the type of lamp used in Portland-street the carbons are fed alternately from the right and left of the vertical centre line of the lamp and its globes. The arc is thus burning about ¼ in. out of centre, and the shadow of the lower part of the frame and ash container is projected on the ground eccentric to the centre line of the lamp and ground. On the arc changing over to the incoming carbons, the shadow is then projected to the opposite side of the centre line, Mr. A. Wilkinson.

Mr. A. Wilkinson.

and as this is repeated every five hours it will be a difficult point with which to deal.

Mr. Wheelwright.

Mr. P. P. WHEELWRIGHT: I think that, for main-street lighting, the gas supply is very clearly and fairly shown to take second place compared with electricity. In these days of motor traffic the main thoroughfares are becoming more like railway tracks, and decidedly more dangerous, therefore the lighting must be improved to ensure the safety of the public; in addition to this, well-distributed and attractive lighting is an asset of considerable value to a town. For instance, in Blackburn, where people from surrounding districts regularly come to do their shopping, it is an acknowledged fact that the better-lighted thoroughfares are the more attractive. I am of opinion that the main streets should be brilliantly and well lighted, and that the warmer the colour the better the general effect.

Mr. Locke.

Mr. T. A. LOCKE: With regard to the usual methods of measuring illumination by photometer tests, the difficulty of obtaining uniform results is well known. Owing to the personal element and the dependence upon individual visual organs, little reliance can be placed upon the measurements. Having also realized how difficult it is, especially for the layman, to comprehend the meaning of polar curves, I have for some time been experimenting with a view to obtain a method which gives results capable of accurate repetition. By the use of a standard photographic plate in an ordinary camera, light from the illuminating source being allowed to fall upon a standard medium of special form, and the plates being exposed and developed under standard conditions, it is possible to obtain results showing exactly how the intensity of illumination varies at different angles and distances from the light source, errors due to the personal element thus being eliminated. By the use of a plate graduated to represent foot-candles at different distances from the light source, it is possible to take definite readings from the photographic record made under standard conditions and to have the record permanent. The great advantage of this method is the elimination of variations in the intensity of the light due to fluctuations of voltage, and in arc lamps also due to slight variations in the quality of carbons causing flickering, because the observations recorded will be the summation of the means of the instantaneous illuminations instead of the approximate instantaneous values as with the ordinary visual photometer. I have used this method for illustrating the distribution of illumination from carbon and metal filament lamps, and find it of particular advantage in demonstrating the applications of various forms and makes of shades and reflectors, the layman usually being able at once, without preliminary examination and explanation of the scientific character of the records, to make a correct choice of the values of the illuminations demonstrated. The authors have shown very clearly how they have improved the illumination in the direction of the 15-deg. and 20-deg. angles by the use of reflectors and special globes, and it seems as though with globes and reflectors having different focal lengths, according to the

directions in which the illumination is required along or across the street, further improvements could be made. The suggestion is that globes and reflectors should be elliptical in horizontal cross-section instead of being circular. The latter form, in order to give the desired effect along the street, also results in an intense illumination on the walls of buildings, where it is absolutely useless. This light could be deflected so as to increase the illumination along the street, and if this suggestion could be carried out, the further result would be obtained of increased economy of lamps and current, for the spacing of the lamps could be greater to secure the same foot-candle illumination as at present. Mr. Locke.

Mr. J. W. Record: Dr. Rosenberg, in referring to the human eye, struck a note which should be taken up by those concerned with lighting problems. It is well known that the condition of the human eye in the daytime is different from what it is at night; and it is not much good producing enormous candle-power if the human eye is not enabled to see better thereby. I believe that if the verdict of a dozen persons were taken, instead of the reading of a photometer, the above point would be realized. Mr. Record.

Mr. J. Jackson: I note that the nominal candle-power of the gas lamps is given at 4,500, whereas the actual tests show that only half this value has been obtained. Is this accounted for by the lamps being centrally suspended, thereby resulting in considerable leakage and consequent drop in pressure? If this does not account for the discrepancy, then it is owing to the fact that at some time or other under exceptional laboratory conditions the illuminating value of 60 candles per cubic foot of gas was obtained, the subsequent method of rating the various units being to take the consumption per burner and multiply it by the above figure, which of course gives an altogether misleading and valueless result. I should like to know if the method of suspending the gas lamps by means of span wires attached to the abutting buildings has proved satisfactory. I should expect that trouble will result sooner or later. If, as mentioned by one speaker, the low-pressure gas supplied to the by-passes had not been included, this item may account for anything between 5s. and 10s. per lamp per annum. I also wish to refer to the maintenance charges given in the paper. For electric lighting the figure of 0·05d. per lamp works out at 150 lamps per man; I have no hesitation in stating that no man can trim and keep in order 150 flame arc lamps. Again the figure of 27s. 2d. per lamp per annum quoted for the maintenance of the gas lamps is altogether too low, and in both cases therefore the charge should be increased by at least 50 per cent. Mr. Jackson.

Professor E. W. Marchant: With regard to the absorption of light by the globes, some 2 or 3 years ago I made a large number of tests on globes, and found that frosted globes absorb a good deal more light than might have been expected. It would be very interesting to know if the authors have made any measurements of the light absorbed by the globes. I should like to ask the authors if a Trotter photometer Professor Marchant.

Professor Marchant.

was tried, *i.e.* one in which a flat plate is illuminated by the light that is being examined. I have found this type of photometer very much better than the flicker photometer, which is more trying to the eyes than any other type of photometer I have used. The tests made to determine the distance at which a printed sheet could be read was one for which it is very difficult for physiological reasons to get even approximately exact results. The rods and cones of the retina which are used at night are different from those used in the daytime—their capacity for detecting colour differences is different and their sensitiveness is much greater. If the eye is rested to allow this part of the retina to work, the distance away from the lamp at which print can be read would be much greater than it would be for the ordinary retina. On page 612 there is a statement as to what is called a "variation factor." I hope the authors will change this to "diversity coefficient." Many names have been given to this quantity. It was first called a "diversity factor"; later this was altered to "diversity coefficient"; but so far as I know "variation factor" is a new term.

Mr. Cramp.

Mr. W. Cramp (*communicated*): There are one or two points in the paper to which the attention of Manchester engineers in particular should be drawn. The first is the extraordinary effect apparently due to the street lighting having been in the hands of the Gas Committee, namely the small amount of electric lighting which has been done in the streets of Manchester. But there is a more important point; I refer to the statement in the paper that 2s. 3d. per 1,000 cubic feet has been charged for gas for street lighting. Considering that the Gas Committee were contributing to the rates, and have even drawn attention to their large contribution, it seems scarcely right that some of this contribution should have been obtained by charging such a price to the community. I think that some public explanation of these facts is due. Again, why were the capital charges omitted from the figures for the cost of the gas lighting? I should like to know how the series groups of electric lamps mentioned on page 600 are connected. Do alternate lamps belong to different series, or not? The definite statement as regards light rays in fog on page 619 is very interesting, but it does not appear quite to accord with the experience of lighthouse authorities. I think it is unfortunate that some statement is not included as to the use of high candle-power metal filament lamps for street lighting. I know these have been used in Shrewsbury-street. What is the result?

Discussion before the Birmingham Local Section on 12th March, 1913.

Mr. Chattock.

Mr. R. A. Chattock: The figures in the paper demonstrate that high-pressure gas lighting is considerably more costly than arc lighting. Thus, the cost of the high-pressure gas lighting amounted to £11 9s. 2½d. per lamp per annum, without including anything for interest and depreciation on the capital spent on the installation and without globe renewals while the cost of the arc lighting was £11 7s. 8d. per lamp per

Mr. Chattock.

HANDSWORTH STREET-LIGHTING.

	Consumption in units per annum.	Cost of Current at 1d. per Unit.	Cost of Carbons.	Cost of Labour.	Total Cost.	Illumination in Foot-candles measured on Road Surface.		
						Maximum.	Mimimum.	Average.
MAIN ROAD.		£ s. d.	£ s. d.	£ s. d.	£ s. d.			
Original Installation—50 open-type arc lamps, 11-ampere, 18-hour	44,750	186 10 0	17 10 0	146 0 0	350 0 0	0·51	0·02	0·140
Present Installation—50 magazine flame arc lamps, 8-ampere, 70-hour	33,000	137 15 0	50 0 0	72 15 0	260 10 0	2·35	0·05	0·660
SIDE (Residential) ROADS.								
Original Installation—70 open-type arc lamps, 11-ampere, 18-hour	65,000	271 10 0	25 0 0	204 0 0	500 10 0	0·51	0·02	0·140
Present Installation—70 twin-carbon flame arc lamps, 7-ampere, 50-hour	40,000	166 10 0	79 10 0	100 10 0	346 10 0	1·00	0·04	0·327

Mr. Chattock.

annum, including all capital and other charges. In Birmingham practically the whole of the street lighting is carried out by low-pressure incandescent gas mantles, and this is now under the control of a special Lighting Committee. There is a small amount of high-pressure gas lighting in the centre of the city, but this is really only for demonstration purposes by the Gas Committee. In the Handsworth and Aston areas, which were recently incorporated in Greater Birmingham, there is also a small amount of arc lighting, which was taken over and is controlled by the Electric Supply Committee, being paid for by the Lighting Committee. This arc lighting has recently been considerably cheapened in cost, and it may be of interest if I explain how this was done, and what effect it has had. Taking the Handsworth district as an example, there were originally 120 open-type 11-ampere arc lamps which required recarboning every 18 hours. These were replaced by 50 magazine flame arc lamps taking 8 amperes each, and requiring retrimming every 70 hours, and also 70 twin-carbon flame arc lamps taking 7 amperes each and requiring retrimming every 50 hours. The accompanying table gives particulars of the consumption of energy, illumination in foot-candles, and cost of current, carbons, and trimming, the lamps being spaced approximately 400 ft. apart. The 8-ampere magazine flame arc lamps showed a saving of 25 per cent in running costs, and gave approximately four times the amount of light furnished by the old open-type lamps, each lamp costing £5 2s. per annum for current (at 1d. per unit), carbons, and trimming, the burning hours being from dusk to 11·15 p.m. The 7-ampere twin-carbon flame arc lamps showed a saving of 30 per cent in running costs over the 11-ampere open-type lamps, and gave over twice the light, each lamp costing £5 per annum for current (at 1d. per unit), carbons, and trimming, from dusk up to 11·15 p.m.

After the arc lamps are switched out at 11.15 p.m. two 50-c.p. metal filament lamps are switched on in each post and are run until daylight. The cost of these metal filament lamps is not included in the above table. In connection with Fig. 14 reference is made to the rather pronounced shadows caused by the ash-trays; the same disadvantage was noticed in Birmingham in connection with the lamps fitted with converging carbons. It was found, however, that with the twin-carbon lamps fitted with vertical carbons no difficulty of this kind was experienced.

Mr. Crocker.

Mr. E. Crocker: The great problem in connection with street lighting appears to be to make what the authors term the "variation factor" as small as possible, and to make the change from maximum to minimum illumination as gradual as possible; and to do this *not* regardless of cost. It is of course quite easy to obtain uniform illumination by multiplying the number of lamps, but with a given number of lamps (as we found at Handsworth when this district became part of "Greater Birmingham") it is obvious that by increasing the candle-power, although the illumination over the whole lighted area is increased, the variation factor will remain

Mr. Crocker.

the same, and the areas of minimum illumination with the higher candle-power lamps appear to the man in the street much darker than they really are, by contrast with the more intensely illuminated parts. Hence the importance of improving the distribution. In this connection the design of the globes plays a most important part. We have experimented on much the same lines as the authors, with very similar results. We have not, however, tried frosted globes, and it is with these the authors have obtained their best results, which are very striking. For example—turning to Table III—with clear inner and outer globes and with opalescent outer globes the authors obtain variation factors of 8·5 and 7·58. In our experiments the variation factors were, of course, higher, in consequence of our lamps being twice the distance apart; but at the same distance as that at which the authors have taken their minimum measurements, viz. 60 ft. from the lamp, we obtained variation factors varying from 6·9 to 9. The authors' figure of 3·75, obtained with frosted globes, is therefore an enormous improvement. From tests which I made of the gas lighting in Bristol-road, the variation factor was as high as 18 over a distance of only 20 ft. from the lamp-post.

I quite agree with the authors' remarks *re* outside reflectors, and with the importance of illuminating the buildings at the sides of the road. While making our tests we heard plenty of remarks from the man in the street, and while we were experimenting with these reflectors the general opinion was most unfavourable. With the space above the lamps darkened, and the buildings at the sides of the road in shadow, the general effect was not at all pleasing, although the thoroughfare itself was well illuminated. With regard to shadows underneath the lamps caused by the ash-tray, while experimenting with various types of lamps the entire absence of this shadow in the case of one make was very noticeable, in fact, the space immediately underneath the lamp was the most brightly illuminated of the whole area; although there was an unusually large mass of metal just under the globe, yet there was no shadow at all, and the variation factor with this lamp, which was fitted with a clear cylindrical inner globe and spherical opalescent outer, was lower than that of any other make. This particular flame arc lamp had only one pair of carbons fixed vertically, as in the ordinary open-type lamp; the bottom carbon was made the positive, and it burned with a long arc, about 1 in. to 1¼ in. I am not advocating the use of this lamp, but merely mention it to call attention to the absence of shadow.

It is most important to aim at steadiness in street lighting; this is the most noticeable feature of high-pressure gas lighting, although, as the author remarks, the candle-power varies considerably from day to day. There is no better way of testing the steadiness of the light given by any lamp than by taking photometric measurements; variations quite unnoticeable by the man in the street are then easily apparent. When testing some of the arc lamps which we have had on trial at Handsworth I have been able to obtain a balance almost, if not quite, as readily as with gas lamps. The steadiness of arc lamps depends very

Mr. Crocker.

largely on the nature of the feeding mechanism, and I have found that lamps with direct electrically-driven feeds give the steadiest burning. By a direct electrically-driven feed I mean one in which the feeding is effected by an auxiliary shunt coil, which comes into action when the pressure across the arc rises above the normal, so regulating the feed that the pressure drop across the arc varies not more than one volt, thus maintaining steady burning; whereas with a slip feed, which depends upon the slip of a chain or band brake, or piece of metal on a wheel controlled by a lever operated by a main and shunt coil, it is impossible to depend upon a one-volt feed; generally the variation is two or three volts or more, and this affects other lamps in series, especially if these are themselves on the point of feeding. There appears to be a tendency on the part of the makers of magazine lamps to design lamps so as to accommodate as many pairs of carbons as possible, in order to increase the burning hours per trim. There is a limit to the advantage of this. One sees lamps advertised to burn 130 and even 200 hours without retrimming. My experience with flame arc lamps is that they require cleaning and the deposit from the carbons brushing out at least after 60 to 70 hours' burning, and if the lamp has to be lowered for this purpose it may just as well be carboned at the same time. Another important point with regard to the choice of magazine lamps for street lighting is that there should be no extinction of light when a new pair of carbons comes into operation. There are some excellent lamps on the market which have this defect, and although the makers state that the period of extinction does not last beyond two or three seconds, I have known it extend to from 15 to 18 seconds.

With regard to the flicker photometer, this certainly has an advantage over other forms as regards difficulties of measurement owing to difference in colour between the illumination from the standard lamp and that from the lamp under observation, but, as the authors say, the continual use of this instrument puts a great strain on the eyesight. My observations of the illumination were taken on the ground level. The instrument was of the size of an ordinary hand camera, and was used in conjunction with a test card of dull white celluloid. The scale on the instrument, which is graduated in foot-candles, is standardized in the laboratory by illuminating this card to a known value. The card can then be put down on the ground at any point where it is desired to measure the illumination. I consider all street photometer measurements are more valuable as a comparison of the illumination from different sources than for absolute measurements of intensity. In this respect I do not think we can look for accuracy beyond about 5 per cent. It is gratifying to know that we are able to battle successfully against high-pressure gas. In respect of illumination we can compete favourably by means of high-candle-power metal filament lamps, but as regards illumination and cost I think there can be little doubt that it is the flame arc lamp that will give us the victory.

Mr. Solomon.

Mr. M. SOLOMON: With regard to the contour figures on pages 611, 612, 615, 616, and 618, I would point out that if Fig. 8, which relates to

Mr. Solomon.

the arc lamps in Portland-street, is casually compared with either Fig. 9, which is for the gas lamps in Princess-street, or Fig. 10, which is for the gas lamps in Mosley-street, one gets the impression that the general illumination produced by the gas lamps is as good as, or better than, that due to the arc lamps. As a matter of fact, however, the shading is different in the two sets of figures, the unshaded portions of Fig. 8 representing an illumination of 1 foot-candle or over, whereas in Figs. 9 and 10 the unshaded portions only represent an illumination of 0·5 foot-candle or over. Again, the comparative table on page 614 shows the arc lamps to be, roughly speaking, twice as good as the gas lamps; but I think it was admitted that the gas-lamp figures are not really representative of the best that can be done with high-pressure gas lighting. Mr. Abady in his report claimed, I believe, that they were only about half as good as could be obtained; and even Mr. Harrison subscribed to the opinion that the gas lamps were not as good as they might be. I only emphasize this point as I do not think that it is of any benefit to create the impression that arc lighting has a walk-over as against high-pressure gas, when, as a matter of fact, I believe it is, and will be for some time to come, a very close race between the two rival illuminants. With regard to the method of obtaining suitable distribution curves by graded frosting applied to the outer globe, I should like to point out that it would appear from the passage on page 603, dealing with the subsequent alterations to the lamps, that improvement in the distribution curves (apart from that due to the graded frosting) is attributed to the alteration of the spinnings and of the shape of the outer globe. As a matter of fact the shape of the inner globe was also altered, and a very considerable amount of the improvement in the 20-deg. rays is due to this alteration, as the new-pattern inner globe was one designed so that the light reflected from its inner surface should be reflected at an angle of 20 deg. with the horizontal. I read before the Institution last year a paper on "Yellow Flame Arcs," * in which I published a number of light-distribution curves. One curve was, I believe, for practically the same lamp as the curves in Fig. 13 of this paper. I have been impressed by the very close correspondence between the curve obtained by the authors and my own curve. As my curve was obtained under laboratory conditions, and theirs under working conditions in the street, it is interesting to note this confirmation of my results, as it not only confirms the accuracy of both sets of readings but also shows, I think, that flame lamps and flame lamp carbons are now so standardized that the results obtained may be relied on with considerable certainty. I am also interested to note Mr. Crocker's remarks that it is not advisable to overload the magazine with carbons in order to obtain longer burning hours with one trim. I deduced the same conclusion from a series of experiments described in the paper already referred to, and it is of interest to note that this conclusion is confirmed by results obtained in actual practice.

* *Journal of the Institution of Electrical Engineers*, vol. 49, p. 737, 1912.

Mr. Taylor.

Mr. A. M. Taylor: I think the most important part of the paper, from the point of view of the man in the street, is the table on page 614, which comprises a *résumé* of the principal features, such as candle-power of lamps, running costs per lamp per hour, running costs per 1,000 c.p.-hours, minimum illumination, etc., for gas and electricity. I suggest that to the items given might be added the variation factor for the two systems, and I suggest an additional basis for comparison, which also might be added to the table, and which might be called the "Maximum variation of foot-candles per foot run." As regards the cost of current, I think that the gas people cannot and should not take any exception to the division of the total costs, viz. £447,498, into "running costs" and "fixed costs" of £87,431 and £360,067 respectively, neither do I think that they can take any exception to the deduction of £25,078 and £84,311 respectively (see page 631) from the above figures on account of the cost of traction supplies included in the previous figures. A committee of experts has decided on this particular method of computation, and electrical engineers are surely the ones to say what is the correct way of charging for electrical energy. The authors have, however, carried the distribution a little further, and allocate a certain proportion of the above £275,756 (£360,067–£84,311) to lighting and to power in the proportion of £147,371 and £128,385 respectively. This again is a point on which electrical engineers are fully justified in contending that their own methods of computing fixed charges are correct. Exception, however, has been taken to that method, and I have therefore worked out what the fixed charge would be if the whole £275,756 had merely been divided by the total number of kilowatts. In this case it comes to £8·76 per kilowatt connected per annum. This increases the cost of current approximately 25 per cent for the "all-night lamps," and approximately 33 per cent for the "11-o'clock lamps." I wish to emphasize that I merely made this calculation out of curiosity, and as it is obviously an extreme view it may be taken as certain that the authors' figure of £6·133 per kilowatt is substantially correct and cannot be seriously challenged. It therefore follows that Appendix C, which is based upon this charge, is also correct. According to the table on page 614, the cost per mile per annum (for presumably equal minimum illumination) is given as £675 for the gas lamps and £254 for the arc lamps. Gas, according to this statement, is 2·65 times as dear as electricity for equal illumination, so that it will be obvious that any trifling divergencies from the authors' figures for the cost of current which might be suggested by the gas people cannot to any material degree affect the great superiority of the figures for electricity on the basis of cost; and, after all, cost is the principal factor, provided that equal illumination is given.

Discussion before the Scottish Local Section on 18th March, 1913.

Mr. Mitchell.

Mr. R. B. Mitchell: I disagree with the views expressed by the authors on page 600 of the paper, and I strongly support those who

consider that lowering gear effects a great saving. I should like to relate our experience in Glasgow. We have here, I think, a system which is complete. That is to say, we have not only gone in for flame lamps with lowering gear but for magnetically-operated switches with control from a central point. Where flame arc lamps are substituted for ordinary arc lamps it is almost necessary to adopt these appliances owing to the greatly increased cost of carbons with flame lamps. Economies have to be sought in other directions in order that the total cost may not be seriously affected. Lighting authorities in general are quite willing to accept the greatly increased illumination, but they are not willing to pay more, at any rate not very much more, for the change. It is by automatic switching, long-hour lamps, and lowering gear, that these economies may be effected. In Glasgow we now have 837 arc lamps, nearly all flame arc lamps, and we are busy fitting lowering gears to the last 100 lamps. When the electric control was first introduced the trimming staff numbered 23 men. This change enabled us to reduce the number to 11 with 2 inspectors. With the long-hour magazine lamps and lowering gear a further reduction to 3 regular men, 1 spare man, and 2 inspectors, will be brought about. Each man attends to 62 lamps per day, and there need be no Sunday work. During the summer two of the four men can be taken off the lamps to do overhauling in the shop. The switching is done by the inspectors, who, after switching on, set out on bicycles to do their rounds, each inspector taking one-half of the city. All the lamps are switched on in five minutes and switched off instantly. I consider that electric control should be taken into account for any new scheme of public arc lighting. The control cables can be laid at small expense at the same time as the arc lighting cables. Mr. Mitchell.

My experience is that the candle-power of high-pressure gas lamps falls off 50 per cent, and that quickly. I have found the candle-power of high-pressure gas lamps at Glasgow to decrease by about $33\frac{1}{3}$ per cent in one week's time, the first reading being 3,200 c.p. at 20 deg., and the second reading being 2,200 c.p. at 50 deg., and at 20 deg. less than 2,000 c.p. In the London discussion it was mentioned that the gas pressure at Manchester was not so high as in London: I should like to know the reason of this difference. The authors remark that the use of clear globes is objectionable owing to the glare causing eye-strain. I do not think there is glare to that extent. Even when the globes are quite clear the light appears to emanate from the whole surface of the inner globe, more intense at the top, of course, but the arc itself should not be taken as the locus. I consider that clear globes with only their lower surfaces obscured are inevitable in view of the competition to be faced. The gas authorities never think of using anything else than clear globes, glare or no glare. With reference to the experiments made with dioptric globes, the authors state on page 623, that the tremendous dip in the curve was due to the open lower end of the globes. No doubt that is so, but did not the authors also find when using a dioptric globe with an open

Mr. Mitchell

lower end that the fumes from the carbons condensed on the cold surface of the outer globe and in a very short time obscured it? Some experimental lamps at Glasgow showed this effect after having been in place for only two weeks.

Mr. Wilson.

Mr. A. WILSON: Although a gas man I can appreciate the amount of work carried out by the authors in preparing this paper. It appears that there was evidently great room for improvement in the flame arc lighting, and the experiments with the globes show that comparatively unimportant details have a very strong bearing upon the important results afterwards obtained. I am glad they have gone so far as to state that there is much to be said in favour of both systems of lighting. They also say that gas lamps give a much steadier light, but they go on to argue that unfortunately the candle-power of such lamps varies very considerably from day to day, and that the light given by a particular lamp may decrease at least 50 per cent before the mantle is renewed. I grant that there is deterioration of the mantles, but our experience is that the deterioration is nothing like 50 per cent. The results of our tests are borne out by the testing done regularly in London in connection with the high-pressure gas lighting there, where the gas authorities are compelled by the conditions of their contract to maintain a certain minimum candle-power or to be fined very heavily. With arc lighting the candle-power undoubtedly varies a great deal from moment to moment, and that is far more objectionable than a gradual deterioration from week to week, or from month to month. Some of our mantles burn from 3 to 4 months, and we try these to see how long they will last. On page 610 the authors state that from the point of view of illuminating effect there is much to be said in favour of both systems, but that the electric lighting system possesses all the practical advantages, a few of the more important of which the authors give, and with these I entirely disagree. I will run over them briefly. As to lower cost, I agree that what they say may be quite right as regards Manchester. The arc lighting has been taken in hand there in a scientific way and everything has been done to obtain the highest pitch of efficiency. But what do we find in connection with high-pressure gas lighting at Manchester? Mr. Mitchell has asked what was the reason of the difference between the state of affairs in Manchester and in London. The reason of the difference is brought out all through the paper. It is the excessive leakage which is shown to have existed at Manchester. As to the candle-power of the lamps at Manchester, the efficiency was far too low. I can understand that if the lamps were erected as they came from the makers' hands, and not tuned up for Manchester gas, they would have a low efficiency. We have gone to a great deal of trouble in Glasgow in having the lamps adjusted; but having done this we get an efficiency of, say, 60 candles per cubic foot of gas, and we are not quite satisfied even with that. Even after the mantles have burned a considerable time we can nearly maintain that figure. With regard to the simplicity of switching operations

and the possibility of dispensing with lamp-lighters, several of our lamps require individual attention, but we have a large proportion where the switching is done automatically. Automatic switching can be carried out in great detail in any way desired. We have experimented with many systems, and some of the later ones are simplicity itself, both in erection and in working. On page 633 the running costs and the construction costs have been taken out very carefully, but I notice it is stated in one of the tables that the outlay was very heavy. I see that the actual cost was about £30 per arc lamp and £48 per gas lamp. I do not know how the money was spent We should want four or five gas lamps for that amount here. I notice that there is nothing allowed for gas mains. No doubt there is also nothing put down for cables on the electric lighting side, so that the one balances the other. There are rather serious omissions, however. Mantles are stated to cost 1s 1d. each. We are paying 6s. to 8s. per dozen, and we get good value at that. I am glad to see that the authors take a sensible view with regard to the testing of street lighting. The present aim is to obtain readings on a horizontal plane 3 ft. 3 in. from the ground. I hope the authors are satisfied with that. I know one electrical engineer who takes his readings at a height of 6 ft. Why should he not go to 12 ft., and gas would then not be seen at all? I think the street level for the horizontal test, or 3 ft. above for the vertical test, is high enough for any practical purpose. I have no objection to the statement that the vertical test may be made at 4 ft. or 5 ft. from the ground. I am glad to hear that opinion, because the point was brought forward in a discussion in London on street lighting. The system proposed was all against the lower-placed gas lamps and in favour of flame arc lamps, which are usually placed on tall poles. Mr. Wilson.

Mr. J. A. Robertson: I am surprised to find that lowering gear was not adopted for the arc lamps in Manchester. I can, however, understand that there are difficulties in obtaining satisfactory lowering gear for gas lamps, but it is just here that electricity should score. If the installation consisted of 700 or 800 lamps, instead of 16, the matter would, of course, be more important. I note the authors' statement that gas mantles deteriorate about 50 per cent throughout their useful life, and this confirms my own experience. It would be interesting, however, to have any particulars regarding the total life of the mantles on which these tests were made, as the life may have had an important bearing on the result, and would account, partly at least, for the discrepancy between the actual candle-power and the rated candle-power. The most outstanding fact brought out by these tests is, I think, the variations which can be obtained in the candle-power in any given direction by the use of special globes and shading. I believe that certain local authorities, in drawing up street-lighting contracts, specify a certain angle, and in some cases two angles, at which a given candle-power must be obtained. With special globes it would be easy to obtain any desired result of this kind, although the general illumination might Mr. Robertson.

Mr. Robertson.

be far from satisfactory. To my mind an illumination test over a large area is preferable to a candle-power test for street lighting. I do not agree with the authors in their conclusion regarding the undesirability of a uniform lighting effect. Our ideal should be the nearest approach to daylight that is obtainable, and it seems somewhat absurd to use these high-candle-power units in such a way that it is necessary to provide dark patches between them to prevent one's eyesight from being strained. High-candle-power units may be necessary meantime in business thoroughfares, but for better-class streets and public squares the correct system would be to employ low-candle-power units placed at short distances from one another and fixed on standards of artistic design. It has been suggested that the real costs of arc lighting have been underestimated in the paper. On the contrary, I think the authors have rather erred in overstating the costs. The capital cost per lamp appears extremely high, and a street-lighting load would easily justify a lower price for energy than is allowed for. If a more extensive scheme were to be introduced in Manchester, I have no doubt that the capital cost per lamp would be reduced, with a corresponding decrease in the standing charges. It is to be hoped that the careful and impartial tests which the authors have made and published will provide an effective answer to the claims that have been made in recent years for high-pressure gas lighting.

Mr. Langlands.

Mr. S. B. LANGLANDS: I am placed to-night in a position somewhat akin to that of a referee, and standing, as I do, between the Gas and Electricity Departments in Glasgow, I propose to offer little or no criticism and to maintain my neutral position. Some remarks have been made about the deterioration of mantles. Many tests of mantles have passed through my hands, and I know of figures of only 17, 18, and 19 per cent deterioration at the conclusion of 1,000 hours' burning. I am not a believer in tests taken in the street except in so far as they are relative tests. Many things tend to upset accurate photometric reading in the streets. The colour of the buildings—grey, white, red, or black—a wet night, a glossy surface on the streets, a fall of snow, in fact there are a hundred and one conditions which may prevail to preclude an accurate test being made. The Glasgow Street Lighting Department are now equipping a laboratory for the purpose of making tests of every kind of gas and electric lamp that is placed on the market, and I prefer tests of this kind to any made in the streets. Mr. Robertson mentioned the centre lighting of streets. I am glad he raised that point, because in Glasgow we met the difficulty that he had to contend with by staggering our suspended lamps at the lower end of Bath-street.

Mr. McWhirter.

Mr. W. McWHIRTER (*communicated*): There have been marked improvements in street lighting since the introduction of electric arc lamps of a practical type in 1878; and all such improvements in street lighting have been due to the introduction of electric arc lamps. Public gas lighting has certainly improved greatly along with electric lighting, but the introduction of the metal filament lamp for small lighting units,

and of the flame arc lamp for large units, has again left gas lighting far behind. The experimental work on the various forms of globes is most interesting, and the improved distribution of light amply repays the time spent on these improvements. An accuracy of 3 per cent is claimed for the photometric measurements, but it is very doubtful if anything approaching this accuracy is possible; as, however, the same arrangements were used for testing both the gas and the electric lighting, the comparison between the two systems should hold good. The failure of arc lighting to penetrate a fog has often been referred to, and there is no doubt that with plain cored carbons the light was very deficient at such times. With the flame arc lamp, however, a great improvement has taken place in this respect, and fog now seems to have less effect on flame arc lighting than on gas lighting. Mr. McWhirter.

Messrs. S. L. PEARCE and H. A. RATCLIFF (*in reply*): Several speakers have alluded to the absence of lamp traversing and lowering gear from the Portland-street installation, and have emphasized the great saving which can be effected by the use of this gear in place of tower wagons for trimming purposes, etc. Whilst the value of the economies that can be obtained by using traversing and lowering gears is quite appreciated, those who know the relative positions of Portland-street and Dickinson-street generating station will appreciate our problem, which was to balance the extra capital expenditure on lowering gear, etc., against the possible saving of time and labour taken up in trimming. Another point which weighed with us was the extreme difficulty of getting the necessary permission from property owners to attach the overhead gear to the walls of their buildings. In Manchester we have no compulsory powers in this direction. We are there entirely on sufferance, and every tenant can refuse (if he thinks fit) to allow us to attach our rosettes, lowering gear, winches, etc., and it was with the greatest difficulty, in certain instances, that we obtained the necessary permission to attach even the ordinary rosettes, without all the other fixings which are incidental to lowering and traversing gears. The close proximity of Portland-street to Dickinson-street renders it a very easy matter for the tower wagon to be run out and the lamps attended to in a minimum of time. In the event of further extensions of street lighting being secured, the saving incidental to the use of lowering gear would undoubtedly render its use imperative. Messrs. Pearce & Ratcliff.

Mr. Seabrook has raised the question of the cost of carbons; we quite agree that in the case of street lighting it is false economy to use anything but the best carbons. It is interesting to know that the Manchester cost for carbons is the same as at Marylebone.

The cost of current has given rise to many interesting points. The figures given in Appendix A simply show the actual cost to the Electricity Department, there being nothing added to these figures by way of profit. At the same time it is only fair to point out that these figures are very ample. Certain items might very properly be ruled out, as having nothing to do with the cost of a street-lighting load. This course, as a matter of fact, was taken by the Gas Department in com-

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piling their figures. Mr. Seabrook raises the point as to whether 16,821 kw., representing 70 per cent of the lighting connections, is not too high a figure to take. It should be borne in mind that the business lighting load very largely predominates in Manchester, that is to say, the ratio of the business lighting connections to the total lighting connections is a very high figure, and we do not think that to take an all-round average of 70 per cent gives too high a value; but again, if it is too high and should be reduced, then so much better the case for the street-lighting costs.

Alderman Walker asks why any "loading" should have been added to the figures which appear in the published accounts of the Electricity Department. The total which appears in the paper is £447,498, as against £407,498 in the published accounts. This difference is accounted for by an assumed contribution of something like £15,000 to a reserve fund which was not made last year, but in succeeding years we hope to be able to do so; and also a supplementary contribution of £25,000 in respect of depreciation. The underlying idea being to present the gas and electricity accounts on an equal footing as regards financial stability.

Mr. Taylor pointed out that differentiating between the fixed charges for lighting and power respectively might be open to criticism. On the other hand, he clearly shows that without this differentiation the increased cost of current only adds 25 per cent to the cost of the all-night lamps and 33 per cent to that of the 11-o'clock lamps. As there still remains such a wide margin between the cost of gas and of electricity, the point put forward by Mr. Taylor will not in any way seriously challenge the superiority of electric lighting on this score.

Mr. Robertson, on the other hand, thinks that the cost per unit has been over-estimated, and that the capital costs per lamp installed are high. With the former opinion we are not disposed to differ, we have intentionally erred on the high side. As regards the capital cost per lamp, doubtless this would be materially reduced if a more extensive scheme were to be introduced in Manchester.

Mr. Harrison, speaking on behalf of the electrical interests, on the one hand, and Mr. Wilson, on behalf of the gas interests, on the other, have made some interesting remarks on the question of gas pressure in Manchester, London, and elsewhere. The pressure in Manchester is 55 to 60 in. (water gauge). This is substantially lower than the pressure used in London. We understand that a pressure of 80 in. (water gauge) was at first used in Manchester, but was subsequently reduced on account of leakage. Mr. Harrison has also supplemented the paper with a good deal of very useful information, most interesting to members who have not had an opportunity of perusing the joint report presented by himself and Mr. Abady. The question of the candle-power obtainable per cubic foot with high-pressure gas is probably one of the most debatable points in connection with the subject. In this connection the figures quoted from Dr. Bloch's work in Germany, which are in close agreement with Professor Morris's published results, are very interesting and corroborate very closely the Manchester test

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results. On the other hand, Mr. Wilson still maintains that with the lamps properly adjusted he has no difficulty in getting an efficiency of 60 candle-power per cubic foot of gas, and he further states that this figure is maintained even after the mantles have been in use for a considerable time. Presumably these are the laboratory guaranteed figures, as we note from Mr. Langlands' remarks that the Glasgow Street Lighting Department is not in favour of, nor does it believe in, tests taken in the streets, except in so far as they are relative tests. In the light of this statement Mr. Wilson's results must be discounted. Electrical engineers have never disputed the fact that with properly adjusted lamps a figure of 60 candle-power per cubic foot can be obtained under certain conditions; but this figure has been used in a somewhat unfair fashion by the gas authorities. Only recently it has been stated on high authority in the gas world that it is not safe to reckon on more than 40 candle-power per cubic foot of gas consumed being obtained under average outdoor conditions. We agree with Mr. Harrison that there is a good deal to be said in favour of not extinguishing the street lamps at 11 o'clock. In large cities it is not unlikely in the future that, with a view to improving the traffic conditions, a good deal of heavy haulage which is now done in the day-time will be carried out at night. This will particularly be the case in large commercial cities, in which case it will be necessary to keep up the standard of illumination in the streets during the night.

Mr. Bailey is rather critical in his remarks with regard to the blackness of the Manchester buildings. We agree that black smoke should be got rid of first of all, and it is in this direction (though apparently not known to Mr. Bailey) that both the Manchester gas and electricity departments have, and are doing, so much. The former by the substitution of gas fires for coal fires, and the latter chiefly by the substitution of electricity for steam drives. The industrial power load on the Manchester mains exceeds that of any other municipality. Mr. Bailey appears to think it undesirable that so many details in connection with the Manchester experimental work should have been given in the paper. So far as we know, investigations along the line pursued by us have not previously been published, and we venture to think that progress can very frequently be made by the publication in papers not only of successes but also of the failures leading up to those successes. Whether all that we have published was well known before or not may be a matter of opinion, but if so then it is surprising that so many makers are content (as Mr. Harrison pointed out) to send out lamps which work satisfactorily from the mechanical feed-point of view, but which give their most efficient light at angles quite unsuitable for street illumination. Mr. Harrison further points out that the indiscriminate way in which engineers have installed such lamps, without making any scientific tests, has certainly done harm to the progress of electricity for street lighting. Saying this, we are not unmindful of the very valuable work which Mr. Bailey himself has carried out in Cheapside (London). Each locality has its own problems, and each engineer

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probably prefers to work along his own lines of development. With regard to the effect of the process by which the grading is secured, Mr. Bailey and Mr. Angold have raised doubts about its influence on the skin of the globe and the permanency of the graded flashing. We have had these globes now in use for nine months and there is apparently no diminution. The flashing cannot be scratched or washed off with soda, and we believe it to be as permanent as the globe itself. Of course all inner and outer globes slightly etch in damp weather. This really determines their life under ordinary Manchester atmospheric conditions; which life we put at about eighteen months.

Mr. Jackson asks how it is that the nominal candle-power of the gas lamps is given at 4,500, whereas the actual tests only show that half of this value has been obtained. The answer is probably three-

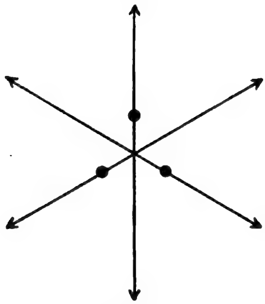

FIG. A.

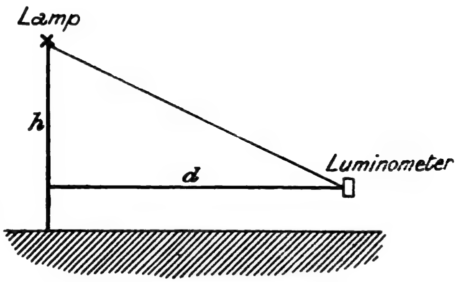

FIG. B.

fold. First, the over-estimation on the part of the gas authorities of the candle-power obtainable per cubic foot of gas. Second, the low working pressure in Manchester as compared with London; and third, the fact that even assuming it is possible to get 1,500 candle-power from a single high-pressure gas mantle, it by no means follows that 4,500 candle-power is always obtainable from three mantles in the same lantern, because there will probably be a certain masking effect. If the three burners are so fixed as to form the corners of a sufficiently large equilateral triangle, it will be possible to obtain the combined candle-power in six directions (see Fig. A).

On the question of fog penetration there appears to be a considerable difference of opinion between gas and electrical engineers. Professor Morris asks if we have any measurements by which we can corroborate our statement that the electrical flame arc lamp is superior in its penetrating power to the high-pressure gas lamp in foggy weather. We made several experiments during the month of February on the occasion of severe fogs in the city, and our results in favour of the electric arc lamps were confirmed by observations made by officials of the Tramways Department. Mr. Cunliffe, speaking on behalf of the Tramways Department at Manchester, stated that he had made careful tests by observing the distance at which the

light becomes invisible to the eye, and when tested by this method the arc lamps were found to be far more penetrating than the gas lamps. Several other speakers from different parts of the country have also endorsed this opinion. It is an admitted physical fact that a reddish-yellow light is the best for fog penetration. The greenish light of the gas lamps is admittedly superior for penetrating a clear atmosphere, but it is in foggy weather that the light is most required, and under these conditions the more refrangible rays at the green end of the spectrum are much inferior to those of longer wave-length nearer the red end.

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The test card used in the luminometer was fixed at an angle of 45 deg. to the tubular opening, and was therefore always at the same angle to the light rays. The luminometer tests had only just been commenced when the paper was written, consequently very little information had been obtained. The figures given indicate the superiority of the arc lamps, but as the actual candle-power of the gas lamps at these large angles of incidence was not known, the figures are necessarily rather indefinite. Attempts were made to obtain comparative observations during a dense fog, but unfortunately the fog was of such a rapidly shifting nature that the figures obtained could not be regarded as reliable, although apparently favouring the arc lamps.

In order that tests might be taken nearer to the lamps, and consequently at more acute angles of incidence, at which the average values of the candle-power were known, the opening in the luminometer was closed with a fogged photographic plate. The results obtained were rather remarkable as will be seen from the table on p. 676.

They either imply that the light from the arc lamps was superior to that from the gas lamps for the particular purpose of reading type, or that there had been a considerable reduction in the candle-power of the gas lamps. The atmosphere was clear at the time of testing, and presumably the fogged photographic plate, which was of a perfectly neutral tint, did not discriminate between the two sources of light in any way.

Also in reply to Professor Morris, the calorific power of the gas at Manchester is 575 B.Th.U.

We have read with very great interest Mr. Trotter's communicated contribution, and note that in his opinion from the points of view of copious illumination, uniformity of distribution, and economy, both the Gas Department and the Electricity Department have succeeded admirably. We can only add that the above were the results aimed for. It is also very impressive to read Mr. Trotter's opinion of Manchester as being the worst lighted of our great cities ; it is high time that Manchester got rid of its bad character in that respect. Mr. Trotter and others have drawn attention to the charges made by the Gas Department for street lighting, and it is not surprising that the fact that the department has obtained the same price for gas for public street lighting as from ordinary householders should have roused comment. Allusion has already been made to the fact that in arriving at the total cost of the experimental lighting of Princess-street, several

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items had been struck out of the published cost-sheet of the Gas Department, on the ground that they had no connection with the cost of street lighting. The sum thus struck out by the Gas Department amounts to no less than £158,559 out of a total of £461,667.

Results given in paper.

Size of Type.	Distance of Luminometer (d).*		Ratio of (hypotenuse)².
	High-pressure gas.	Flame arc.	
Large	339	371·5	1·2
Medium	265	313	1·39
Small	212·5	242	1·3

Later Check Results.

Size of Type.	Distance of Luminometer (d).		Ratio of (hypotenuse)².
	High-pressure gas.	Flame arc.	
Large	355	390	1·21
Medium	252	295	1·37
Small	183	220	1·44

Mean of Four Complete Tests taken with Fogged Photographic Plate.

Size of Type.	High-pressure gas.		Flame arc.		(A) Ratio of Candle-power.	(B) Ratio of (hypotenuse)².	$\frac{B}{A}$
	Actual Candle-power at angle concerned.	Distance of Luminometer (d).	Actual Candle-power at angle concerned.	Distance of Luminometer (d).			
Large...	2,200	71·9	3,000	94·5	1·365	1·67	1·22
Medium	2,050	50·5	3,350	75·7	1·635	2·045	1·25
Small ...	1,940	37·7	3,490	60·5	1·8	2·17	1·21

* See Fig. B.

Mr. Trotter has expressed the opinion that the standard of illumination in both Princess-street and Portland-street is too high, and that it savours of a spectacular display rather than of an economic experiment. After all, this is largely a matter of local conditions. As

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Mr. Trotter puts it, the Corporation are well able to judge for themselves to what extent this degree of lighting is suitable for Manchester. The density of vehicular traffic in this city between the hours of 4.30 and 6.30 p.m. is remarkable, and demands a high standard of illumination. Mr. Trotter admits that he is not acquainted with the traffic in the streets before 6.30 p.m., and therefore we venture to think that he would have expressed a different opinion if he had had any practical acquaintance of the streets between the above-mentioned hours. Several other speakers have also followed in the same strain, viz. that too high a standard of illumination has been attained, and some have gone so far as to state that there is no need for this standard. In this connection the remarks of Mr. Cunliffe, again speaking as an official of the Tramways Department, are of interest In his opinion one effect of the central lighting of Portland-street by centrally suspended lamps of high illuminating power has been the speeding-up of the street traffic. The value of good lighting cannot be over-estimated in the interests of drivers of high-power and high-speed vehicles. He attributes much of the congestion at night-time to slow and cautious driving, due to the inability of the drivers to obtain a clear view of the roadway for a fair distance ahead of their vehicles. In a valuable report recently submitted to the Manchester City Council by the Tramways Department, it is clearly brought out that owing to the congestion of the streets in the central area of the city, the average speed of the cars is so reduced, and the current consumption per car-mile so increased, as to account for no less a sum than £20,000 per annum. Whilst the authors do not intend to convey the impression that the whole, or even the major portion, of this loss is due to the bad illumination of the streets, there is no doubt of the fact that at present the lighting of the important thoroughfares in the city is poor, and tramcar and other vehicular traffic is often held up from this cause. The authors believe that the tramway authorities in many of our large cities could very much reduce their energy consumption per car-mile if the illumination of their important streets was improved. This seems to be the general answer to all those critics who do not believe in a high standard of illumination for important thoroughfares.

We agree with Mr. Trotter that the illumination of streets might reasonably be graded to suit their traffic requirements. In a recent paper read by C. L. Eshleman on "Industrial Lighting" before the American Institute of Electrical Engineers, the author gives the following figures for the desirable illumination of streets:—

	Foot-candle.
Business thoroughfares (not including illumination received from shop fronts, etc.)	0·5
Prominent streets in residental districts	0·2
Country roads	0·05

The first-named figure supports the standard aimed for in Portland-street.

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The overhead system of distribution appears to find general favour with engineers where this system can be applied.

The data given by Mr. Baker of the public electric lighting of Milan, Turin, and other cities is very interesting, as bearing out our contention that equivalent illumination can be obtained with fewer lamps centrally suspended than with the usual arrangement of side lamps. On the Continent the street-lighting authorities have got compulsory powers for the fixing of the necessary overhead work which we in this country do not possess. This will doubtless explain why so many of the streets of large cities on the Continent are centrally lighted. On the other hand, the central lighting system with high-pressure gas lamps is not suitable, owing to the complications involved and the considerable leakage which must take place in the flexible gas pipes, with consequent drop in pressure and diminution of illuminating power. Mr. Baker is of the opinion that the height adopted in Portland-street, viz. 27 ft. 6 in., is too high, and that better results could be obtained by reducing this height. This is certainly not the case, and any alteration in height would be "upward" rather than "downward." The uniformity of light distribution would be entirely destroyed if the lamps were lowered, and no possible design of reflector would counteract the effect. It is impossible to avoid sharp contrasts in the illumination if the lamps are fixed at a height which is much less than a quarter of the distance between them. In Portland-street the existing height of the tramway trolley wires would determine the minimum height at which the lamps could have been erected.

Mr. Walker brought out an important point when, on the question of reliability, he mentioned the figures of lamp failures. Of course Mr. Abady's comparison was most unfair, in so far as he was comparing 16 arc lamps against 4 high-pressure gas lamps, and half of the 16 arc lamps were burning all night against the 4 gas lamps burning only until 11 o'clock.

Mr. Burnett and other speakers have advocated the use of metal filament lamps instead of flame arc lamps. In this connection a very interesting statement was made by Mr. Burnett to the effect that the advantage lies with metal filament lamps for all cases in which a minimum illumination of less than 0·1 foot-candle is required. We have been unable to check this figure, but there can be no doubt that for street lighting the tendency all over the country to-day is for higher candle-powers, and therefore the tendency must be for flame arc lamps to be used instead of metal filament lamps. The low illuminating value of 0·1 foot-candle is entirely insufficient for important thoroughfares, and it may be stated at once that metal filament lamps would not compare in cost with high-pressure gas lamps for illuminating values in excess of 0·1 foot-candle. The authors have found from calculations that the lighting of Portland-street with metal filament lamps to the 0·5 foot-candle standard of illumination would have been at least four times as costly in comparison with flame arc lighting. It is interesting to note from the remarks of several speakers who have had extended experi-

ence with metal filament lamps that these can hold their own in competition with gas lighting at ordinary pressures. Messrs. Pearce & Ratcliff.

Mr. Harrison, Dr. Rosenberg, Mr. Dow, and others have alluded to the important question of glare. It is interesting to know that in Mr. Harrison's opinion there is a much higher intrinsic brilliancy in the average high-pressure gas lamp than in the average arc lamp fitted with a globe which obscures some 25 per cent of the light. Mr. Trotter, whilst supporting Mr. Harrison in his opinion that gas mantles have a higher intrinsic brightness, said that the glare or stress to the eye when looking straight at the electric arc is greater in his opinion than with the gas lamps, and his explanation of the fact, viz. that it is due to the use of a white reflector, is probably correct.

The discussion has certainly indicated the importance of avoiding glare, although unfortunately it has not provided any very definite suggestions as to how the desired end is to be obtained. Admitting the existence of glare, its cause or causes have never been clearly defined. The following are undoubtedly contributory conditions :—

Contrast,
Intrinsic Brilliancy,
Angle of Incidence,
Colour.

The first is probably the prime cause of glare. The second is incidental to the first; and the two last are to a large extent accidental and the results of departure from natural conditions. The relative importance is probably in the order given. Intimately connected with the fourth item is a physiological condition depending upon the accommodating power of the eye. So far as the lamps in Princess-street and Portland-street are concerned, any glare, if existent, is about equal, although the actual candle-power of the arc lamps is much higher than that of the gas lamps. Glare must of necessity be largely a matter of personal opinion, and in this respect it is interesting to note that although Mr. Trotter considers the Portland-street lamps to be more glaring than those in Princess-street, Mr. Haydn Harrison and Dr. Rosenberg consider the reverse to be the case. The importance of the angle of incidence is evident from Mr. Cunliffe's remarks concerning the high-pressure gas lamps fixed on low standards.

The point raised by Mr. Cridge is extremely interesting, and is certainly connected with glare. As a rule, advancing age is accompanied by the defect known as presbyopia, or in other words, the eye loses its power of accommodation, and is normally focused for distant vision. Those distance lights will therefore be most appreciated which have the best carrying power and give the greatest physiological effect for the least expenditure of energy, *i.e.* lamps having a greenish light. For some obscure psychical reason it is natural to prefer a warm light to a cold one, but the more red a light becomes, the greater the expenditure of energy for the same physiological effect, and consequently the

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greater the pathological effect on the eye. Fortunately the eye is better able to protect itself against these hot lights than against the more restful green lights, but probably this power of protection also decreases with advancing age, which may explain the preference of young people for reddish-yellow lights, and of elderly people for lights possessing a greenish tint. The restfulness of green is naturally the result of the wide distribution of this colour throughout nature.

It is extremely doubtful whether Mr. Trotter's suggestion for the use of reflectors on the arc lamps would be either practicable or effective, but at the same time it must be admitted that the theoretical considerations prompting the suggestion are perfectly sound, and there is no doubt that if illumination could be produced without pronounced contrasts of intrinsic brilliancy, glare would be a thing of the past.

The experiments of the authors in the direction of eliminating the effect of glare are still being continued, and at the present time it is not considered that the final and best results have been achieved. We shall all be in accord with Dr. Rosenberg in his plea for the more scientific illumination of our streets.

Mr. Burnett and other speakers have referred to the low maintenance costs for the arc lighting. We can only say that these costs are actually borne out in practice, and so far as the trimming and carboning are concerned their smallness may be explained by the fact that Portland-street is so near to Dickinson-street that very little time is lost by the attendants in going to and fro. Our figures are based on six trims per hour, or 60 per day, which can be got without difficulty.

At the same time it is only fair to point out that no heavy maintenance charges by way of extensive renewals have been included in the figures given in the paper. As regards the gas maintenance costs, the low pressure will probably account for the small upkeep of the mantles. High pressures and high maintenance charges go together. Pressure must be kept up, otherwise it would be impossible to maintain the minimum candle-power specified. In the case of the well-known Westminster contracts the candle-powers were of course guaranteed under certain penalties. The amount of photometric work daily in progress in the streets under contract is a very good testimony to the value of the penalty if the gas illumination falls below the pre-determined standard, and this also furnishes strong grounds for believing that the expenditure for maintenance in London on high-pressure gas lighting must be exceedingly high.

With regard to the table given on page 614 of the paper (this table is extracted from Mr. Haydn Harrison's report), we wish to point out that the incidence of the capital charges is excluded from both sides, as the Gas Department had omitted in their estimate of cost per lamp per annum all capital charges on the installation.

Several speakers have drawn attention to the capital costs of the two installations as being unduly high, and Mr. Wilson is especially severe on the gas costs. Attention must be drawn to the fact that nothing

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is included in the gas capital account for high-pressure mains or the compressor plant. On the electrical side, the special arc lighting mains laid account for £306. This figure is not included in the total of £564 given in the paper, but forms a part of the fixed charge of £6·133 per kilowatt connected. Mr. Burnett also thought that 8½ per cent on the capital charges per annum was insufficient. Of the total of £564 the arc lamps themselves only account for £140. These admittedly may have a somewhat short life, but the rest of the expenditure can fairly be taken over a long term of years, and therefore the equated percentage of the total is not unreasonably low at 8½ per cent. The high capital costs per lamp are undoubtedly due in the first place to the small size of the installation. The charges made by the Tramways Committee for certain erection work and materials are also on the high side. In any thoroughfare where the traffic is less, the costs could undoubtedly be very materially reduced.

Mr. Angold asks which is the final type of globe used in Portland-street. At the present time Fig. 20 shows the polar curve obtained with this globe.

Mr. Hollingsworth asks how the price of current compares with our average total cost per unit. The average total cost per unit sold to-day in Manchester is 0·55d.

With regard to the gas consumption in Princess-street, it may be taken that there is nothing included for the by-passes in the figures returned by the Gas Department. On the other hand, all leakages were included in the measurements taken. The pressure is recorded on an instrument specially fixed on the wall of one of the buildings abutting on to Princess-street.

In reply to Mr. Cramp, alternate lamps are run in series. Shrewsbury-street is not in the Manchester area of supply, so no information is available with regard to the high-candle-power metal filament lamps used therein for street lighting.

With regard to the type of cable used for the service branch work, it is perhaps not made quite clear in the paper that the cable used is of the type known as "Cab Tyre Sheathed"; it is believed to be the first installation in which this class of cable has been used for public lighting. This type was chosen because of the likelihood of trouble with ordinary taped and braided rubber cables from condensation if used with iron piping protection, and also because of the severe strains that the cable would be subject to when drawn in around the numerous curves. The corroding influence of the Manchester atmosphere on ordinary armoured or otherwise protected cables is also a point that had to be borne in mind.

We think Mr. Solomon is quite justified in sounding a note of warning to the effect that it is not any benefit to create the impression that flame arc lighting has a "walk-over" as against high-pressure gas. There is much to be said for both systems, and in our opinion it will be for some time to come a matter of the keenest competition between the two. On the other hand, we must point out that so far as the

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Manchester results can be taken as any guide, assuming electricity costs twice as much as it is stated to cost in the paper, or assuming that the gas lamps were twice as efficient as they actually were found to be, still the final result is in favour of electric lighting from the point of view of £ s. d., for approximately the same degree of illumination.

We are quite in accord with Mr. Crocker's remarks regarding the importance of a steady feed, which we think is after all the chief influencing factor on the steadiness of the arc, and that it is not a question of the correct type of globe as stated by Mr. Bailey. We see no reason to alter the opinion stated in the paper that on the whole the high-pressure gas lamp is a steadier light than the arc lamp, although it requires the photometer to emphasize the fact. Mr. Crocker's remark regarding the capacity of the lamp magazines to accommodate as many pairs of carbons as possible is quite in accord with our own views on the subject.

Many speakers have alluded to the question of the deterioration of the mantles. Here again great differences of opinion were expressed. Whilst everybody admits that there is a very considerable deterioration (which is probably greater as the pressure is increased), Mr. Langlands and Mr. Wilson put the value not higher than 20 per cent, whilst electrical engineers are generally agreed that the figure is nearer 50 per cent. No doubt this depends largely on local conditions. For instance, the nature of the street traffic has a great deal to do with it, and in this respect Manchester will be a bad city for gas lighting, having regard to the exceedingly heavy street traffic. Again, heavily smoke-laden and foggy atmospheres appear to have a bad effect on the ordinary high-pressure gas mantle.

The authors are particularly gratified with the contribution made to the discussion by Mr. Wilson, the Gas Engineer of Glasgow, and also for his many remarks on the paper. A considerable amount of work has been spent upon it, and they are glad to hear that it may be of considerable use to many engineers interested in high-pressure gas lighting.

The authors consider that the central lighting system has no disadvantages from the point of view of the drivers of tramcars or of ordinary vehicles. The advantage of staggering suspended lamps is not apparent. Traffic never takes the exact centre of the road, and consequently drivers are far more likely to be dazzled by lamps suspended nearer the side of the road than by centrally suspended lamps.

Professor Schwartz raises several important points relating to the design, construction, and calibration of a portable photometer. Fortunately our instrument was free from most of the defects and errors which he mentions. A narrow adjustable slit, used under the conditions he describes, would probably constitute a source of considerable error, and it was mainly for this reason that extreme care was taken in the adjustment and subsequent calibration of the photometer. In order to avoid very narrow openings of the slit, three separate ranges of adjustment were provided. For high values of illumination a lens was interposed between the slit and the photometer head; for medium

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values the lens was removed; and when measuring very low illumination a resistance was connected in series with the sub-standard lamp. In practice the resistance was only required when testing ordinary low-pressure gas lamps. Back-lash on the micrometer screw is practically non-existent, as the screw is always held tightly in mesh by means of a spring. The zero of the micrometer scale was always checked before commencing a test, and doubtful readings on the scale were confirmed by observations taken with the shutter opening and closing, *i.e.* with both directions of screw travel.

The law of inverse squares does not affect a photometer such as ours, which is calibrated empirically, and in which the standard lamp is a fixture. Errors might be introduced during calibration; but these were reduced to reasonable limits by having the standard source of light as small as possible, and limiting its distance from the photometer to at least four or five feet. No attempt was made to obtain an academic degree of accuracy, and the 5 per cent limit adopted was sufficient for the requirements of all practical work, and was moreover a limit which was shown by careful checks to be easily maintainable. Reflection from the black paint used in the photometer is of no consequence, and if it were possible to maintain a clean surface, the interior might just as well have been painted white. The mean illumination curves were deduced from the mean polar curves and the mean distances between lamps, as suggested by Professor Schwartz. The contour curves were also calculated from the mean polar curves. In some cases the mean curves given in the paper are the result of nearly 1,000 observations.

Several speakers have referred to the standard height for tests of outdoor illumination, and we are pleased to observe that Professor Schwartz, Mr. W. R. Cooper, and Mr. A. Wilson agree with us regarding the drawbacks to the adoption of the 3 ft. 3 in. standard. It is necessary in this respect to avoid confusion between the actual testing level and the level used as a basis of comparison for illumination on a horizontal plane. If the illumination on the horizontal plane means anything at all, it means the illumination on the *ground level*, and all figures should be converted to this basis irrespective of the actual height of the testing plane. The ground level is the only horizontal plane at which the measurements provide a common basis of comparison for all lamps irrespective of their heights. If the design of the photometer admits of measurements being taken actually on the ground level, so much the better; but if not, it becomes necessary to select a suitable height for the test plane. 3 ft. 3 in., or one metre, happens to be a convenient standard of length, and is also a suitable height for one or two illumination photometers, but possesses no other advantages. Such a height is in many respects inconvenient to work at, and 4 ft. 6 in. to 5 ft. is far more convenient, and possesses the additional strong advantage that measurements taken at this height on the vertical plane are directly applicable to the light rays which are mainly effectual in illuminating the features of pedestrians and also

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vehicular and other objects at eye-level. For interior work the illumination on the horizontal plane is admittedly required at a height of from 2 ft. 6 in. to 3 ft. 6 in. from the ground.

Mr. Bailey considers that the most important function of a properly designed globe is to secure a steady arc. It is certainly one of the functions, although not necessarily the most important, and in this respect the inner diffusing globes used on the Manchester lamps are quite as good as if not superior to dioptric globes. Despite Mr. Bailey's strong championship of the flame arc, it cannot be denied that such a lamp does not, and from the very nature of its action cannot, give such a steady light as an incandescent gas lamp, although at the same time it will readily be admitted that the light is sufficiently steady for all practical purposes. Certainly it cannot be improved by the use of dioptric globes, and, in fact, unsteadiness is probably more noticeable with refracting than with defracting or diffusing globes.

Mr. Edgcumbe is apparently in complete agreement with our frequent use of illumination on the horizontal plane as the criterion of the effectiveness of the general illumination in a street. In most cases, minimum illumination on the ground level and the variation factor probably provide all the data necessary for determining the value of a scheme of lighting. The illumination on a vertical plane is also frequently of considerable importance, and fortunately is usually sufficiently good if the corresponding illumination on the horizontal plane is satisfactory. Many of the results were given on a horizontal plane 3 ft. 3 in. high, but only because this has become a more or less recognized standard for such measurements. Despite Mr. Edgcumbe's claim for the metre level, the advantages, both theoretical and practical, in favour of a ground level standard appear to be overlooked.

It is interesting to hear that the definition of "horizontal illumination" is perfectly correct, as the drafting of a trustworthy definition in view of the diverse views on the subject was by no means an easy matter. Having admitted the correctness of the definition, why expect the authors to state more definitely what they mean? It was certainly not intended to suggest that illumination was in any way a variable depending upon the nature of the ground surface. Throughout the paper it has been clearly indicated that what was referred to was the impressed illumination, or, as it is known in England, merely the illumination. More than one English authority has within quite recent years confused impressed light and reflected light or surface brilliancy, and in America the latter is still regarded as the real illumination, although the English conception is now used to a large extent. Much confusion would be avoided if illumination (as now recognized) were regarded as impressed illumination, and the reflected light or surface brilliancy as the resultant or effective illumination. Mr. Paterson's coefficient of reflection is apparently somewhat similar to Lambert's *albedo.*

The suggestion that horizontal illumination should be calculated from the candle-power measurements, was perhaps to some extent due

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to the fact that a candle-power photometer was used, but the chief reason for making it was that illumination photometers are at a great disadvantage when measuring low values of illumination and with large angles of light flux incidence. Candle-power photometers give true values for the impressed illumination irrespective of the nature of the buildings and stray light from shop windows and other extraneous sources. The impressed illumination is what is paid for, and is therefore what should be measured; any extra illumination due to reflection, etc., may be regarded as "unearned increment." It would be very risky to reckon on such reflection when laying out an installation, and by no means easy even if the *albedo* of the particular street were known. In the case of Portland-street the difference between the impressed illumination and the actual illumination would be extremely small, as the reflection from the buildings is certainly negligible.

The flicker photometer has its good and bad points. No difficulty has been experienced when using it for measuring values of illumination within the limits obtainable in ordinary street work. It appears to be indispensable when comparing lights of different colours. Flicker photometers are tiring to use, and no really satisfactory explanation of their physiological action has yet been given. Experience has shown that different observers obtain more consistent results with the flicker than with the grease-spot photometer when comparing lights of different colours. The comparative photometry of different coloured lights is at the best unsatisfactory, and a really satisfactory solution of the problem is a matter for the physiologist rather than the physicist.

The suggested "foot-candle-yard" is not a very scientific unit, but it is certainly a simple and convenient one for comparing the cost of different lighting schemes. Strictly speaking, comparison should be on a "foot-candle area" basis, and it would therefore not be fair to take the average ordinate of an illumination curve, since the areas actually illuminated are the bases of cones of which the lamp forms the apex.

Professor J. T. Morris and other speakers raise the question of the effect of fog on incandescent gas mantles. There is apparently an actual reduction in the candle-power of the gas lamps during foggy weather. The effect is similar to that so noticeable in the case of low-pressure gas lamps, but of course in a much lesser degree. The physical changes taking place in an incandescent gas lamp are somewhat complex, and consequently the actual cause of the diminution in candle-power rather obscure; but it is probably due to a slight reduction in the thermal efficiency of the burner, in consequence of which the radiating power of the mantle is reduced to a very appreciable extent.

We do not recommend the use of translucent globes, mentioned by Mr. Dow. Some of our experiments with various combinations of globes appear to have given results very similar to those of Sweet referred to, particularly the combination from which Fig. 18 was obtained.

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We certainly had no intention of resurrecting the "Purkinje" bogey, although casually referring to it. Up to a few years ago the "Purkinje" effect was always brought forward by authorities on the subject as one of the main reasons why it was impossible to give an absolute figure for the candle-power of an arc lamp; although probably angular variations of candle-power and departure from the law of inverse squares were of far more importance. However, the "Purkinje" effect did indicate certain physiological phenomena, and it is therefore natural to suppose that if such effects were existent in the case of the flame arcs and the high-pressure gas lamps, they would be of the nature indicated, and this is further supported by the relative sensitivity of vision for orange and green light at high and low values of the illumination. With illumination of the order of one foot-candle, the sensitivity for the two colours is approximately equal.

Mr. Dow and Professor Marchant state that the luminometer is not a reliable criterion of the relative values of the illuminations tested. It is, however, probably quite as reliable as photometer tests where colour differences are concerned, and one eminent authority states that it is the only reliable ultimate criterion. In the case of our tests, however, we had a 23 per cent margin to cover discrepancies arising from abnormal physiological conditions or undefinable psychical effects. Mr. Trotter's opinion of our photometer is not exactly complimentary. Much of his criticism may be regarded as reasonable, as the instrument was admitted to be in some respects a compromise, having been modified from an earlier model. The fatigue was largely the natural result of using the telescope necessary with a flicker photometer. So much is admitted, but we cannot agree that the work actually undertaken could have been done with any of the other instruments on the market, and certainly not with illumination photometers. All accurate scientific measurements involve, as a rule, tiring observations and more or less complicated calculations. Much of the success of the work undertaken depended on the care and accuracy with which the polar curves of the lamps concerned were obtained, and for this purpose the photometer used was eminently suited. Systematic methods reduced the incidental calculations to comparatively simple operations.

Mr. Trotter questions the desirability of uniform illumination, although not giving any reasons for doing so; the consensus of opinion expressed during the discussion, however, has been distinctly in favour of uniform illumination. Perfect uniformity is admittedly unnecessary, and in fact even undesirable, but a variation factor of 15 is in our opinion too high.

The choice of a suitable size for the globes is not such a simple matter as might appear at first sight, and it is at best a compromise, since the features affecting the design and operation of the lamps require as much consideration as the question of light distribution. Large globes would have altered the polar curves of the lamps, and the resulting distribution of light to an appreciable extent, and the

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necessary modification of the curves by means of graded frosting would have been by no means an easy matter; further, it is extremely doubtful whether the results obtained would have been possible with large globes.

Large globes certainly have an imposing appearance, but that this advantage is more than outweighed by their disadvantages is obvious from their gradual disappearance in recent years.

The following are a few of the practical disadvantages: increased cost; increased risk of breakage; mechanically weaker than small globes; greater surface offered to wind, resulting in dangerous swinging; unsatisfactory on flame arc lamps owing to increased cooling surface, with consequent condensation troubles.

We agree with Mr. Cooper's remarks regarding vertical illumination. Actually, in the case of objects in the roadway, only light from one source can be considered to be effective, but on the building line the resultant illumination from several sources may be obtained, just as in the case of horizontal illumination. It is precisely owing to the fact that horizontal illumination varies with the height of the poles that we advocate comparisons on the ground level, since any other level is bound to penalize the lamps fixed on the shorter poles. The reason for testing at a height of 5 ft. was clearly indicated, and was not due to any inconsistency. The coincidence of the mid-point on the horizontal illumination curves, Figs. 5, 6, and 7, was due to the fact that the slight increase in distance from the lamp to the ground level and the variations in the polar curve just about balanced the effect of the reduced angle of incidence. The economizer is a necessary evil in a flame arc lamp, and its design does not offer much scope for modification or improvement. No alterations were made in those used. The illumination curves were in every case drawn from mean polar curves, most of which were obtained from a very large number of observations taken in many cases over a period of several days, or even weeks. On one or two occasions, when the lamps were burning exceptionally well, a complete polar curve was obtained in one evening. The further information given regarding the visibility test will probably answer the points raised by Mr. Cooper. Departure from the inverse square law may be partly responsible for the increase ratio in favour of the arc lamps, but such an explanation is very doubtful.

Mr. Angold and Mr. Solomon refer to the reflecting power of the globes now used. Their claims are to some extent supported by the results of our observations and tests. The frosting has not interfered with the reflecting power to any appreciable extent, but has enabled the lower portion of the outer globes to be usefully employed for defracting purposes, thereby removing the objectionable shadows from under the lamps and modifying the polar curves to such an extent that the light distribution has been vastly improved.

With the exception of the rays between the horizontal and 10 deg. below the horizontal, it is extremely probable that the alteration in the shape of the outer globes had more to do with the improvement in the

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light distribution than had the change in the position of the arc obtained by raising the spinnings. Mr. Angold has apparently not quite understood the explanation of the splash of light from the dioptric globes. The strong beam between 60 deg. and 80 deg. to the horizontal is certainly not the natural light flux from the arc in that direction, but rather a strongly focused beam caused by reflection.

In reply to Dr. Zettel regarding the cleanliness of the globes; those on the high-pressure gas lamps were quite clean when tested, the lamps at the time being carefully nursed by the Gas Department. When testing the arc lamps in Portland-street, we took the globes as we found them, and they were doubtless in an average condition of cleanliness. Most of the purely experimental tests were made on a lamp specially suspended for the purpose, and in order to obtain consistent comparative results, it was usual to dust out the globes before commencing an evening's test.

Complete sets of polar curves with different angles between the plane of the carbons and the photometer have not been obtained, as the work involved would have been enormous. Sets of observations, however, have been taken, and the following figures obtained at an angle of 24 deg. will give a guide to the extent of the maximum longitudinal variation.

Position of Carbons.	Relative candle-power.
Broadside to photometer	1
Positive crater facing photometer	0·9
Tip of negative carbon facing photometer ...	0·82

It is interesting to note that Mr. H. T. Wilkinson advocates perfectly uniform illumination, although it has been condemned by such a recognized authority as Mr. Trotter. Incidentally Mr. Wilkinson does not suggest how his ideal is to be attained. It therefore seems probable that our advocation of a moderate variation factor offers a more satisfactory general solution and one which should be applicable to all systems. Apart from being psychically monotonous, perfectly uniform lighting is invariably the outcome of freak methods of illumination. The authors consider that the lighting of Victoria-square, Birmingham, is perhaps the nearest complete approach to uniform lighting that they have seen. The effect is certainly very fine, but they venture to think that it is simply a spectacular show, that it cannot be regarded as a commercial piece of lighting, and that the cost must be tremendous.

Mr. Cunliffe has evidently observed some of the shadows on the road surface caused by the arc lamps when fitted with the early experimentally and temporarily obscured globes, hence his misconception of the nature of the contour curves. The curves mark the limits of certain definite foot-candle values, but between any contour and the adjacent ones there is a continual and gradual variation in the intensity of the illumination. His remarks with regard to a visual comparison

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of the illumination in Portland-street and Princess-street are quite in accordance with the results of our own observations, and Mr. Trotter has also expressed the same opinion. As the minimum illumination in the two cases is of the order of four to five, it would appear that a difference of 20 per cent is hardly noticeable to the eye without the aid of photometric appliances.

Mr. A. Wilkinson refers to the canting of the globes. The existence of an appreciable amount of reflection has been claimed and admitted, consequently it is obvious that any tilting of the globe will deflect the reflected rays through an angle equal to twice the angle of tilt. Carelessness on the part of the trimmers is apparently the chief cause of the tilting, and the remedy is therefore obvious.

Mr. Locke's remarks are interesting, and his results are more or less novel. It should, however, be clearly understood that the methods which he advocates are in no sense photometric, since they are useless for comparing different light sources. Photometry must of necessity be to a large extent physiological in its nature, and all attempts to reduce it to a mere matter of physical chemistry are obviously futile. The results obtained are merely a measure of the actinic properties of the light rays. Elliptical globes were tried many years ago, but abandoned as impracticable.

The difference between the eye during the daytime and at night, referred to by Mr. Record, is merely a result of its enormous power of adaptation to changing light conditions. Intensity of illumination is purely a matter of contrast, and consequently the flame arc lamps, if lighted during the daytime, are hardly noticeable, but that does not prevent them giving a magnificent degree of illumination after sunset.

We are familiar with the "Trotter" photometer, referred to by Professor Marchant, and also others of a similar type. They are essentially illumination photometers, although it is possible to use some of them for candle-power tests. We have never used photometers of this type and do not consider them to be so suitable for the work undertaken as those of the candle-power or focusing type, such as the modified Simmance-Abady instrument which we used.

Professor Marchant objects to the term "variation factor." "Diversity factor" was obviously a bad choice, as the term had already been appropriated for another quantity, and "diversity coefficient" is likely to cause confusion owing to its similarity. The term "variation factor" has not previously been appropriated; it is practically self-explanatory, and moreover it is euphonious.

Mr. Cramp refers to the experience of the lighthouse authorities in connection with fog penetration. The explanation of the apparent want of agreement in the observed results is that our statement referred to flame arc lamps burning with a reddish-yellow light, whereas the lighthouse experiments were made with ordinary open-type carbon arcs.

Mr. Chattock refers to the absence of shadows in the case of the lamps having vertical twin carbons. This might be naturally expected,

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as the globes are presumably opalescent instead of clear, and there are no ash-trays.

Mr. Crocker's remarks are very interesting, and as they are based on actual practical experience they constitute a valuable testimony to the general correctness of the statements made in the paper. The 5 per cent limit of accuracy is quite sufficient for all street photometric work.

Mr. Solomon's reference to the agreement between his own results and ours is certainly very satisfactory. Many of our curves had been obtained before his paper was published, and we were agreeably surprised to note such comparatively close agreement between laboratory tests conducted under more or less ideal conditions and street tests conducted under anything but ideal conditions.

Mr. Taylor's "maximum variation of foot-candles per foot run" appears to be an unnecessarily complicated and not very practical quantity. If the variation factor is kept reasonably small the variation per foot run cannot be very excessive. Where the variation factor is very high Mr. Taylor's new factor might probably provide a criterion of the absence or otherwise of glare, which is largely due to marked contrasts.

The automatic centrally controlled system described by Mr. Mitchell is very convenient and ingenious, although somewhat complicated. Such a scheme involving a complete network of pilot cables is only possible where facilities for drawing-in exist; in most cases the cost of opening up the ground for the purpose of laying them would be prohibitive. We have had very satisfactory results with clock-controlled switches. It is interesting to note that Mr. Mitchell regards the "Manchester" type of globe as inevitable if competition is to be faced. The condensation of fumes when using dioptric globes has not been observed to any appreciable extent, but the globes were only used experimentally for comparatively short periods.

Mr. McWhirter has mis-read our claims for the accuracy of the photometric measurements. Five per cent was stated to be the probable limit of accuracy, and a strong doubt was expressed as to the possibility of guaranteeing results to an accuracy of 3 per cent. In any case, as he aptly remarks, the comparison between the gas and the electricity tests results would be unaffected by any errors in the absolute values of the measurements.

Mr. Langlands has a strong partiality for laboratory tests. No doubt laboratory tests have their advantages, particularly in connection with experimental work, but we consider that owing to the doubtful application of the inverse square law in many cases, and for other practical reasons, the final judgment should be based upon actual tests taken in the street under working conditions. Laboratory tests of high-pressure gas lamps are invariably quoted, and are invariably better than results obtained in the street.

Mr. Robertson favours the practice of specifying the candle-power of lamps at certain definite angles. It is, however, quite wrong in

principle, and encourages the production of freak lamps. What is actually paid for is illumination, and this should be specified. Low candle-power units may have a pleasing effect, but they cannot compete with high-pressure gas. Messrs. Pearce & Ratcliff

Several speakers have criticized the frosted globes, and compared them unfavourably with dioptric ones. Fortunately the test results provide ample vindication of the graded frosting. Professor Marchant mentions absorption, but actually there is very little as the larger portion of the globe is clear, and the frosted portion forms a defracting surface which improves the distribution; the absorption is comparatively slight, and quite negligible, since it is at an angle at which the natural polar curve of the lamp may with advantage be modified. Professor Schwartz considers that the desired effect has been very cheaply obtained at the expense of a little absorption. Mr. Bailey's remarks convey the impression that successful dioptric globes are difficult to design, and still more difficult to make; on these grounds alone, therefore, the frosted globes have a strong advantage, which is quite sufficient to balance the slight superiority of the dioptric ones in the matter of light distribution. Mr. Trotter admits the success of the results obtained with the graded frosting, and then proceeds to criticize the method. The frosted globes are admittedly crude dioptric ones, but only in so far as defraction may be regarded as irregular refraction. The analogy of the steam engine brake is hardly a tenable one, since the frosted globes greatly improved the light distribution without reducing to any appreciable extent the maximum candle-power of the lamps at angles of importance.

Mr. Dow also refers to the improvement in the distribution curve of the lamps obtained with the graded globes. It is evident from the curves that the primary object of the frosting was to remove the shadows under the lamps and reduce the intense splash of light at angles of 40 deg. to 70 deg. to the horizontal. It also follows as a matter of course that the glare was very much reduced; this point, however, is referred to elsewhere. The action of the globes used is somewhat complex and they are actually giving: direct rays, regular reflection, defraction, irregular reflection, slight diffusion, and slight absorption.

SOME PROBLEMS IN TRACTION DEVELOPMENT —TRAMWAY FEEDING NETWORKS.

By J. G. CUNLIFFE, M.Sc.Tech., and R. G. CUNLIFFE, M.Sc.Tech.

(*Paper first received 24th October, and in final form 6th December,* 1912; *read before the* MANCHESTER LOCAL SECTION, *3rd December, and before the* BIRMINGHAM LOCAL SECTION, *4th December,* 1912.)

The object of this paper is to present a suggestive treatment of the modifications which, as a result of continued development, are rapidly becoming necessary in the design of the feeding networks of many large tramway undertakings and in the legislative limitations imposed thereon.

Street traction history has shown that, with increasing traffic density and greater distances traversed, consequent upon rapid growth of the cities, the methods of application of the requisite energy have been subject to continual change, and that, even after electrification, the equipments have undergone frequent transformation, generally piecemeal, but in some cases, as at Chicago, resulting in the complete rehabilitation of the whole undertaking.

Larger and more powerful cars are continually being adopted, and are run at closer intervals. Traffic stops increase in frequency, necessitating a quicker acceleration in order to maintain the schedule speed, and the rate of consumption of energy per car is greatly increased. The net result of this increased rate of consumption and of the reduced headway is to cause a still greater increase in the intensity of electrical loading as expressed in kilowatts per mile. These effects are illustrated by the comparative operating statistics given in Fig. 1 as between various undertakings, and by the curves in Fig. 2 for a particular undertaking. The values of the ratios illustrated by Fig. 1 are average ones only, the maximum values being much greater, and bearing, in general, no relationship to them. Thus at Manchester, owing to the small extent of the "City area," the rate of electrical loading at the centre is some 1,500 kw. per mile of route, and the rate of energy consumption of a car is some 38 per cent greater at the city end than at the suburban end of a route. The intensity of electrical loading at London is very high, but is only of passing interest here, as the general case alone, with overhead principle of operation and uninsulated return circuit, is considered. Special references are made to Chicago owing to its very heavy loading, and to the fact that by the courtesy of Mr. Bion J. Arnold and the Board of

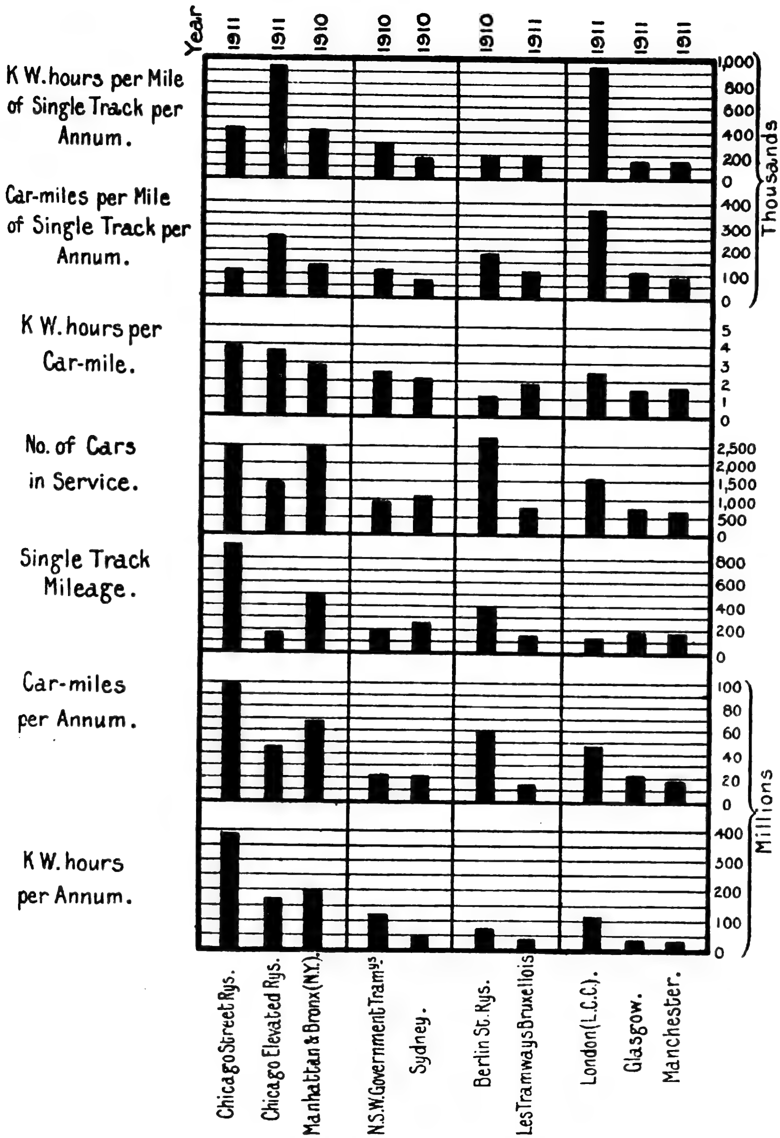

FIG. 1.—Comparative Operating Statistics.

Supervising Engineers the authors have been placed in possession of detailed accounts of the whole of the rehabilitation work.

If the number of supply stations were increased in proportion to the increase in the intensity of electrical loading, the nature of the feeding problems would remain practically unchanged, but this is not the case, the capacity of the stations increasing rapidly. Thus at Chicago several of the sub-stations are designed for an ultimate capacity of 18,000 kw., whilst sub-stations of 12,000 kw. capacity are not uncommon. These large stations are interconnected by heavy cables, which generally serve also as feeders.

It is obvious that, under these circumstances, in view of the strict limitation of the line pressure the governing factor in the feeder designs must, at some stage in the development, change from overheating

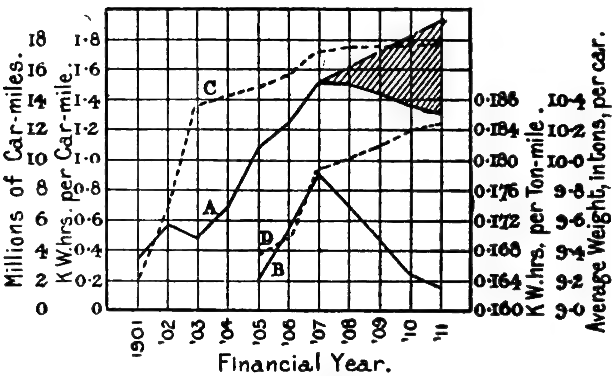

FIG. 2.—Operating Statistics for Manchester.

Curve A = kw.-hours per car-mile.
" B = kw.-hours per ton-mile.
" C = car-mileage run.
" D = average weight per car.

to pressure drop, and that boosting must ultimately be resorted to. Boosters have hitherto been adopted in isolated cases only, mainly in Great Britain, but electrical energy is very cheap in many foreign cities, and their use will undoubtedly become more common. As the intensity of loading increases, special means become necessary in order to maintain the rail drop at a sufficiently low value, and negative boosters must ultimately be adopted. The main principles underlying the use of boosters both positive and negative are therefore considered.

POSITIVE FEEDING.

With light loading and few supply stations the system of feeding illustrated in Fig. 3, A, is generally employed, and consists of feeders supported by distributors which may or may not be graded in size according to the nature of the load variation and to the special duties which they may be required to fulfil. With a large number of supply

stations, as at Manchester and Liverpool, the simple feeder system shown in Fig. 3, B, is employed, since, even with the short lengths of feeding section available, with economical distribution through the trolley wires alone, the feed points are nearer to the stations than to each other. As the intensity of electrical loading increases, the length

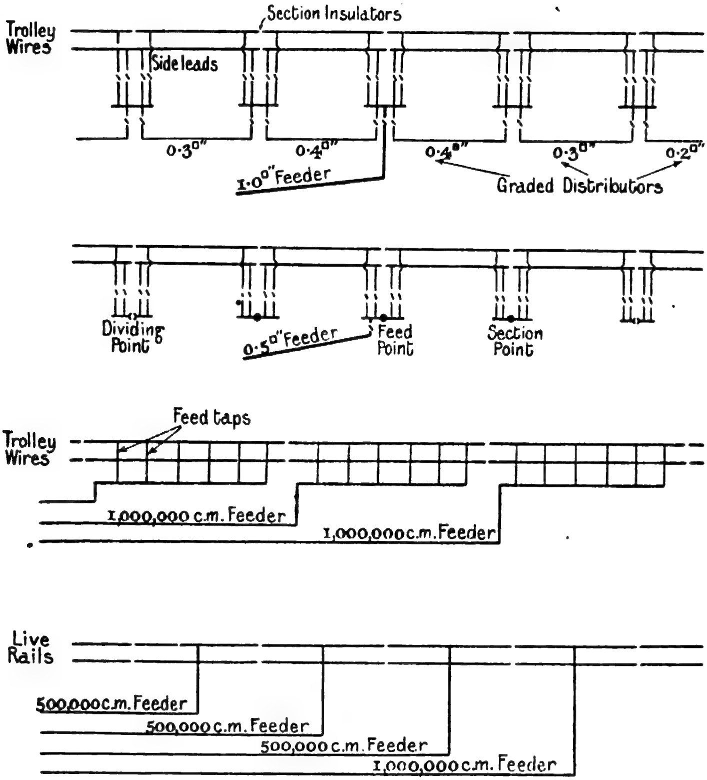

FIG. 3.—Methods of Positive Feeding.

A. British Standard with side leads and distributors (Birmingham).
B. British Standard with side leads and simple feeders (Manchester).
C. Chicago Surface Lines.
D. Chicago Elevated Lines.

of feeding section rapidly diminishes, and with very heavy loading, owing to the reluctance of engineers to employ feeders much larger than 1 sq. in. in cross-section, the simple feeder system of feeding is universally adopted, although, in many cases, with modifications, as at Chicago, where the feeder is continued throughout the whole length of the section and tapped on to the trolley wires at intervals of from three to six spans as shown in Fig. 3, C, the conductivity of the trolley wires being ignored in the design.

The conductivity of the trolley wires is a matter of importance with the feeding system illustrated in Fig. 3, B, and at Manchester their section is increased on renewal, according to the intensity of electrical loading, this being a temporary expedient until such time as the installation of additional feeders is justified by the increased intensity of loading, the rate of consumption of energy being reduced by this and similar means as shown by the shaded area in Fig. 2.

Several feeders are often operated in parallel for the purpose of preventing too frequent opening of the circuit-breakers controlling very heavily loaded sections; and, even with very short sections, it is observed that, owing to the relatively high conductivity of the feeders as compared with that of the trolley wires, the feeders do not share the load equally, the centre of gravity of the load moving over a range of quite half the length of the section. As the whole of the copper must be fully utilized, this has led to the use of heavy feeders with several short sub-feeders radiating to the adjacent feed points. This method is becoming very common, more especially for the feeding of complicated networks. It is merely the natural outcome of continued development of the distributor system of feeding.

At Chicago the length of the feeding sections situated in the immediate neighbourhood of the station is such as to load the standard 500,000 C.M. (0·3927 sq. in.) feeders to the limit of their safe-carrying capacity, but the more distant sections are of such a length as to give a limiting pressure drop of 50 volts on the same standard feeder, which, however, is doubled in size if the loading is very heavy and the distance great, the current density being greatly reduced. The methods of design have been fully described in a paper* by Mr. R. H. Rice, Assistant Engineer to the Board of Supervising Engineers, and the importance of pressure drop is shown by the fact that the equipments have been designed for a working pressure of 600 volts, which is also common to other very heavily loaded undertakings.

Overcompounding.

Little relief can be afforded long feeders by overcompounding, as the permissible pressure rise at the busbar is limited by the pressure rise which may be allowed at the feed points of the shorter feeders, and this should not exceed 10 per cent. If a pressure drop materially in excess of 10 per cent is, for economical design, required in the long feeders, therefore, it becomes necessary to employ separate positive boosters.

Positive Boosting.

It is unsafe to operate a group of separate feeders from a common booster owing to the risk of abnormal pressure rise at the feed point of a temporarily lightly loaded section, and although, by parallel operation of the feeders, this risk may be avoided, the practice leads to unequal

* *Journal of the Western Society of Engineers*, vol. 15, p. 367, 1910.

distribution of the load amongst the feeders, the shorter ones being overloaded. For satisfactory operation, therefore, a separate booster ought to be installed in each long and heavily loaded feeder, and with the heavy loading under consideration this would often be practicable. In order, however, to avoid the installation of a large number of boosters of varying capacity the authors would suggest that advantage be taken of the method already described of employing main feeders, each having several short sub-feeders. The arrangement is illustrated in Fig. 4, and the pressure drop in the main feeder alone would be compensated by the booster, so that the sub-feeder busbar would be maintained at a constant potential, the design of the sub-feeders being governed by considerations of overheating alone, and their load fluctuations not materially affecting the pressures at the feed points.

Automatic circuit-breakers with tell-tale indicators at the generating station, whence they could, if desirable, be operated by remote-control, might be installed as shown in order to confine the area affected by a feeder breakdown, and would immediately localize the fault. A somewhat similar system to that shown in Fig. 4, but without booster, is em-

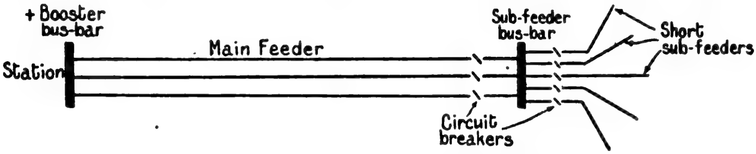

Fig. 4.—System of Feeding by Sub-feeders, with Main Feeder only compensated for Pressure Drop.

ployed on the lines of the New Jersey Public Service Railway Company, U.S.A., the circuit-breakers in this case being mounted on cross-arms on a traction pole, and fitted with tell-tale indicators to the nearest office. They would be better placed in a manhole.

Engineers are so familiar with the principles of positive feeding that novelty is possible only in their application, and hence the light treatment, but this does not apply with the same force to negative feeding, concerning which there is still considerable difference of opinion.

Negative Feeding.

The general method of negative feeding is by means of separate feeders, as illustrated by Fig. 5, B. It is impossible to divide the rails mechanically into definite feeding sections as is done in the case of the "line," and all negative feeders from any station must be operated in parallel, the difficulties encountered arising from the resulting lack of exact control of the load distribution in the rails. The conditions are exactly the reverse of those applying to the parallel operation of positive feeders, the conductivity of the four rails of a double track being equal to that of some 4 sq. in. of copper, and hence greatly superior to that of any individual negative feeder. It follows, therefore, that with light loading the amounts of current returning by the respective

feeders, if of the same size, will be almost inversely proportional to their lengths, and in order to compel the longer feeders to do their duty they are in some cases made heavier than the shorter ones. In other cases resistance is inserted in the shorter feeders.

However carefully the negative feed points may be selected the distribution of the return current amongst the feeders with very heavy loading is to a far greater extent influenced by the nature of the load distribution, and there cannot be without excessive outlay in copper

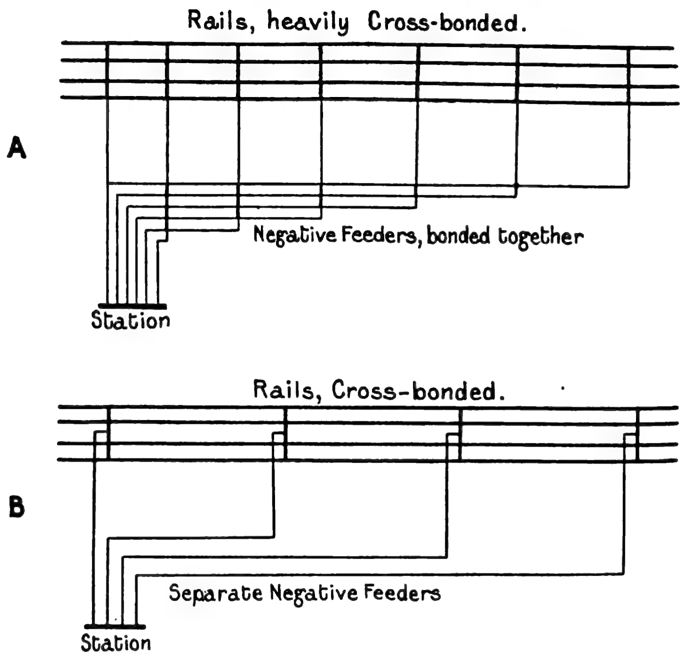

FIG. 5.—Methods of Negative Feeding.

A. Chicago Tramways. B. British Tramways.

more than one point in the track on each side of the supply station at which there is no current flow, *i.e.* the rails on each side of the station cannot be divided into more than two feeding sections in which the directions of current flow are mutually opposed, the direction near the station being outwards from it. It is merely a question of the balance of potential, etc., following Kirchhoff's laws. The balance point between the two sections can, for fixed loading conditions, be moved farther from the station by increasing the resistance of the shorter negative feeders or by reducing that of the longer ones, and vice versa, but the extent to which this can be economically done with very heavy loading is limited by the pressure loss in the feeders and

resistances, the other alternative being too expensive in copper. For a given length of track with increasing loading the maximum permissible rail drop is ultimately attained when copper must be provided to carry any additional current, and as this copper is operated at a very low current density its use is very uneconomical.

At Chicago, the 129-lb. rails are reinforced, as shown in Fig 5, A,

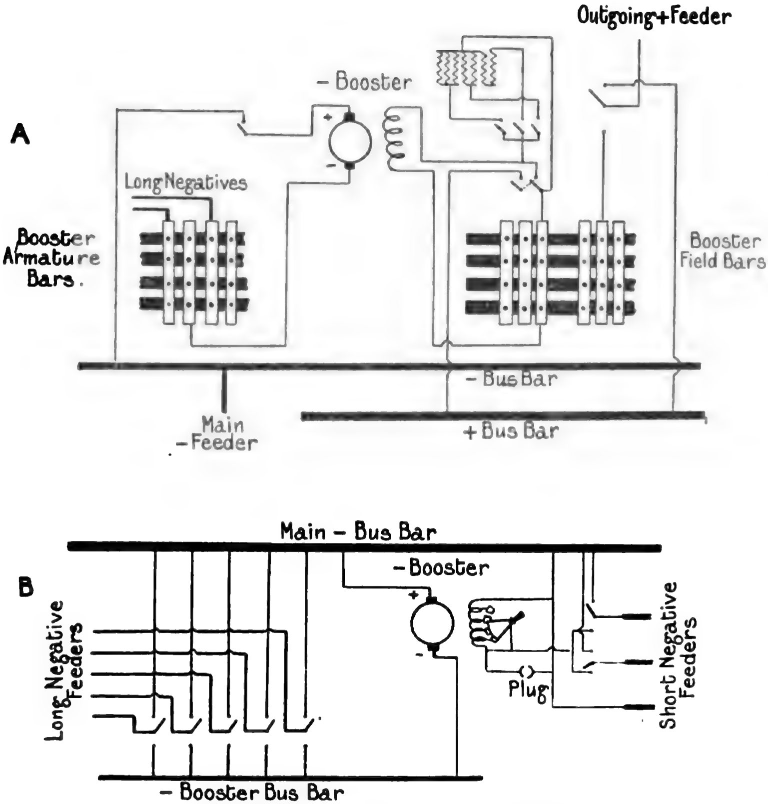

FIG. 6.—Negative Booster Connections.

A. Glasgow, Sheffield, etc. B. Leeds.

by bare copper cable increasing in section from 500,000 C.M. (0·3927 sq. in.) on the outskirts of the sub-station area to 10,000,000 C.M. (7·854 sq. in.) in the neighbourhood of the station, and even with this great outlay there is much electrolytic trouble. The direction of flow is uninterrupted, and if the bonds were cut and the cables separated, the longer ones would bring back more current.

In order to split up the track into smaller areas additional substations have in some cases been installed, but this is evidently

uneconomical, as it is contrary to the tendency of modern practice, this being, as has been stated, to increase the capacity and not the number of sub-stations. Even with a greater number of sub-stations the difficulties are only alleviated; they are not necessarily removed.

By the use of negative boosters the track may be divided into definite feeding sections almost as perfectly as is the line, and the flow of current in the rails may be controlled in a manner which is almost ideal; but in the few cases in which negative boosters are in use this has not been done, no definite principle having been adopted, the boosters serving merely as a means of compelling long negative feeders to do their duty without excessive outlay in copper, the principles underlying the application being the same as in positive boosting.

Thus, Glasgow has a negative booster mounted on the shaft of each rotary converter at the sub-stations, and provision is made, as at Sheffield, for switching any booster on to any one of the long negative feeders, or on to a group of long negatives as shown by the connections illustrated in Fig. 6, A. At Glasgow and Sheffield the negative boosters are excited from positive feeders, but at Leeds there is a large negative booster, capable of being switched on to a group of long negative feeders, excited from a shorter negative feeder as shown in Fig. 6, B, whilst several smaller boosters, used on occasions of heavy traffic, are excited from positive feeders. A negative booster has been in use for some time at Birmingham in order to maintain the feed point on a route, served by a long negative feeder, at the same potential as that on another route fed by a shorter negative from the same station, whilst the authors know of no case of the use of negative boosters in America, and of only two cases—at Dantzig and Rouen—on the Continent. The application of the negative booster at Manchester on the principle of definite sectionalization of the rails is discussed later.

Correct Principles of Negative Feeding.

In order to understand the correct principles of negative feeding it is necessary to study the effects on the flow of vagabond current of the various methods of feeding, since protection from electrolysis is the first object of the design.

With a uniformly distributed load, such as is approximately obtained with cars spaced at short intervals, the potential of the rails at any point with respect to the negative feed point is represented by a parabolic law; and if the curve of potential gradient in the rail is dropped until the two areas enclosed between it and the datum line are equal, as shown by the broken lines in Fig. 7, A, the area enclosed above the rail will represent the amount of vagabond current leaving the rail, whilst the area enclosed below the rail will represent the amount of current re-entering, the curve itself then representing the absolute potential of the rail, and also the vagabond current density at any point, the point O of

intersection being the point at which the rail is at normal earth potential.

The broken lines shown in Fig. 7, A, represent, neglecting any mutual effects, such curves of absolute potential for the two sub-sections of rail, the left-hand one of which is supposed to have (whether owing to greater length or to heavier loading is immaterial) a higher value of rail drop than the right-hand one. The point X, however, being common to the two sub-sections, must have a potential represented by a point on both curves which will accordingly be shifted to some positions such as those represented by the full lines, when it is evident that the two neutral points must be moved to the right and that there must be an interchange of vagabond current between the two sub-sections as

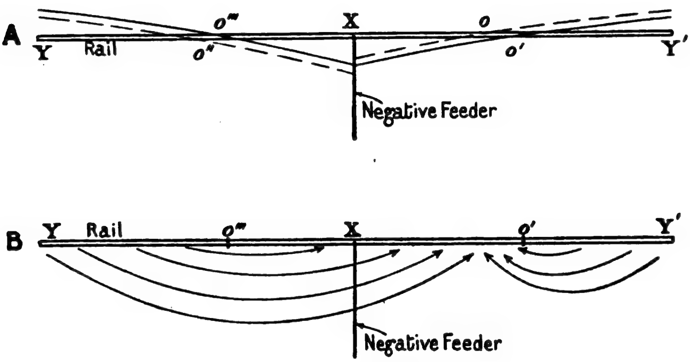

FIG. 7.—Unbalanced Sections.

In A—

Broken line = absolute potential gradients of sections considered independently.
Full line = absolute potential gradients of sections considered jointly.

A. Mutual effects of unbalanced sections.
B. Interchange of vagabond currents between sections.

shown in Fig. 7, B. The higher rail drop is reduced and the lower one increased, the tendency being to reduce their values to equality.

In considering the absolute potential conditions of any sub-section full allowance must be made for the effects of other sections at a different absolute potential, and a sub-section may from this cause be rendered wholly negative with respect to earth, whilst safety and danger zones may be situated at totally unexpected places. Evidently then it is of importance that all sub-sections should have approximately the same value of rail drop, *i.e.* that all negative feed points should be at the same absolute potential. This conclusion, based on the authors' researches into vagabond current phenomena described in a previous paper,* also forms one of the recommendations of the recent joint Commission † representing the Verein von Gas- und

* *Journal of the Institution of Electrical Engineers*, vol. 43, p. 449, 1909.
† *Elektrotechnische Zeitschrift*, vol. 31, p. 491, 1910.

Wasserfachmännern, the Verband Deutscher Elektrotechniker, and the Verein Deutscher Strassenbahn- und Kleinbahn-Verwaltungen, which will govern future German practice.

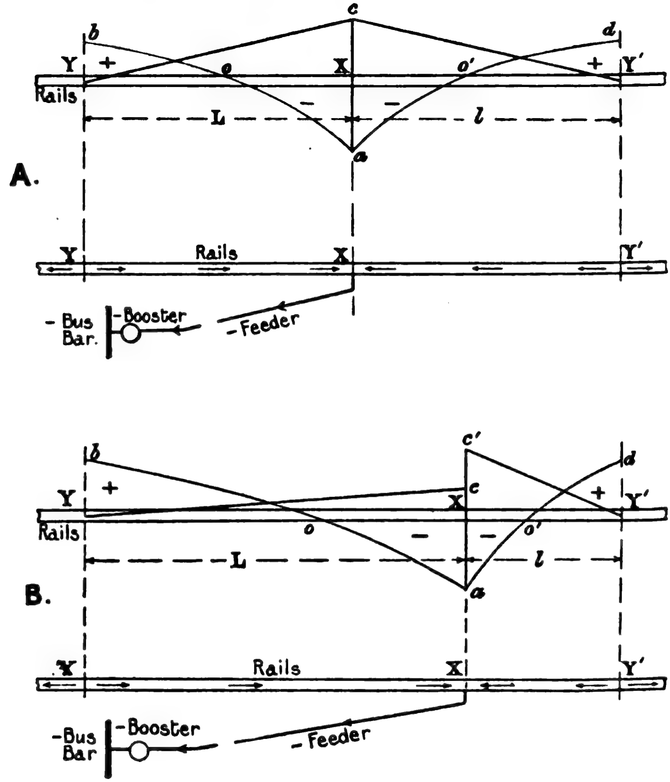

FIG. 8.—Negative Boosting Conditions.

A. Uniform Loading of Sub-sections.

Index—

X = negative feed point.
Y, Y' = artificial dividing points created by negative booster suction.
o, *o'* = neutral points.
a o b, *a o' d* = absolute potential gradients, and are identical.
Y*c*, Y'*c* = loading at any point in the rails.
L = *l* = length of sub-section.

B. Unbalanced Loading of Sub-sections.

L > *l*.
X *c*, X *c'* = loading of sub-sections.
Y *b*, Y'*d* = absolute potentials at dividing points.
X *a* = common absolute potential at negative feed point.

For a given rail drop the authors have previously shown the vagabond current to be proportional roughly to the square of the length of sub-section, so that it is of importance that such length should be a minimum.

Ideal Negative Boosting.

The general use of negative boosters alone permits of the attainment of the two ideal conditions, viz. :—

1. Uniform absolute potential at the negative feed points, and hence no interchange of vagabond current.
2. Minimum length of sub-section, and hence minimum vagabond current.

Further, full advantage is taken of the high conductivity of the track; the rail drop is easily maintained at a low value; the pressure loss in the negative feeders is no longer subtracted from the supply pressure; great potential-difference between the negative busbar and earth is avoided, and, finally, with heavy loading a very material reduction may be obtained in the amount of return copper as compared with the amount required to obtain satisfactory conditions, even if they could possibly be obtained, without boosters. The saving in cost over the installation of additional sub-stations would be very considerable.

Economy in copper, however, must on no account be allowed to influence the amount of current to be brought back by any individual booster or the number of boosters to be employed. A maximum permissible value of rail drop must be adopted, and the amount of current to be returned must be determined solely by such rail drop considered in conjunction with the maximum intensity of loading and the specific conductivity of the track, with due allowance for increase of traffic, and it is in the determination of the booster voltage that the saving in copper may be balanced against the cost of dissipated energy.

The problem is—knowing the maximum and average intensities of loading at all points along the routes—to divide each route into negative feeding sections, each of such length that its own current flowing to its selected feed point shall split it up into two sub-sections having equal rail drop opposed in direction as shown in Fig. 8, A, which illustrates the absolute potential gradients, and shows by means of arrows the direction of current flow in each of the sub-sections of rail of lengths L and l respectively, the conditions illustrated being applicable to sub-sections of equal lengths, equally loaded. This rail drop must have the same value on all sub-sections. Fig. 8, B, illustrates the conditions to be obtained in cases of unequal intensity of loading in the two sub-sections. The lengths L and l must each be such as to give the common rail drop, with their different conditions of loading, when there will be no interchange of vagabond current. The length of each sub-section may be determined by the following considerations :—

Let—

i, I = maximum intensity of loading in amperes per mile on the respective sub-sections,

R = specific resistance of track in ohms per mile,

L, l = respective lengths of sub-sections in miles,

V = maximum permissible rail drop in volts.

Then—

$$V = \frac{i \cdot R \cdot L^2}{2} = \frac{I \cdot R \cdot l^2}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\therefore L = \sqrt{\frac{2\,V}{i \cdot R}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and—

$$l = \sqrt{\frac{2\,V}{I \cdot R}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

hence—

$$L : l :: \sqrt{I} : \sqrt{i} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Typical values of the length L of sub-section and of the booster current, with two such sub-sections, are given in Fig. 9 for various

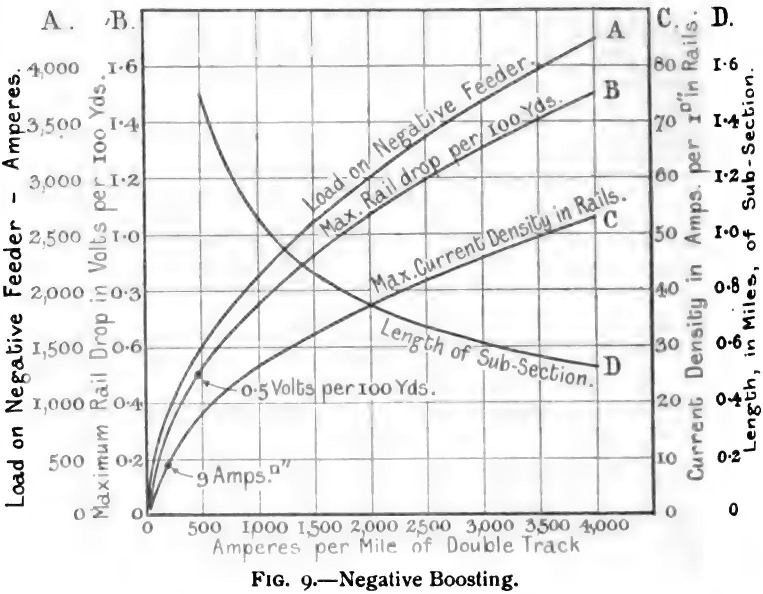

FIG. 9.—Negative Boosting.

Curve A. Total load on negative feeder—amperes.
" B. Max. pressure gradient in rails—volts per 100 yds.
" C. Max. current density in rails—amperes per square inch.
" D. Length of each sub-section in miles = L.
Total length of section = 2 L.
The curves apply to a maximum rail drop of 7 volts.

Note.—With a limit of 9 amperes per square inch the limit of loading is 200 amperes per mile. Contrast above limits with the value of 4,000 amperes per mile taken from actual American practice, and upwards of 2,000 amperes per mile from British and Continental practice.

intensities of loading with double track composed of 105-lb. rails of British standard section and corresponding to a rail drop of 7 volts. The values of current show that the boosters would be quite large enough to justify a separate one to each negative feeder with heavy loading, whilst, if a higher rail drop than 7 volts were allowed, or if

rails of a heavier section were employed, both current and length of section would be larger, *i.e.* the size of the boosters would be increased and their number reduced. The total length of section if the sub-sections were equally loaded would be twice the value shown in Fig. 9.

In practice, however, the feeders would in the majority of cases be run to junctions, when the values given in Fig. 9 would be doubled and the number of boosters almost halved, a slight sacrifice being made in the resulting conditions to effect this end. The ideal conditions could not in any case be perfectly obtained owing to imperfect distribution of the load, regulation of the boosters, etc., but the conditions actually attained would be greatly superior to the best that could be obtained in any other way. Many unboosted feeders could still be retained where favourably situated. Each booster should be excited from the grouped positive feeders serving the same area.

Combined Use of Negative Boosters and Unboosted Feeders.

The application of the negative booster is not likely to become sufficiently general to permit of the attainment of the ideal conditions,

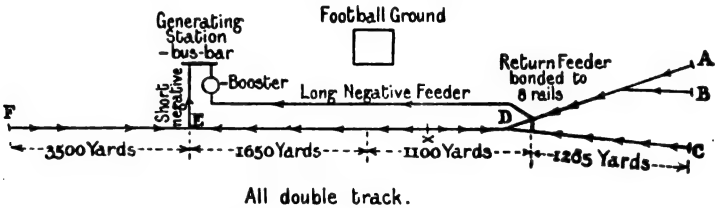

Fig. 10.—Negative Boosting.

A, B, C and F = boundaries of system.
D and E = negative feed points.
Line previously supplied by E only.

but the latter should be kept in mind. In the most general cases a booster is required either occasionally or continuously to operate a long negative feeder in parallel with shorter ones, and gives satisfaction provided the permissible maximum rail drop is not exceeded.

An installation of this kind designed by the authors to deal with very heavy football traffic is illustrated in Fig. 10. The heavy loading was experienced between the football ground shown and the boundary at A, B, C, and the rail drop between these points and the point D was found to be below 7 volts. The negative booster was so designed that an artificial dividing-point was created at X and moved with varying load distribution over a range such that it never reached the point D of attachment of the boosted feeder. In this way the section was split up into three sub-sections, the direction of flow of current in each being indicated by the arrows. The rail drop in each sub-section was maintained below 6 volts, and although the total drop from D to E could not

be reduced to zero—the ideal value—it was reduced to 2 volts as against 18 volts without the boosted feeder. This imperfect balance of potential was due to the loading of the sub-section E F, which was beyond the control of the booster. In this way alone, by splitting up the section into sub-sections with their directions of flow mutually opposed under all conditions of loading, could the rail drop under such difficult conditions be maintained below 7 volts. The practice of connecting a group of long negative feeders to a single booster would not be possible with really heavy loading, nor would that of exciting the booster from a shorter negative feeder, and in any case with such methods the track sectionalization is not definitely assured.

Superimposed Return Circuits.

The authors have known cases where return current from a section fed by long feeders from one station entered another station by its negative feeder, flowed through its generators along its trolley wires, and through the equipments of its distant cars to re-enter the former station by a shorter negative feeder on another route. This would not occur if the potential at the negative feed points were uniform, and cannot be prevented by merely equalizing the resistances of the negative feeders, although it can be minimized, since such equalizing is correct for one value only of the load distribution. With boosters it is correct for all values.

Legislation.

It has been shown by the authors in their previous paper that one regulation alone is of any use in assisting in the prevention of electrolysis, viz. limitation of the maximum permissible rail drop, and other regulations are not merely useless, and often contradictory, but may impede progress. Thus, for instance, for a given rail drop the potential difference between the rail and a pipe at any point is definitely fixed and is beyond control.

There are two regulations in particular which would effectually prevent the use of the ideal system of negative boosting, viz. the British limit of 9 amperes per square inch in current density in the rails, and the present Chicago limit of steepness of potential gradient in the rails, which must not exceed 1 volt per 1,000 ft. in the down town sections and 1 volt per 700 ft. in other places. The latter is a reflection of the old French administrative rule of 1 volt per kilometre also adopted for suburban lines by the German Commission.

The maximum values of current density and rate of fall of potential in the rail with the ideal boosting conditions, British standard 105-lb. rails and 7-volt rail drop, are illustrated by the curves B and C given in Fig. 9, whence it is obvious that the above values would be far too low. And yet the conditions in Fig. 9 represent the maximum of safety. The limitation of current density in the rails has been adopted as the most practical way of limiting the local steepness of the potential gradient to a maximum of 0·5 volt per 100 yds., so that the two regulations are really identical in nature and are based on the fear that

with a potential difference of, say, 1 volt per 100 yds., there would be a heavy flow of current in any metallic structure which happened to approach the rails at two points several hundred yards apart. It is clear, however, from the curves shown in Fig. 8 that these high values are obtained only in the neighbourhood of the negative feed points, where at no single point could current leave the rails, and the danger is purely imaginary.

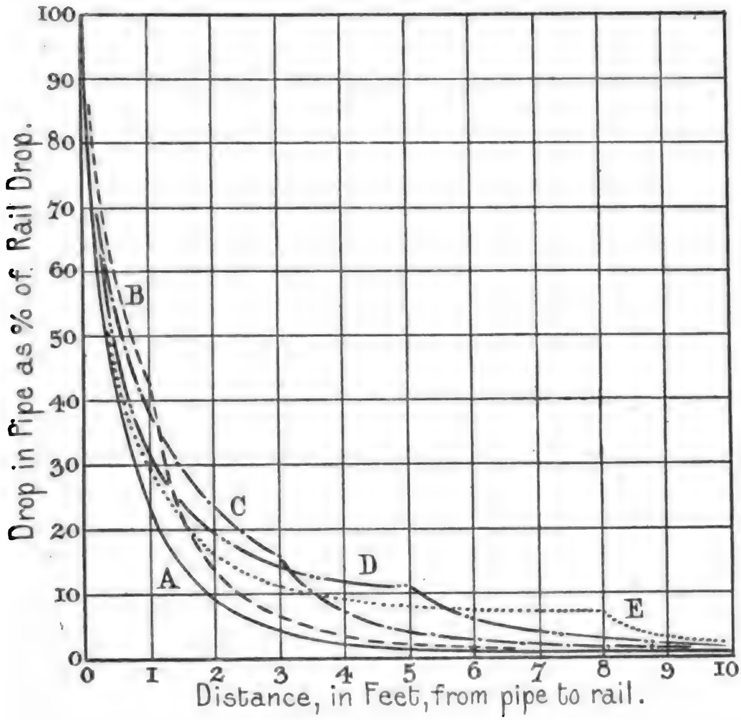

FIG. 11.—Voltage Drop in a Pipe in Terms of the Rail Drop and Distances from Rail.

A. Pipe and rails in soils free from electrolytes.
B. Ground impregnated for 12 in. round rail with strong electrolyte.
C. As B, but electrolyte extending 36 in.
D. As B, but electrolyte extending 60 in.
E. As B, but electrolyte extending 96 in.

Note.—Curve A represents the general case, the other cases being very exceptional with standard track construction.

It is widely believed that with very heavy loading the British limit of 7-volt rail drop would be prohibitive, but with negative boosting this would not be the case. The German Commission already referred to recommends a maximum value of 2·5 volts, but this is too low, as the 7-volt limit has proved to be a reasonable insurance against damage,

which is all that should be expected. A value recently incorporated into the City ordinances at Chicago of 12 volts maximum is considered very harsh, but the curves given in Fig. 11 show that the pressure drop along a pipe situated within 3 ft. of the rails may easily exceed 10 per cent. of the total rail drop, and so values greatly in excess of 7 volts are not to be encouraged.

Construction.

Legislation alone cannot provide complete protection against electrolysis. It must be assisted by careful construction. Metallic structures must not approach within 3 ft. of the rails (the German Commission recommends 1 metre), and in new construction or reconstruction care must be taken to reduce to a minimum the number of poles which require to be bonded to the rails as these pass down amongst the pipes beneath the footpath.

Guard wires ought to be insulated from the poles and "earthed" by means of insulated cables passing down the interior of the poles to the rails, and poles carrying gas-lamps ought to have triple insulation rather than to be bonded to the track, as the latter is equivalent to bonding the pipes themselves to the track, which is bad practice.

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DISCUSSION BEFORE THE MANCHESTER LOCAL SECTION ON 3RD DECEMBER, 1912.

Mr. B. WELBOURN: I am pleased to find myself in agreement with the authors as to the ideal to be attained in the negative feeder arrangements of any tramway system, in endeavouring to bring the points of connection of the tramway feeders to the rails to a uniform potential throughout, so that all the negative feeders shall be used in the most economical manner. If this could be brought about, it would make electrolysis of adjacent metallic structures an entirely negligible quantity. The mention of superimposed circuits reminds me of a patent brought out by the late Mr. J. R. Salter. I do not know if it was put into practice. It was intended that, on a tramway system where the feeding was done from both ends, there should be overlapping of the positive and negative feeders, and the idea was that the return currents in the rails should balance one another and should decrease the drop in the rails. I should like to know whether the system was tried in practice on the Lancashire United Tramways, or elsewhere, and with what result. Mr. Welbourn.

The question of whether to use negative boosters and the solution of other problems in tramway engineering are really only financial questions, and one looks round to see whether there is any other way in which the problems can be solved at the same or lower cost. I am not sufficiently familiar with the early history of tramways to know what caused the adoption of 500 or 600 volts C.C. as the standard pressure. It is evident now, however, that if it were practicable an immense advantage would be gained by raising the pressure which is used. Have the authors really considered the raising of the pressure on the trolley wire, and if not, would they be good enough to do so, and give us the result of their investigations at some other time? The authors mention a loading of 1,500 kilowatts per mile of route in Manchester. At 600 volts this means that 2,500 amperes have to be dealt with in the track; but at a pressure of 1,500 volts, which is now being considered for continuous-current railway work, the current would be reduced to 1,000 amperes. If the pressure could be raised there would thus be a very large decrease in the cost of both the positive and negative feeders, and there would also be other consequential advantages, such as a reduction in the number of sub-stations, the

Mr. Welbourn.

extension of the area of operation of the tramways, etc. I do not know what view the Board of Trade would take, but there is no inherent difficulty in insulating or in making trolley wires perfectly safe over the streets, even if it is necessary to resort to an auxiliary suspension to prevent a broken wire from falling into the street. It is for the designer of tramway equipments to say what is the practical limiting voltage for which tramway motors, controllers, and other gear can be constructed. I do not suggest that the general raising of the pressure on all tramway systems is immediately practicable, or advisable, but I do think that this is a question which might be usefully considered in cities such as Manchester. Where the loading is heavy, and where what the authors call "rehabilitation" of the system has to be dealt with from time to time, it might not be practicable to make a complete change-over to a higher voltage at once, but a commencement might be made when opening out a long new route.

Mr. Pearce.

Mr. S. L. Pearce: It would have been very interesting to us if we could have had some of the subject-matter which I know the authors were proposing to introduce into this paper, giving particularly some concrete figures of the Manchester tramway system, to show the effect of the increased traffic service and the effect of increasing the proportion of the larger to the smaller cars in relation to the question of the power. The authors tell us that the energy consumption per car is some 38 per cent greater at the city end than at the suburban end of the route. I should have liked the figures dealing with the kilowatts of maximum demand per car to have also been given, as these would have shown what a striking increase of power has been required owing to the increase in the car service conditions. The paper deals mainly with the subject of negative booster systems. In Manchester we have a large number of sub-stations, and the particular subject is not of such vital importance in Manchester as it may be in other systems having a smaller number of sub-stations to feed approximately the same area; the authors have told us, for example, that at Glasgow there are only six sub-stations as against nineteen in Manchester.

On page 694 the authors state that "in view of the strict limitation of the line pressure the governing factor in the feeder designs must, at some stage in the development, change from overheating to pressure-drop, and that boosting must ultimately be resorted to"—that is rather a sweeping statement; I take it that it entirely refers to continuous-current 600-volt systems, as of course it would not be actually correct if one considered the case of mixed systems, *i.e.* E.H.T. transmission and low-tension distribution.

On page 695 the authors refer to "the reluctance of engineers to employ feeders much larger than 1 sq. in. in cross-section"; we have had occasion to look into that recently in Manchester, and have come to the conclusion—and I believe we are following Continental practice—that where a total cross-section of 1 sq. in. is necessary it is better to use two ½ sq. in. cables rather than one 1 sq. in. The current density can be raised much higher with two ½ sq. in. cables than with one 1 sq. in.; and

Mr. Pearce.

even after allowing for the slightly increased cost of the cables, the larger section of troughing, etc., it will still be found to be a commercial proposition to use two ½ sq. in. cables in place of one 1 sq. in.

On pages 697 and 698 there are two statements which at the first glance may possibly seem somewhat contradictory. The authors state that the "amounts of current returning by the respective feeders, if of the same size, will be almost inversely proportional to their lengths," and then later they go on to say that the "distribution of the return current amongst the feeders is to a far greater extent influenced by the nature of the load distribution." Our experience in Manchester is that the low resistance of the rails, to which the authors have drawn our attention, will to a very great extent swamp the load distribution, and therefore it has been found that employing, as we do, resistance in the short negative returns to a limited extent, the current does return by the respective negative feeders in the inverse proportion to their lengths. I quite agree with remarks the authors make with regard to Fig. 5, A; frequently it will be found that it is far better to cut the bonds and allow the separate negative feeders to bring back their own current respectively.

There is an interesting statement near the top of page 699:— "For a given length of track with increasing loading the maximum permissible rail drop is ultimately attained when copper must be provided to carry any additional current, and as this copper is operated at a very low current density its use is very uneconomical." I entirely agree with that statement; in fact it seems to me that it is quite a mistake to put copper in parallel with the rails; if there is any copper to spare I suggest that it should go into the negative feeders.

Near the top of page 700 the authors say, "By the use of negative boosters the track may be divided into definite feeding sections almost as perfectly as is the line, and the flow of current in the rails may be controlled in a manner which is almost ideal." I lay stress on the word "ideal" bearing out the remarks of Mr. Welbourn when he very rightly emphasized the necessity of bringing us back to the £ s. d. point of view when considering this question. I agree it may be ideal, and is ideal, but is it commercial or is it really necessary that the flow of current should be controlled in a manner which is almost ideal? There is a very great difference between having "necessary" conditions and "ideal" conditions. In a previous paper by the authors* it was pretty well established that there was no case of electrolysis in Manchester. Of course to a very great extent that may be due to the fact that we have a large number of sub-stations with short feeders, but probably the authors have rather over-emphasized the necessity of achieving the "ideal." With the view of closely approximating to these ideal conditions we do in a good many cases use a resistance on the short negative returns and so secure all that is really necessary or desirable at a cost considerably less than is involved in the use of a negative boosting system.

* *Journal of the Institution of Electrical Engineers*, vol. 43, p. 449, 1909.

Mr. Pearce.

On page 703 the authors set forth the advantages to be gained from the general use of negative boosters; it seems to me that the strongest point for their use, apart from the ease of load distribution, is the fact that the "pressure loss in the negative feeders is no longer subtracted from the supply pressure." On page 704 there is the very valuable Fig. 9. I venture to say that if we had got nothing else in this paper but this figure the paper would be well worth writing; I am sure it will be of very great value when tackling problems other than those with which we are immediately concerned this evening.

I think one ought to remember, when preparing or trying to draw a comparison between negative boosters and the use of resistances in short negative feeders, that the expense of the two systems must of course be taken into consideration; no one would pretend for a moment that it is possible to install the negative booster system for such a low capital expenditure as is possible by using resistances. I should be very glad if the authors could give us some further details of the phenomenon described on page 706 as "superimposed return circuits." I do not quite follow the meaning of that paragraph; it seems to me that if the current actually passes as a superimposed current through the generator of the second sub-station only one thing could occur on compound-wound machines.

With regard to the paragraph dealing with construction, the authors refer to the necessity of having triple insulation when tramway standards carry gas lamps. I think that the correct thing is obviously to remove the gas lamps.

Mr. Ratcliff.

Mr. H. A. Ratcliff: I fully agree with most of the points raised by the authors, but I think that they have given the subject of negative feeder resistances rather scant treatment. As Mr. Pearce has stated, there is no comparison between the cost of a booster and that of a resistance; and there is no doubt that in many cases a resistance may be used with advantage. Within reasonable limits a resistance produces exactly the same rail-potential regulation as a booster; this is owing to the low resistance of the rails compared with the resistance of the negative feeders; since four rails in parallel have a resistance equal to 4 sq. in. of copper, whereas the negative feeders will probably only be of 0·5 sq. in. section, consequently the effect of load distribution is swamped to a great extent by the low resistance of the rails. It is usual to regard a resistance as an expensive and wasteful piece of apparatus, and as an electrical energy-dissipating or converting device its efficiency is in fact exactly 100 per cent, or, in other words, the loss on any load is always 100 per cent. The maximum efficiency of a booster will not exceed 80 per cent, *i.e.* the loss expressed as a percentage of the output at full load will be 25 per cent, and proportionally greater at lower loads, the loss at no load being an infinite percentage. The rail-potential regulation is a result of the booster output only, and the comparison is therefore between the resistance loss and the booster loss at an output corresponding to the resistance loss. As the load factor of a traction system is always less than

100 per cent, it is therefore extremely probable that in many cases the efficiency with a resistance will equal or even exceed that with a negative booster; and when the question of capital charges and running costs is also considered there is a distinct advantage on the side of the resistance. The rail-drop conditions resulting from the use of a resistance may not be ideal, but they will probably be sufficiently good for most practical purposes. The use of a resistance will naturally necessitate a wider range of voltage regulation on the generators. I do not understand the authors' reference to superimposed current, and should be glad if they would give a detailed explanation in their reply. It appears to me that a superimposed current on the series field of a compound-wound generator would have disastrous effects. I agree with the suggestion that the Board of Trade 7-volt rail-drop regulation is the only one practically affecting electrolytic and vagabond current troubles. Mr. Ratcliff.

Mr. C. L. E. STEWART: There is one point with regard to the connection of the trolley pole, etc., to the rail; in most cases the cable sheathings are bonded directly to the rail, which practically makes them into negative feeders; but from what the authors say it would appear to be bad practice. On the other hand, if the sheathings of the cables are not kept at the potential of the rails, burning is likely to result if the sheathing should come in contact with the metal-work, more especially if the cables are laid solid or drawn into earthenware conduits, when the sheathings are pretty well insulated. At Rawtenstall we use negative boosters and we have them connected in the way suggested by the authors, that is, the boosters are excited from the positive feeders, grouped according to the district which the negative cables serve. The armature is in series with the negative cables. I think the booster is a much better way of dealing with the varying conditions, for even in small tramway systems the local loading at special times may be heavy and may occur in widely different parts of the system. I think negative boosters are in some ways objectionable because they are very apt to run at a dangerous speed should the motor circuit-breaker open. Positive boosters are worse still in this respect, as they attain an excessive speed more rapidly. Automatic devices are absolutely necessary with positive and negative boosters, but I think everybody will agree that the fewer automatic devices we have to make up for shortcomings of certain plant the better. Mr. Stewart.

Mr. J. J. McMAHON: One or two points in the paper appear to me rather inconsistent with modern English practice. First with regard to the positive feeding system referred to by the authors as at New Jersey, U.S.A. I should like to know what purpose the circuit-breakers serve when fixed to the tops of poles in a congested area as shown in the diagram of that system. It appears to me to be very antiquated, and I should like the authors to explain the method that is adopted for re-setting these circuit-breakers. In Manchester some years ago it was suggested to adopt an automatic appliance which was to be installed Mr. McMahon.

Mr. McMahon.

in the section boxes, but the idea was never carried out, fuses being used instead. We found these also were not satisfactory, and eventually knife switches replaced the fuses and have been in every way a success. I am therefore still of the opinion that circuit-breakers or any kind of automatic appliance installed in section boxes in the street or fixed to poles would mean considerable calls on the emergency staff of a tramway undertaking; and under the circumstances I am still in favour of the various sections being controlled at the source of supply.

The second point I wish to mention is with regard to parallel feeding. In Manchester this is carried out in one or two cases only, as for example along Piccadilly and London-road, where there are heavily loaded sections. In this particular case there are three feeders running in parallel, and in case one of the feeders broke down it would be cut out without materially interfering with the traffic in that section, whereas the method suggested by the authors of having one main feeder connected to sub-feeders would, I think, hold up all the traffic which is fed by the sub-feeders concerned, if the main feeder happened to break down.

I am afraid it would be exceedingly difficult to comply with the regulation that metallic structures should be kept at least 3 ft. away from the rails. I cannot see the necessity of such a limit so long as the rail drop is kept within regulations. With regard to guard wires being insulated from the poles, this was carried out at Leeds in 1891, but at Walsall in 1892 the guard wires were earthed, and the pressure on both these tramways was 300 volts. Gas lamps have to my knowledge been attached to tramway poles (in this country) for the last twenty years, and I have not known of any faults having taken place. As many as 400 gas lamps have been attached to the poles in Manchester, and there have not been any complaints, except with regard to the mantles, which, I am informed, are expensive to maintain; the Gas Department is removing them on that account. Lastly, in Manchester since the commencement of the electric tramway system the whole of the overhead line has had triple insulation, and in a number of cases quadruple.

Mr. Hollingsworth.

Mr. E. M. HOLLINGSWORTH: Some years ago we tried negative feeders without boosting, but found they were absolutely useless. Later on, owing to the voltage drop and earth leakage exceeding the Board of Trade limit, we installed a negative booster, a combination of two boosters driven by one motor. For the reasons already pointed out by Mr. Pearce, we could not possibly hope to get ideal conditions, but we tapped the rails at the junction of five sections, the most congested part of our system, and the results have been very satisfactory. The booster is excited by two of the most heavily loaded positive feeders supplying the sections to which the negative feeders are connected. Regarding the construction of overhead equipment, we have had trouble on one or two occasions owing to guard wires making contact with the live wires, the current going to earth by way of the standards, the water mains, and gas service pipes. I quite agree with

the authors that it is advisable that guard wires should be connected to earth by means of insulated cables, particularly where gas lamps are attached to standards. I do not approve of bonding the lead covering of the tramway or other cables to the rails, or connecting them to earth in the close vicinity of the negative feeder. From my experience it is advisable to separate them as far as possible. Mr. Hollingsworth.

Mr. C. C. ATCHISON: A great feature of the paper is that it not only applies to large undertakings, such as Manchester, Chicago, etc., but also to small undertakings. In the early days of tramways, certain probable schedules of running were apparently fixed upon as being the likely requirements in the future, and in view of these the feeder cables were laid out so as best to suit the prospective schedules; but as time has gone on we find that the conditions of working are entirely different, the schedules completely altered, and the sizes of the negative feeders, which though large enough at the outset and arranged to be suitable for the assumed schedule, have now become much too small and the arrangement practically unsuitable. Many large tramway undertakings have been called upon, and apparently have been able, to deal with these matters, but the smaller undertakings are now also feeling the difficulties of the situation. I think I am right in saying that it was not generally the practice, at all events in small undertakings, to insert ammeters to measure the current in the return feeders, although these instruments were utilized in the feeders supplying the overhead line; with the result that although one is quite aware how the current is being supplied through the positive feeders, almost complete ignorance prevails with regard to how it is divided between the return feeders. Having an arrangement of return feeders myself originally laid down to meet certain conditions, I find it is absolutely essential with the growth of the tramway undertaking to find out how the current is returned, and this is no easy matter with the varying traffic and an arrangement of return feeders suitable for an entirely different schedule of working. Mr. Atchison.

With regard to the use of one large feeder and radiating sub-feeders, in a busy area I am inclined to think this is not as suitable as the use of a number of separate feeders, at all events in the smaller undertakings, as in case of trouble I consider it is better to localize it as far as possible; this can be better done with a number of feeders than by the use of one or a very few large feeders with radiating sub-feeders. From ordinary experience one realizes that should the circuit-breaker operate on a feeder supplying a busy route, every motorman on the section tries to start up directly the supply is again available, and it is almost impossible to keep the circuit-breaker in. I cannot endorse the authors' statement about construction on page 708. I have a very disrespectful opinion of guard wires as a whole, and I don't agree with the suggestion that they should be insulated from the poles in order that these latter may not, through the guard wires, form connections between the different sections of pipes. Also I consider it is necessary to bond the poles

Mr. Atchison.

to the rails frequently. After all, it is the pipes that have to be protected, and there seem only two things to do. One is that all pipes in the vicinity of the rails should be removed to a safe distance, or wherever they approach the rails they should be stoutly bonded to them throughout the whole of the system, though I must admit that there are certain arrangements where exceptions should be made.

A point which has been raised in the discussion is the case of pipes that are laid after the tramways have been put into operation. I think that in towns where the municipality is working the tramways it should be an instruction to all other departments when laying pipes that these should be laid at such a distance away from the rails as to be entirely safe from electrolytic action, but if this cannot be done, and on that account there may be any danger to the life of the pipes, the department owning them must take their own responsibility and not turn to the Tramway Department for compensation should the pipes become corroded. I do not think it is fair where proper arrangements have been made for dealing with return currents under certain conditions that these conditions should be altered by some other department, and the responsibility still remain with the Tramway Department. From experience I am quite able to endorse Mr. Pearce's remarks with regards to the suitability of utilizing two 0·5 sq. in. cables in place of one 1 sq. in.; and the authors' statement on page 705, that "each booster should be excited from the grouped positive feeders serving the same area," appears to me to be the only correct way of arranging such excitation.

Mr. Cooper.

Mr. A. G. COOPER: With regard to bonding feeders, I find that the best arrangement is to bond the feeders at the station only and to keep them clear everywhere else; thereby trouble is avoided. The Tramway Company at Colne when they first started did not bond their feeder although it was laid solid; they started with electrolysis and the cables have never been good since. It seems to me that heavier rails ought to be used so as to minimize the rail drop, because the extra price for the rails should be cheaper than installing copper. I quite agree with Mr. Pearce that it is much better to lay two small feeders rather than a large one, on account of the higher current density at which the smaller cables can be operated.

Mr. Rowland.

Mr. R. ROWLAND: I should like to refer to Fig. 10. In our case I have arranged to supply all the feeders through the booster, so that if a circuit-breaker comes out there is still the load on the remaining section—we have three positive feeders. There is no mention made by the authors of any safety device to cut out the booster in the event of the motor failing. In our case I have arranged for an electrical control governor to be placed on the end of the shaft so as to cut out the booster before it reaches a dangerous speed. With regard to Mr. Pearce's remarks as to the size of feeders, at Stretford we have run two negative 0·8 sq. in. feeders instead of one 1·6 sq. in. My experience was that when we had 100 cars in our district it interfered with the regular traffic, and the load rose to 1,800 kw.; but now that the

Mr. Rowland.

police regulate the traffic the kilowatt demand has fallen to 1,300 or 1,400 kw. with the same number of cars. The average power demanded at the station per car in use depends on how the traffic (cars) is regulated. I think the authors have made a mistake in putting forward Fig. 10 as their own scheme. We had a scheme to deal with this particular district as far back as 1908, which was before the football ground at Old Trafford was thought of, only we did not intend to install a booster at first, but to put a cable parallel to the track and bonded to same at certain intervals along the route. About 1910, due to the football load, a booster was suggested, and in 1912 my Committee agreed to this. I drew up the specification and carried out the work. In this particular case I think D is not the correct point of connection, as it involves an unnecessary outlay. Referring to Mr. McMahon's remarks *re* circuit-breakers in the box outside the station, the circuit-breaker referred to was a reverse-current breaker and was eventually taken out altogether.

Mr. Preece.

Mr. G. G. L. PREECE: I believe that Glasgow was the first city to adopt the negative booster; the Glasgow system is rather prone to electrolysis mainly due to the fact that the Glasgow water supply makes a good electrolyte, combined with the fact that their mains are laid near the track in cement-lined conduits. Consequently a great deal of attention has been paid to limiting the rail drop. I remember that at one time they had a very large system of potential wires and used them in their system of negative feeders; the function of these potential wires was, I think, similar to what the authors describe in the second paragraph on page 701. As far as I remember, the positions of the negative feeders were fixed from results obtained with the potential wires, and I remember being shown in a sub-station the pressure-drops of a great number of sub-sections in the city; I noticed that the drop did not exceed 2 volts in any section, most being below 1 volt. Possibly the authors have obtained some information from Glasgow, and might therefore be in a position either to amplify or correct what I have said. In any event such a system as that at Glasgow shows that the Tramway Department must have gone into the question of negative boosters with some knowledge of what they were after.

Mr. Yerbury.

Mr. H. E. YERBURY (*communicated*): I am pleased that the bogey of electrolysis has not been resuscitated. The deplorable state of things formerly existing in Chicago and other American cities could not have existed under British Board of Trade rules and regulations, and I see no reasons to fear any trouble now that overloading conditions prevail in many of our large towns. The Sheffield Corporation Act of 1901 had a protective provision inserted by the Sheffield Gas Company relative to possible electrolytic injury or damage to mains, pipes, etc.; and although our loading and output in units have been more than trebled since that year, it has been demonstrated to the satisfaction of the Company (through their electrical engineer who still takes periodical tests) that no trouble has taken place and that we are still able to work within the Board of Trade limits.

Mr. Yerbury.

In respect to positive feeding, I think the system of feeding with graded distributors shown in Fig. 3, A, is preferable in point of efficiency and economy to the Manchester system shown in Fig. 3, B. I must take exception to the authors' remarks that "no definite principle has been adopted" in regard to negative boosting, and I personally see nothing of a novel or new principle or design in what is called, on page 705, "the authors' design." I dealt with the correct principle of negative boosters and returns in an informal paper read before the Leeds Local Section of the Institution on 13th April, 1905; this paper was published in the technical Press. The parabolic law in relation to the potential of the rails was explained and shown in a diagram in this paper, and the authors' scheme, shown on page 705, Fig. 10, is the only obvious position for a booster return cable. The fundamental principles were, I believe, first shown in Institution papers read in 1898, and I myself dealt with specific instances of successful installations in my paper of 1905—notably at Bristol, Dublin, Sheffield, and on the Schöneberg Tramways near Berlin, where booster return cables allowed of satisfactory continuous-current working up to 4½ miles from the generating station. In regard to present legislation, it must be admitted that taken as a whole the results are satisfactory to all concerned. Still I think the current-density limit of 9 amperes per square inch in the rails could be exceeded under certain conditions without harmful results.

Dr. Rosenberg.

Dr. E. ROSENBERG: Some speakers have mentioned that a resistance inserted in a short feeder is sometimes a desirable alternative to a booster in the long feeders; I fully agree that this may be the case from the efficiency point of view if the necessity for equalizing the feeder drop occurs only for short periods at long intervals. Of course, on the other hand, the resulting reduction of voltage would sometimes make it impossible for the cars to keep the scheduled speed. In Fig. 1 the kilowatt-hours are given per car-mile in different cities. The figure for Berlin is extremely low. In that city trailer cars are used. I should like to know if the figures are obtained by dividing the energy consumption of a train by the number of cars.

DISCUSSION BEFORE THE BIRMINGHAM LOCAL SECTION ON 4TH DECEMBER, 1912.

Mr. Trotter.

Mr. A. P. TROTTER: Electrolysis on tramways is a subject which must be treated in a practical manner. Scientific experts have argued that because 1 ampere will dissolve 13 lb. of iron in a year, or 15 grains in an hour, under laboratory conditions, the same action will go on in the case of tramways. If this were the case, the ends of all the tramway rails which are positive to the earth must have corroded. But no such corrosion has been observed. On the other hand, a striking statement was made in the reply by the authors to the discussion of their paper of 1909,* that pipes buried in the earth do not

* *Journal of the Institution of Electrical Engineers*, vol. 43, p. 481, 1909.

Mr. Trotter.

short-circuit it, as earlier writers have suggested, but the pipes are short-circuited by the earth. I accept that statement as one of importance, but if it is taken apart from other considerations, electrolysis would be impossible. Cases in this country are fortunately very rare. It is generally acknowledged that this is due to the Board of Trade Regulations and to the carefulness with which they are generally observed. It was fully expected when they were drawn up in 1894 that alterations would be needed as time went on. Some additions have been made to meet new developments, but the code remains much as it was. One or two of the provisions are not useful, but they do no harm. It is generally recognized among tramway engineers that these regulations have not hitherto impeded tramway development, and that they have protected pipes from electrolysis to an extent which was not anticipated. If in the further developments of electric traction any regulation is found to impede progress, consideration would be given to any case on its merits.

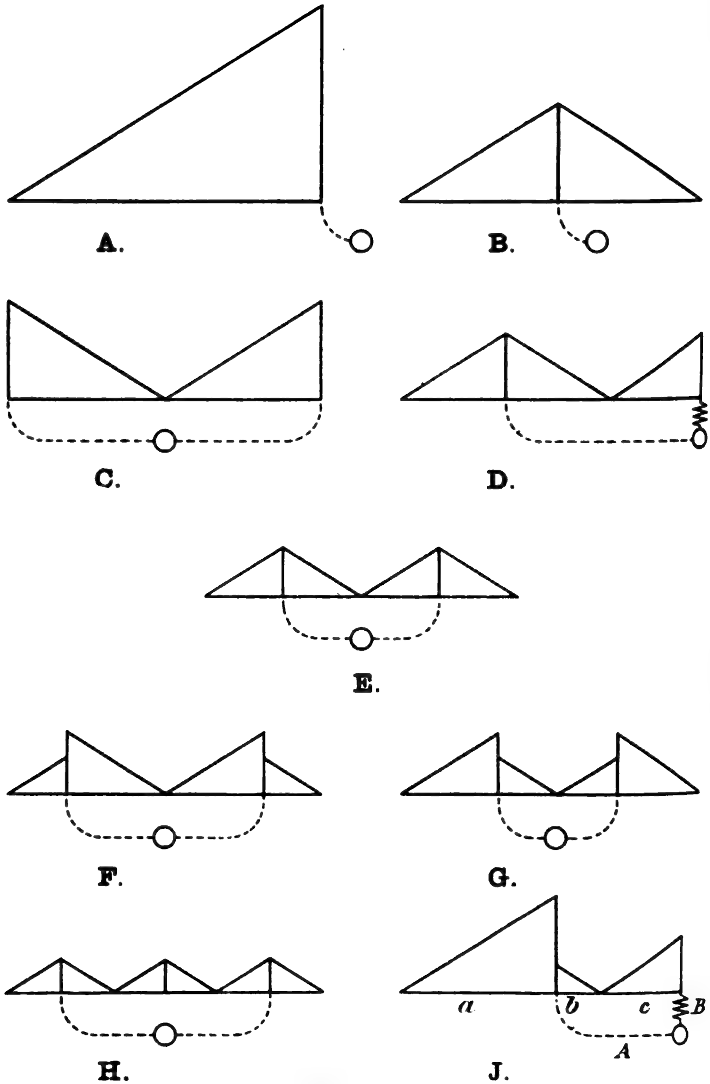
The rule that pipes must not approach nearer than 3 ft. to the rails is well intentioned, but excavation is not made to a depth of 3 ft. in building a tramway, and the owners of old gas and water pipes seldom know the position of them. The law in Western Australia provides that "The promoter shall, at his own expense, alter or divert all wires, metal pipes, tubing, and cables that would be subject to electrolysis so that they shall not be within 24 in. of any portion of the tramway system carrying electric current." But excavation does not as a rule extend even to 24 in. The only cases of electrolysis that I have known have been at a distance of less than 2 ft. from tramway rails. The suggestion that poles should not be bonded to the rails either for guard wire earthing or to avoid "live" poles where gas lamps are fixed to them is a good one. It comes too late, but should be adopted for new work and extensions. In the case of poles carrying gas lamps, triple insulation might be substituted for bonding; but when insulators are arranged in series, when one fails it is sometimes found that that is the one which has been doing the work.

The shaded area in Fig. 2, and the word "rehabilitation," need explanation; rehabilitation seems to relate to various improvements and economies in which the authors probably had a hand. At the end of the section on positive feeding, the working pressure of 600 volts is mentioned. Although 550 volts is the usual maximum, the regulations give 650 volts as the limit at which the supply may be generated. A supply at 600 might be allowed, but it seems a pity to disturb the standard. It appears that the use of bare copper negative feeders bonded together and at many places to the track is a serious obstacle to the use of boosters in Chicago. To increase the number and size of sub-stations in a city is evidently the wrong method, and the copper is not well loaded with current.

The authors recommend that each booster should be excited from the feeders serving the same area. Engineers have found that it is difficult in some cases to divide the districts in that manner.

Mr Trotter.

Mr. H. M. Sayers at one time used separate excitation for a booster field in a manner that was claimed to give automatic regulation, but it



FIGS. A to J.

appeared that if all the cars were off the road the booster would pump a considerable current round the circuit. I hope that the authors will

Mr. Trotter.

explain how Fig. 11 has been calculated, and I also hope that the diagrams of the Chicago network, and those of the Glasgow and New Jersey feeders which have been shown to the meeting, and the interesting matter which was interpolated during the reading of the paper, will be published by the authors on a future occasion.

Some six years ago I began to look into the elementary principles of the design of negative feeders, in continuation of the paper which I communicated to the Institution on 28th April, 1898.* I took the case of a line 7 miles long, 2 cars per mile, each taking about 28½ amperes, making 400 amperes total, and a track consisting of two rails of a total sectional area of 20 sq. in. In the diagrams the horizontal line represents the length of the tramway line. Vertical height represents the current in the rails at any point, or amperes per square inch. The circle represents the generating station or sub-station; the dotted lines, negative or return feeders; and a zigzag, a resistance added to a feeder. In Fig. A herewith the whole 400 amperes return through the rails to the station at one end of the line, giving 20 amperes per square inch. Fig. B represents the station at the middle of the line. The current density is reduced to 10 amperes per square inch. In Fig. C the same result is obtained by two negative feeders extending to the ends of the line, a purely hypothetical case. Fig. D shows what can be done by tapping the line at two-thirds of its length, by a feeder bringing back 266 amperes, while a resistance in the short feeder prevents more than 133 amperes from passing. The current density in the rails falls to 6⅔ amperes per square inch. If the station is at the middle of the line, two feeders each of a quarter of the length of the line, arranged as in Fig. E, bring the current density down to 5 amperes per square inch. For geographical reasons it might be convenient to arrange the negative feeders as in Fig. F or Fig. G, which show the effect on the current density. When the station is at the middle of a line another step would be to duplicate Fig. D by the arrangement shown in Fig. H. It is sufficient to consider the station to be at the end of the line. Fig. J shows a general case. The feeder A brings back 200 amperes from *a* and 50 from *b*, while the feeder B with added resistance brings back 150 amperes from *c*. The necessary resistances may easily be worked out, and the problem becomes ready for mathematical treatment. But such treatment is utterly useless without the practical application of the factors £ s. d. The cost of the cables and of laying them, the cost of the energy lost in the added resistance, the practicability of running at a high current density in feeders instead of using resistances, all these and probably other considerations must be taken into account before the problem of the most economical design of return feeders can be dealt with. I had not the necessary data, and I put the matter aside. No attention seems to have been given to the subject, so far as the Institution is concerned, since the paper read by Mr. H. M. Sayers on 3rd May, 1900. This seems to be an appropriate occasion to give the elementary beginning of the problem in the hope

* *Journal of the Institution of Electrical Engineers*, vol. 27, p. 457, 1898.

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that someone engaged in tramway work will develop it, and no one would be more capable of doing so than the authors of this paper.

Mr. Wedmore.

Mr. E. B. WEDMORE : In a paper contributed to the Institution in 1902,* I produced some data and formulæ bearing on electrolysis. A great deal more data is now available, but my conclusions are not seriously challenged. From the data in the paper under consideration, and that in a paper on "Traction Vagabond Currents" by the same authors in 1909, I now estimate the average specific resistance of the earth to be in the neighbourhood of 50 ohms per yard cube. There has been a good deal of controversy as to whether the pipe should be considered as a short-circuit to the ground or the ground a short-circuit to the pipe. This is purely a question of figures, and I have attempted to estimate the resistance of the relative paths through the ground and the pipe. These estimates are necessarily very rough. They serve, however, to draw attention to the important features in the problem. The two paths we are concerned with are that from the track through the general body of earth and that from the track through the pipe, this latter path including a path through the earth from the track to the pipe and from the pipe to the track. My figures indicate that in the case of iron pipes the earth is practically a short-circuit to the pipe, but in the case of lead pipes or of pipes having a low internal resistance throughout, the resistance of the path through the pipe is low enough to require careful consideration.

With a view to bringing out the important features, I have made an estimate bearing on the length of life of the pipe under average conditions, and have assumed corrosion taking place to the depth of $\frac{1}{10}$ in. uniformly over the surface of the pipe. My calculations apply to pipes lying parallel with the track, and not more than 3 ft. from the rails over the greater part of their length. The problem may be attacked in two ways :—

First, we may assume that the limiting feature is the resistance of the pipe along its length, including the resistance of any joints. This resistance will limit the current flow due to the applied E.M.F., and will therefore limit the average current density on the surface of the pipe. Second, we may ignore the resistance of the pipe and consider the resistance of the earth path between the track and the pipe. This will positively limit the current density entering the surface of the pipe. I assume that the pipe is not connected to the rail where the current is leaving the pipe, and that two-fifths of the potential drop along the track represents the E.M.F. driving current along the pipe, or driving current from the general mass of earth into the pipe. I believe this is an overestimate. I have taken an extreme case where the voltage drop is 2 volts per 1,000 feet of rail, which is twice the limit set by the Chicago Rules. With the voltages we are discussing, the evidence is that the electrolysis will correspond on the average to only 5 per cent of that calculated on the assumption that the whole of the observed current flow is electrolytic in effect. It will be observed that even

* *Journal of the Institution of Electrical Engineers*, vol. 31, p. 576, 1902.

Mr. Wedmore.

with 7 volts drop there is not 1½ volts difference between the pipe and the general body of earth at either end of the pipe, and in considering the effect on short lengths of pipe the voltage available is some small fraction of 1 volt. Thus the calculations probably err a great deal on the safe side in the case of short pipes. No attempt has been made to correct for unequal distribution of current. This is an important item in connection with calculations based on the line resistance, but does not affect those based on the resistance of the earth path between track and pipe. I will now give a few figures illustrating the sort of results obtained :—

In the case of a 6-in. iron pipe 5,000 ft. long it is estimated that it will take 400,000 years to cause the amount of corrosion I have postulated. This figure is based on the line resistance. An estimate based on the surface resistance would indicate a figure of 600 years. Hence it is clear that the line resistance is the limiting feature. I have taken the joint resistance at an average of 14 ohms, which figure is given by Messrs. Cunliffe in their 1909 paper. In the case of a similar pipe 500 ft. long the line resistance gives a corresponding limit, and the surface resistance a limit of about 6,000 years. From these limiting figures one can afford to knock off a nought or two and still find that the investment of 2d. at compound interest will far more than cover the cost of renewals due to electrolytic troubles. In the case of a 2-in. lead pipe 5,000 ft. long the estimate based on the data in my 1902 paper is 160 years. The data given by Messrs. Cunliffe in their paper and represented by the curve on page 707 indicate a much higher figure, the estimate running between 400 and 1,600 years, these figures being based on the line resistance of the pipe. In the case of a pipe 500 ft. long the line resistance gives a similar limit on the assumed potential gradient, and the surface resistance now gives a limit in the neighbourhood of 160 years.

Having regard to the large number of assumptions made and the uncertainty as to their accuracy, I do not consider we can ignore the possibility of serious electrolytic trouble on lead pipes running parallel with the track and not more than 3 ft. away from the rail. On quite short lengths of lead pipe the pipe resistance has a negligible influence, but in this case also the voltage drop is so small that it is improbable that any appreciable electrolytic effect would take place. In the case even of long pipes the line resistance of a pipe with wiped joints is much too low to serve as a safe limiting feature. The surface resistance can, of course, be greatly increased by painting or compounding the pipe or by burying it in a trough filled with compound. In order, however, that such treatment shall show any appreciable effect in limiting the current, one must consider the actual values of surface resistance required. In the above estimates based on a 2-in. pipe the resistance of the path up to the pipe, reckoned over a square inch of pipe surface, is of the order of 20,000 ohms on an average. Thus to guarantee safety one requires a resistance of the order of ½ megohm over a square inch. Such a figure would guarantee a life exceeding

Mr. Wedmore.

1,000 years whatever might be the soil or the pipe resistance. I have not here considered the improvements obtainable by connecting the pipe to the rail. This device does not serve as a general solution, although it may be done advantageously in many particular cases. Safety lies in keeping the pipe away from the rail, and the life rapidly increases as the distance exceeds 3 ft. It will be observed that as the surface resistance is the only safe limiting feature to rely on in the case of lead pipes, regulations covering the maximum voltage drop in the rail are all that is required. These determine the voltage available between the general body of earth and the pipe to cause current flow into and out of the pipe.

Dr. Kapp.

Dr. G. KAPP: In connection with the calculation of Mr. Wedmore as to the length of time required to reduce by electrolysis the thickness of a pipe by a given amount, I wish to draw attention to the fact that the calculation was made on the supposition that the action was uniform over the whole surface. In reality there is not this uniformity and the electrolytic attack is concentrated on a few points, with the result that the pipes become pitted and at certain points holes may be eaten through the metal. I have myself seen a large amount of old gas piping rendered useless by such pitting and perforation. This was in Hamburg. After the introduction of electric traction along a certain boulevard planted with trees it was found that the trees died, and on investigating the cause it turned out to be due to leakage of gas into the ground. The leak was from the gas pipes which had been destroyed by electrolysis of vagabond currents. This was in the days before negative feeders and boosters were used. With such appliances properly installed, however, no electrolysis need be feared.

I cannot agree with the authors in their statement that the use of negative boosters increases capital outlay and working costs. I think the reverse is usually the case. To my knowledge the first case where a negative booster was installed in Germany was on a line which had a light week-day traffic, but which was on Sundays heavily loaded. To provide for the heavy load a very large negative feeder would be necessary, but by installing a much lighter feeder and inserting a booster the cost was very materially reduced, so that the feeder and booster came much cheaper than an unboosted feeder. As the authors have told us that in Chicago there are uninsulated feeders with a section as heavy as 7 sq. in. it is evident that the cost of boosters in conjunction with much lighter insulated feeders is quite insignificant.

As regards working costs, Mr. Trotter's explanation of how by the insertion of resistance in the short feeder it is possible to ensure that that feeder takes no more than its fair share of the return current, simply amounts to this, that the ohmic loss in the short feeder is artificially increased to that unavoidable in the longest feeder. This extra loss must be more than the losses occurring in boosters, since these would only be used with the longest feeders, whilst the shorter feeders would have no more than their natural ohmic loss. Finally, I wish to draw attention to the generous way in which the authors have referred to the

assistance they have received from other professional men. This is not only fair to the other workers, but it also gives the reader of the *Journal* confidence that the statements made by the authors are based on their own experience and also that of other responsible authorities. Dr. Kapp.

Mr. W. E. GROVES: This paper has revived problems with which I had to contend in 1905 when laying out the distribution system of the Birmingham Tramways. In the light of the authors' recent researches a few words in respect of the Birmingham system may be of interest. The alternatives of sub-stations and negative boosters were considered, and the necessity for providing a local supply for power and lighting in widely separated districts in addition to traction supply determined that sub-stations should be equipped; consequently no great difficulty has been experienced in conforming to regulations. Dealing first with the positive side, the arrangement except on terminal routes is a combination of A and B as shown in Fig. 3, dividing switches being provided enabling the route to be sectioned as desired, the underground and overhead copper being always in parallel. The routes between station and sub-station are cabled with equal section throughout, the copper being sufficient to supply the normal load of the sub-station in emergency. The negative feeders are so placed that, as nearly as can be foreseen, there is the same potential at each of the various feeder points. Where several negative feeders radiate from one station or sub-station the balance is maintained by altering the effective section of the feeders, triple concentric cables with pilots being provided for this purpose. The negative booster referred to on page 700 is not on the Birmingham Corporation electric supply system, but is installed in an isolated station formerly owned by a company, the routes now being taken over by the Corporation. There is no doubt that with negative boosters and plenty of pilot wires a very flexible system can be designed and the track sectioned as desired, but commercial considerations will not permit the universal adoption of this electrically ideal method of controlling vagabond currents. The restriction to 7 volts between any two points requires some qualification on a long route consisting of several negative sections; the potential may rise in a series of small steps until it exceeds 7 volts, although the conditions would be quite satisfactory in respect to the safety of pipes. Mr. Groves.

Mr. A. M. TAYLOR: With reference to the authors' statement on page 699 that the addition of sub-stations for purposes of splitting up the track into smaller areas is uneconomical, their remarks are no doubt intended to apply to the case where the traction and lighting systems of a town are quite distinct. Where they are combined under one management the additional sub-stations are desirable for the lighting scheme. In the case of this city, I went very carefully into the use of negative boosters in order to see whether it was at all possible by their use to feed the whole tramway system direct from one generating station, and found that some 20 negative boosters would be necessary. Such a "fleet" of boosters seemed impracticable in a station where floor space was very valuable. I think Fig. 9 is especially interesting Mr. Taylor.

Mr. Taylor. as showing how the requirements of 9 amperes per square inch and 0·5 volt per 100 yd. fail to meet the conditions when the traffic density exceeds 500 amperes per mile, a figure which has already been exceeded many times in Chicago, and soon will also be exceeded in this country. It must be borne in mind that the authors' paper only deals with specially heavy traffic conditions.

Mr. Sayers. Mr. H. M. SAYERS (*communicated*): I have only seen the abstracts of this paper and the reports in the Press of the discussions on it; the following remarks may therefore do Messrs. Cunliffe some injustice due to my incomplete information.

The dates of 1898 and 1900 doubtless seem too far off in the remote beginnings of electric traction for the perusal of papers, etc., of that date to attract modern practical investigators, but reference to a paper read by Mr. Parshall before the Institution on 28th April, 1898,* will show that he was then aware of the uselessness of unboosted track feeders. In a paper which I read before the Institution on 3rd May, 1900,† I referred to Mr. Parshall's proof; and I proceeded to give the general expressions for calculating the feeding points on the track, the sizes of the feeders, and the capacity of the boosters required on them, as well as a concrete example to which these general expressions were applied and the results obtained in commercial terms. The same paper dealt with other aspects of the distributing arrangements for electric traction, and the mutual influence of generating and distributing systems. So far as I can judge from the reports in my possession, Messrs. Cunliffe confirm pretty generally my results of May, 1900. Naturally, in applying the general principles to present conditions some changes have to be allowed for. For example, the Board of Trade rule restricting the current density in rails to 9 amperes per square inch was not then in force. Nor was the distribution of electrical energy for general purposes so developed as it now is—a matter greatly influencing tramway distribution when one authority runs both the electric supply and the tramway undertakings. Also the merely mechanical difficulties of getting conductors of many thousands of kilowatts capacity at 500 volts pressure out of single generating stations or sub-stations were not so close then as they are now. Re-reading my paper, one paragraph which I had forgotten impresses me as a suggestion not yet acted on to my knowledge, but quite a likely solution of several track difficulties, and an alleviation of return feeder and booster expenses, worthy of consideration by those in the position of Messrs. Cunliffe and their chiefs :—

"It seems, however, that a modification in track construction using continuous girder supports under the rails and dispensing with concrete and setts between them, should be a logical consequence of electrical traction, and that the conductivity of the track might thus be largely increased and bonding made much more permanent without much increase in construction costs. The difficulty now experienced at the joints of tram rails should disappear, and the life of the road be greatly

* *Journal of the Institution of Electrical Engineers*, vol. 27, p. 440, 1898.
† Ibid., vol. 29, p. 692, 1900.

increased."—To-day I should add corrugation to the difficulties which might disappear. Mr. Sayers.

I may mention that the numerous tramway systems of the British Electric Traction Company for which I designed the electrical equipment between 1898 and 1903, and some others where I have advised, all had their feeders, track feeders, and boosters designed on the methods discussed in my paper.

Messrs. J. G. and R. G. Cunliffe.

Messrs. J. G. and R. G. CUNLIFFE (*in reply*): From the contradictory nature of the discussion it is evident that a paper on this subject was required, as the true nature of the factors governing the choice of feeding systems does not appear to be generally understood. Thus, much has been said in favour of the use of simple feeders suitably spaced and balanced in resistance as necessitated by the load distribution, and whilst on the one hand the authors have been accused of clinging to an electrical ideal to the neglect of the financial considerations, on the other hand Dr. Kapp is surprised that they apparently consider negative boosting, although ideal electrically, to be financially unsound. Both criticisms are untrue because they are far too general. Again, Mr. Yerbury remarks: "The deplorable state of things formerly existing in Chicago and other American cities could not have existed under British Board of Trade rules and regulations." How was compliance with such relatively severe rules and regulations to be obtained? Not, as a financial proposition, by the use of simple feeders, however well spaced and resistance balanced. Further, Mr. Yerbury states categorically, with regard to positive feeding, that: "The system of feeding with graded distributors shown in Fig. 3, A, is preferable in point of view of efficiency and economy to the Manchester system shown in Fig. 3, B."

The point which the authors wish to emphasize is that, in the discussion generally, the effect of the factors which essentially govern the choice of feeding system has been ignored. The correct system both of positive and negative feeding is determined absolutely by the number of supply stations considered in relation to the nature and extent of the undertaking, and in the paper the scope was purposely limited to the conditions appertaining to large undertakings heavily loaded and having relatively few supply stations each of great plant capacity.

As an illustration, two undertakings of equal size in the United Kingdom may be considered, viz. Manchester with upwards of 20 supply stations, and Glasgow with only 6. In the former the use of the system of simple positive feeders as in Fig. 3, A, and of simple negative feeders suitably spaced and balanced in resistance, are perfectly sound both electrically and financially; but in the latter the distributor system of positive feeding as in Fig 3, B, and the use of negative boosters, are equally sound. It is the conditions of undertakings such as Glasgow, but on a far larger scale, that have been specially considered in the paper. Mr. Yerbury's remarks *re* positive feeding are applicable to Leeds and Nottingham, but are far too sweeping as a general statement.

Messrs. J. G. and R. G. Cunliffe.

A consideration of the diagrams given by Mr. Trotter shows that by the use of negative feeders, suitably spaced and resistance balanced, ideal results electrically may be obtained, although for one value of the load distribution only; but it is evident that with extra heavy loading the pressure loss in the return feeders and resistances could not, in view of the strictly limited line pressure, be tolerated. This pressure loss, as has been remarked by Mr. Pearce, is the most important factor.

To sum up then, the use of resistance-balanced negative feeders is correct in the case of undertakings having, for various reasons as described by Messrs. A. M. Taylor and J. G. Groves, a relatively large number of supply stations, but where the number of stations is relatively small compared with the intensity of electrical loading, the use of negative boosters will be found advantageous on many grounds.

TABLE A.

Date.	Area.	No. of Cars in operation.	Max. demand, kw. per Car.	Observed by	Single Truck Cars / Double Truck Cars
1903	Fed from sub-stations	127	9·4	Pearce and Gunton	
1908	Urban and suburban	364	12·0	Authors	
1903	Centre of city fed from Bloom-street (low tension)	173	10·2	Pearce and Gunton	1903 4·4 : 1
1908		186	16·1	Authors	1908 2·6 : 1

Tramway conditions are so variable in their nature that the authors have deemed it best to describe the electrically ideal use of negative boosters only, and they are pleased to find that their ideal agrees generally with the views expressed by Major P. Cardew and Mr. A. P. Trotter in notes on a paper read before this Institution by Mr. H. F. Parshall,* and also with the views of Mr. H. M. Sayers.† Copies of these papers have been supplied to the authors by Mr. A. P. Trotter, although unfortunately too late for earlier reference. The authors' treatment is based entirely upon their own researches into vagabond current phenomena, and such agreement in results is very satisfactory.

The authors would remind Mr. Welbourn that at the outset of electrification in this country a line pressure of some 300 volts only

* Loc. cit. † Loc cit.

Messrs. J. G. and R. G. Cunliffe.

was permitted, as referred to by Mr. McMahon, and was subsequently raised to the present value, which is considered the maximum safe pressure as regards human life. If the pressure could be raised to 1,500 volts as suggested there would undoubtedly be great saving, but the authors have seen 120 yards of trolley wire pulled down into the roadway, and would scarcely recommend any higher pressure than 600 volts. A strong ground of objection to the catenary suspension would be its unsightliness. In many cases of congested traffic, as at the centre of Manchester, it would be better if the pressure could actually be reduced to, say, 350 volts, as the surplus pressure is largely wasted in the car resistances.

The values of maximum demand per car which are asked for by Mr. Pearce are contrasted in Table A with the earlier values given by Mr. Gunton and himself, the increase being largely due to the greater congestion.

In referring to the reluctance of engineers to employ feeders much larger than 1 sq. in. in cross-section the authors did not necessarily mean in one cable. They were referring to the wide area shut down in the event of breakdown of a feeder of such great current capacity, and they are well aware of the Manchester practice of employing two ½ sq. in. cables in place of a 1 sq. in. feeder. In Fig. 4 they themselves advocated such sub-division of the main feeder. Mr. Pearce emphasizes the value of £ s. d. in negative feeding, and submits that there is a great difference between "necessary" conditions and "ideal" conditions. Under the conditions dealt with in the paper the ideal conditions are also the necessary conditions. The authors would not, as already stated, recommend the general use of negative boosters at Manchester, Birmingham, etc., where owing to a combined lighting and traction load there are numerous stations.

Mr. Ratcliff's defence of feeder resistances refers of course to conditions beyond the scope of the paper. The case of superimposed return circuits is illustrated by Fig. K. The negative feeder S F was long in comparison with S F_2, and current flowed from the section A B to F_1, where it entered the independent station, to flow along the trolley wires and continue on its way to F_2, via the cars between C and some moving point O, whose position was fixed by the momentary load distribution in the various sub-sections. The length O C, although belonging to the station S_1, was in reality, from an electrical point of view, added on to the sub-section F_2 C operated by the sub-station S. There was no difficulty with the over-compounded generators, since the outgoing and incoming currents at the stations were in perfect equality, but the rail drop was increased on sections B F_1 and F_2 C and reduced on section F_1 C. Such interference is common not only to neighbouring stations but even to neighbouring undertakings, and an undertaking having a low rail drop may be prejudiced by another having high values. In the case shown in Fig. K the exchange of current was limited by the insertion of a resistance in the feeder S F_2, but this is correct for one value only of the load distribution. In an exactly similar case

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at Birmingham a negative booster is inserted in the long feeder S F which provides compensation at all loads. If this negative booster is cut out of circuit only some 25 per cent of the outgoing current returns by the long negative feeder, the superimposed current then being of a very high order.

Mr. McMahon considers the use of circuit-breakers on the poles at New Jersey, U.S.A., to be very antiquated, and so perhaps it is in principle, but his objections are based on experience with fuses which, on blowing, necessitated an emergency call for their replacement on the spot. Remote control would remove the objections to automatic switches at the distant end of a compound feeder, and their use offers

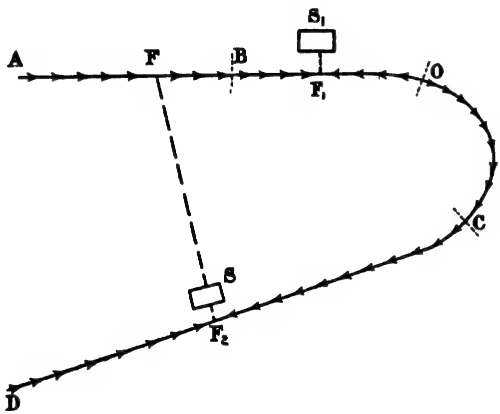

FIG. K.—Superimposed Negative Circuits.

S = Sub-station feeding from A to B and C to D.
S_1 = Steam-generating station feeding from B to C.
F, F_1, F_2 = Negative feeding points.

in certain cases important advantages. The main feeder shown in Fig. 4 consists of several independent cables and the danger of all breaking down simultaneously may be ignored.

Mr. Atchison is correct in saying that the paper applies also to small undertakings which have outgrown their original feeding provisions, but the application to such cases requires to be carefully considered as this is purely incidental. It would be better if, as he suggests, ammeters were inserted in the negative feeders. The suggestions under the heading of construction have been carefully considered and are based on a knowledge of many accidents and mishaps.

The booster scheme shown in Fig. 10 was accepted by the consulting engineers to the Stretford Council in preference to the one mentioned by Mr. Rowland, and, as Mr. Yerbury points out, the

point D is the only correct point of connection of the negative feeder. One would have thought that the results attained would have satisfied Mr. Rowland that the scheme was correctly designed. It would have been sheer waste of money to have run a cable in parallel with the track as originally intended by Mr. Rowland. Messrs. J. G. and R. G. Cunliffe.

The authors have no detailed information *re* the system of potential wires at Glasgow, but they have been greatly assisted throughout by the

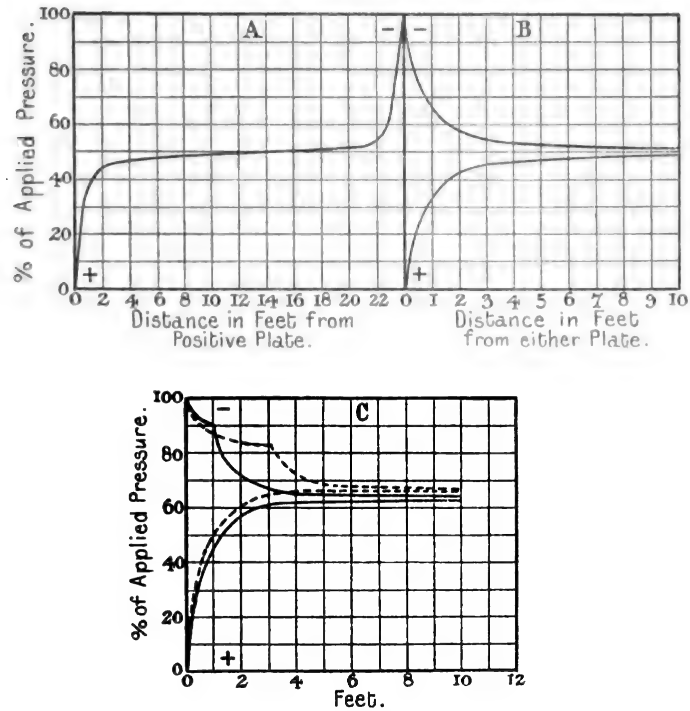

FIG. L.—Potential Gradients between Earth-plates.

A Fully developed gradient.
B Same gradient plotted with + and – zones in juxtaposition.
C Gradient showing effect of electrolyte around the – plate.

peculiarly complete installations at Manchester, whereby the potential may be measured between any two points whatever on the whole undertaking. If all undertakings were equally well equipped in this direction some surprising effects might in many cases be noticed.

Mr. Trotter asks how Fig. 11 was obtained. In Fig. L is shown the potential gradient curve between two earth-plates in uniform earth plotted in two ways. A careful consideration will show that the vertical distance between the two curves, B, in Fig. L gives the potential

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difference between two points in the earth situated at the distances indicated from the two plates respectively. The same treatment applies to the potential difference along any line in the earth parallel to the rails, and the curve A in Fig. 11 is obtained in this way, the other curves being taken from similar gradients traced under the electrolytic conditions described.

THE "KNIGHT" PUBLIC FIRE-ALARM SYSTEM.

By E. E. MOORE, Associate Member.

(Paper first received 21st November, 1912, and in final form 22nd January, 1913.)

This fire-alarm system has been invented and the apparatus designed to comply with the following requirements:—

1. Faults of any description on the line should make themselves known at the central station or be readily discoverable from there.

2. Any one of these faults should, by simple manipulation at the station switchboard, be cleared so far as its effects are concerned, so that the alarm-points are still able to send through an alarm. The fault or faults may then be seen to at the first convenient moment, after which the circuit can revert to its normal state.

3. Only the pulling of a fire-alarm handle, and nothing else, must give the fire signal at the station.

4. The fire signal coming through should at once proclaim itself as a genuine fire-call. If more than one alarm-point is on a circuit the signal should then denote within 10 to 15 seconds of the start of the call the identical point which has been operated.

5. It is an advantage for a code alarm to repeat itself, and a still further advantage for every alarm to record itself automatically.

6. If more than one alarm-point is pulled at a time, there must be no confusion of signals, and yet no alarm must be lost by being automatically held up whilst a previous alarm is ringing in.

7. The fire-alarm transmitter should be simple and positive in action. Once the operator has pulled the handle he should have no further power over the signal.

8. When the transmitter has been pulled a bell should ring at the point, giving some idea to the operator that his signal is being transmitted. This bell should not ring unless the alarm signal is positively coming into the station.

9. If telephonic communication is provided for the public to make use of, this should be arranged in such a manner that the telephone is not accessible for use, or cannot be brought into circuit, until the fire-alarm has been given and fully received.

10. District calls coming into one station in large towns should be automatically transmitted to any other station or stations if necessary.

11. It must be possible to add any reasonable number of new alarm points from time to time to the system. This should be done without necessarily running lines from the new point back to the

station. Lines from the new point to the nearest existing point should be sufficient. A new point should also not entail any alterations or additions to the station apparatus, nor should it be necessary to put any part of the system out of order whilst the new point is being connected up.

THE "KNIGHT" TRANSMITTER.

The actual appearance of a transmitter is shown in Fig. 1, whilst Fig. 2 is diagrammatic of the same to simplify the explanation of its construction and working. The apparatus consists essentially of a small train of wheels capable of being driven by a spring which is enclosed in the spring-box. The clockwork is normally held in check by means of a release cam, which is solid with the release cam axle and engages with the pull trigger. It will be observed that this trigger is pivoted at the inside end of the pull-handle rod. When the handle is pulled, the trigger is likewise pulled forward away from the release cam, thus leaving the clockwork free to revolve. As soon as the trigger is clear of the release cam it is so weighted that it tilts upwards, and no matter how quickly the pull-handle may be released and pushed back by its antagonistic spring, it cannot stop the motion of the clockwork. It will be seen that this pull-handle and rod have nothing to do except release the clockwork. They do not, for instance, wind up a spring when pulled, entailing the release of the handle before the clockwork starts. A snatch pull has the same result as when a person pulls the handle and holds it out or gives it a succession of pulls. Once the clockwork has started, the operator has no further power over the instrument. All that the public is asked to do is to "Break glass and pull handle."

Geared to the release-cam axle in the ratio of 2 to 1 is the code-drum axle, carrying the code drum solid with it. The periphery of the drum is divided into a number of metallic and insulating segments alternating with each other. Pressing on this surface are two code contact-springs. When the clockwork is released and the code drum revolves, as each metallic segment comes into contact with the code contact-springs, these two springs are earthed through the frame of the transmitter. By suitable subdivision this code drum makes first a series of sharp regular intermittent contacts, and then follows a prearranged code made up of a series of longer contacts.

When the code drum has made two complete revolutions the clockwork is stopped by the release cam again engaging with the pull-handle trigger. Should the handle now be pulled again, the result will once more be as first described. This is all the actual gear necessary to transmit the signals to the station; but among other things, provision has to be made for automatically holding up a signal should another post be in operation. For this purpose an electromagnet is provided, together with an extension circuit-breaking drum. This latter is on the same axle as the release cam. The holding-up arm operated by the armature of the electromagnet engages with a pin on

Fig. 1.

FIG. 3.

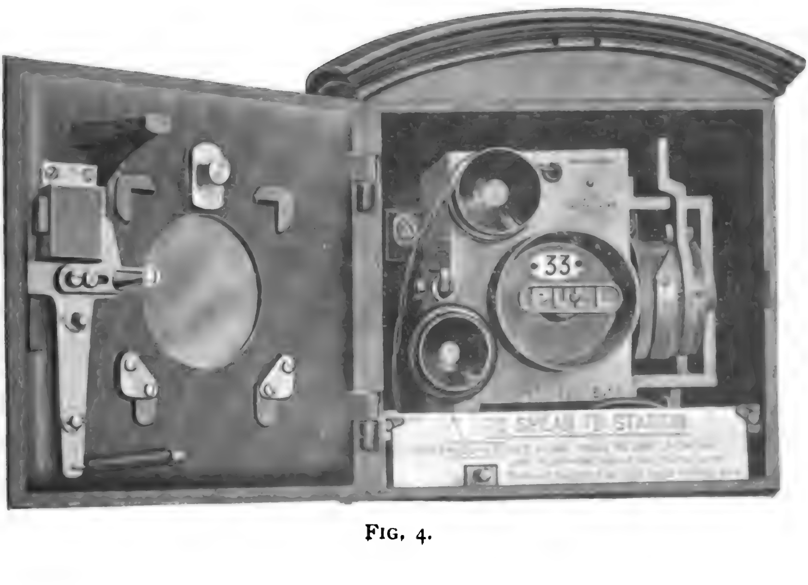


FIG. 4.

Fig. 5.

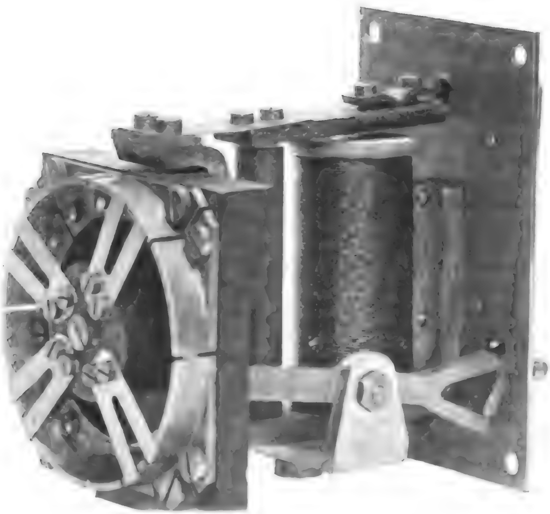


Fig. 6.

FIG. 7.

the code contact-wheel directly the two contact-springs are earthed by the first metallic segment after the pull-handle has been operated. Unless the current then flows round the electromagnet coils and the armature is attracted, the clockwork is stopped. The circuit-breaking drum normally keeps two pairs of line-extension contact-springs respectively in contact; but when the clockwork starts revolving, and

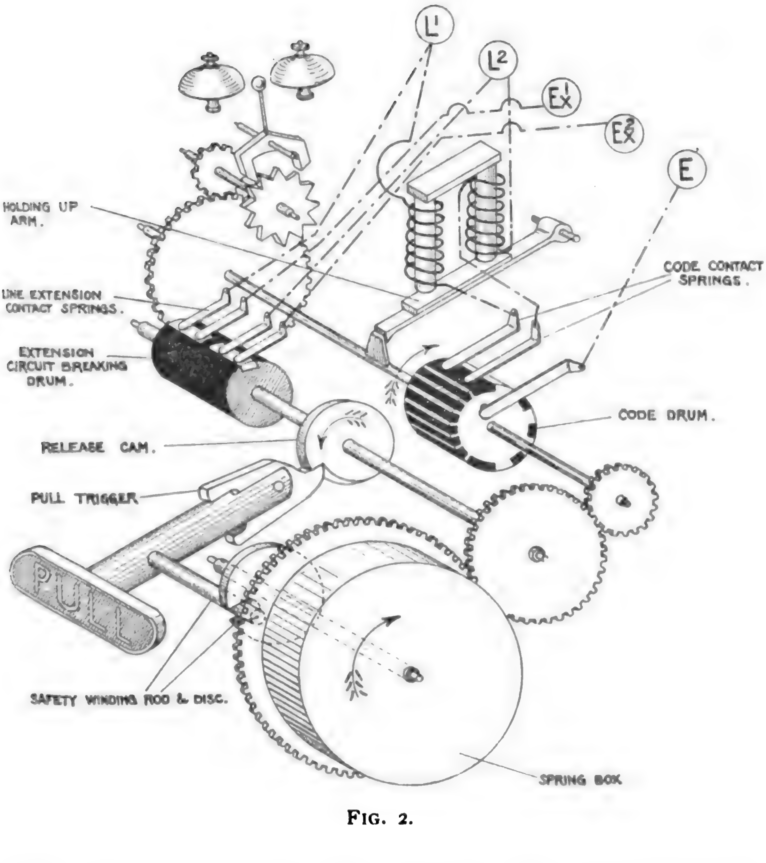


FIG. 2.

before the code contact-springs are earthed, the two pairs of line-extension contact-springs break contact electrically.

The further explanation of the method of holding up signals will better be understood when we come to consider the diagram of connections. The remainder of the small train of wheels is mainly for the purpose of governing the speed of the clockwork. The fastest revolving wheel operates an anchor pawl to which is fixed a bell hammer, so that when the clockwork is revolving, the hammer is vibrated and rings a bell. This bell forms the answering signal to whoever has sent the call,

and gives him some idea that his pulling of the handle is having effect. As the battery is at the station, the electromagnet in the transmitter cannot be operated and thus release the clockwork after the handle has been pulled, unless the signal has reached the station.

Telephonic communication is provided. With one pattern a jack only is fixed. In this case the firemen carry portable hand sets with them and plug in whenever they wish to speak to the station. The action of plugging causes the station to be rung up by a continuous ring, totally distinct from the regular warning intermittent ring given by the code drum. This method has the small advantage of doing away with the cost of fitting a telephone transmitter and receiver to every post, and incidentally minimizes any trouble which might occur on these in consequence of their being left in such a damp place as a street box. Against this there is the disadvantage of a portable hand set having to be carried about by anyone who wishes to get into communication with the station ; and it also of course precludes the possibility of anyone else speaking to the station. It is no doubt an advantage to the fire-brigade if, when giving the alarm, a person can supplement this alarm with some information regarding the extent and locality of the fire, but (and this is most important) the first and foremost object of an alarm system is to give warning to the brigade that they are wanted at a fire somewhere. Once they have got this assurance, and whilst they are turning out, then a word or two of explanation over the telephone, if it can be given without delaying the starting of the firemen, is to be commended. When therefore the public is given the opportunity to speak to the station it should only be possible when and after the complete fire signal has been received at the station ; and as the operator of the fire-alarm is generally in a more or less abnormal state of mind, only the simplest instructions should be in front of him to enable him to send the fire signal, without any confusing particulars of what he is to do in order to speak to the station.

In the fire-alarm now being described, this practical requirement has been met in the following manner :—

The only instructions which need appear on the outside of the box are "Break glass and pull handle." On the mainspring drum is fitted a projecting cam which, when the clockwork has run for a few seconds, engages with the bolt that holds the door of the box fastened, and unlocks it. The door is then pushed open by means of a spring plunger suitably fixed. The door having opened, another instruction tablet is disclosed, which gives sufficient particulars to enable the operator to use the telephone that is accessible only when the door is open. Fig. 3 shows this box before an alarm is given, and Fig. 4 the same box directly the complete signal has been received at the station.

The motive power to drive the clockwork is a strong spring. As stated above, the pulling of the alarm handle does not wind up this spring in any way, but only releases the clockwork. The spring must

therefore be wound up by the fireman who replaces the broken glass; and it would of course be a very weak point in the system if by any neglect the alarm was left in an unworkable condition. The following simple arrangements are therefore made to provide against this:—

On the pull-handle rod, and at right angles to it, is a projecting rod, while on the spring drum is a circular disc with a slot cut diametrically in it. This allows the end of the projecting rod to enter, and the pull-handle to go right home only when the spring drum is in a certain position, which is when the spring is wound up. At any other time the pull-handle is forced out a certain distance, so that if a careless man put a new glass in the door and tried to shut the door when the spring was not fully wound up, the handle would foul the glass and smash it.

The winding lever provided for winding the spring is fitted with a ratchet movement. A few down and up movements to this are sufficient to put the alarm in position again ready for another call. Should the winding lever not be placed properly back in position, the replacement boss on the door will automatically do this when the door is shut.

The spring drum, code drum, and extension circuit-breaking drum, being in solid mesh, will all revolve in a backward direction when the spring is wound up; and if no provision were made, would then send an erratic signal to the station and drop a flap there. Although this would not be recognized as a regular intermittent signal and fire-alarm, the necessity for dropping a flap at all is obviated by arranging a circuit-breaking lever operated by a cam on the winding lever, so that during the short process of winding, the code contact-springs are lifted slightly off the code drum, and the circuit cannot thus be put to earth through it. A non-inductive resistance of 3,000 ohms is provided and connected across the extension terminals of the end post on a circuit. The telephone gear at the post consists simply of a microphone and a receiver. The receiver normally hangs on an automatic switch hook.

Station Switchboard.

The station apparatus calls for no special description in detail, unless it is the drop-flap annunciator shown in Figs. 5 and 6. With this system the dropping of a flap alters 3, 4, or even 5 circuits, according to requirements. Thus besides being merely an annunciator it has further most important work to do. As may be seen by referring to Fig. 6, a circular form of circuit-changer has been adopted. The drop-flap is arranged so that in falling it engages with an arm which rotates the circuit-changing contact-springs through a certain arc. This instrument offers various advantages for this class of work:—

(*a*) The contact-springs or blades are always at the same pressure on the segments of the contact ring, and are therefore never liable to become "set"; which is always the case when a spring is bent backwards and forwards with or against its direction of pressure.

(*b*) The contact surfaces have no practical resistance, and being rubbing contacts are thus self-cleaning.

(*c*) There is no question of adjustment. If the flap is up, the springs must be on their proper segments; and when the flap is down they must all have changed over to their other segments.

(*d*) The weight of the flap acts only just in front of its dead-centre when in the up position; and as the flap does not engage with the lever which rotates the circuit-changer springs until it has fallen through an arc of about 25 degrees a very weak current energizing the electromagnet coil is sufficient to release it. Thus all the line current is called upon to do is to release the flap. It is the falling weight and leverage of the flap that supplies the energy to operate the circuit-changer.

Fig. 7 gives the general appearance of a switchboard for 30 circuits. If only five alarm-points are connected to each circuit this board therefore gives accommodation for 150 alarm-points.

Having described such apparatus as calls for special attention, we are now in a position to consider the working of the system as a whole.

Fire-alarms may be broadly described as either open or closed circuit systems. Fig. 8 shows the principle of the lay-out for fire-alarm points with the "Knight" two-wire open-circuit system. Any number of points can be joined up one after another on a single circuit. Likewise any number of circuits may radiate out from the station, each circuit having new alarm-points added to it as demands arise. This diagram also shows various call-bell groups running (in the case of a non-resident brigade) to the various firemen's houses.

Fig. 9 gives the lines and box connections for circuit A.

Fig. 10 gives connections for two call-bell groups with telephones.

Fig. 11 gives connections for the station apparatus.

In each of the call-boxes (Fig. 9) is the code drum, the extension circuit-breaking drum, and the electromagnet; the last mentioned, unless it is energized, holds up the clockwork after the handle has been pulled. It is proposed to consider the working of the system under every condition likely to be met with.

(*a*) *When a Point is operated.*—The clockwork rotates the code drum and circuit-breaking drum. The latter first disconnects both lines from all points farther away from the station than the point operated on this circuit. The two code contact-springs are then earthed at the code drum, thus allowing current to flow from the station along both lines and through the electromagnet coils. The code drum continuing to rotate, the circuit to the station is thus intermittently earthed and broken. The result of this at the station board can be seen by referring to Fig. 11.

The two lines become common after passing the test keys, the path of the current being through the circuit-changer operated by the drop-

flap annunciator, through the annunciator electromagnet winding to the top contact of the control-relay armature, through this armature to the electromagnet of the alarm relay, and then through the earth indicator, the battery, and back to earth. Directly the armature flap drops, its electromagnet coil is cut out, the line current being

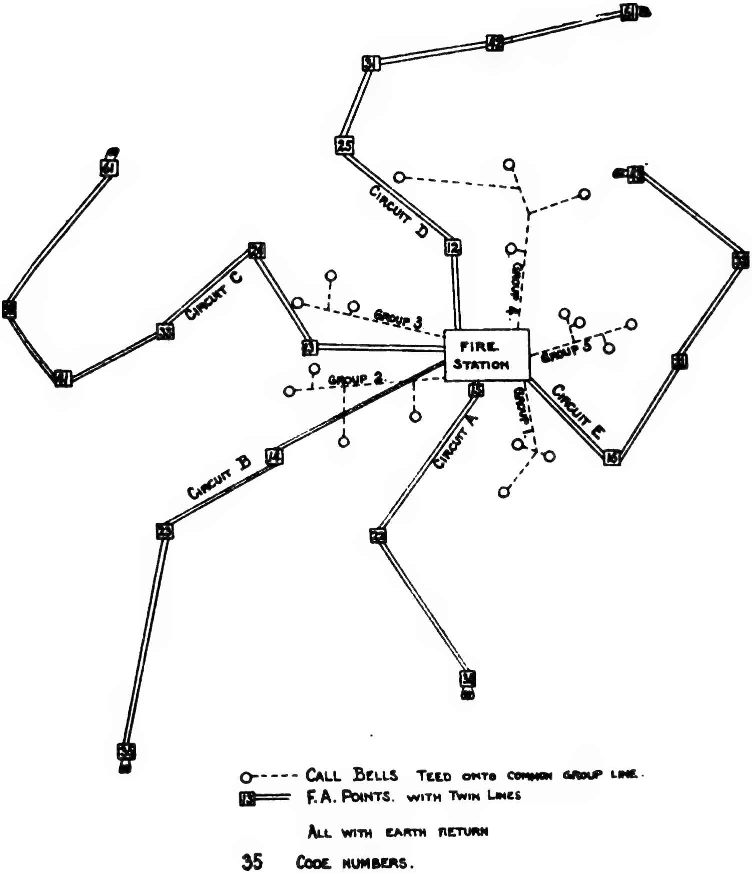


FIG. 8.

switched on to another segment of the circuit-changer, and from thence in series through each of the other circuit-changers on the board to the telephone speaking-key, and then direct to the alarm relay. The pulling of a transmitter handle therefore will ring the alarm-bell at the station, first with a series of regular intermittent warning rings and then with a code ring. The ink recorder joined in shunt with the

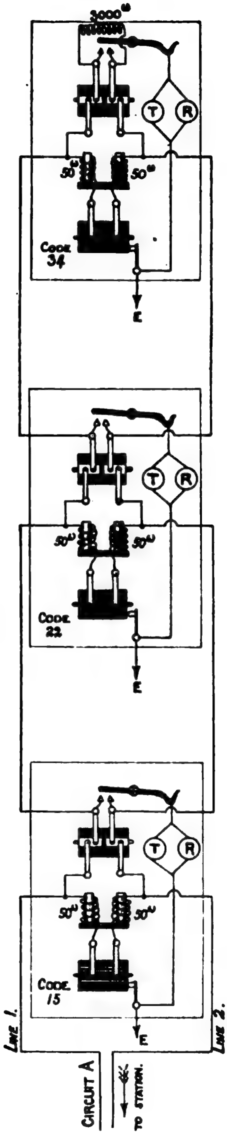

3000ω
T
R
50ω
50ω
Code 34
E
Code 22
Code 15
Line 1.
Line 2.
Circuit A
To Station.

Fig. 9.

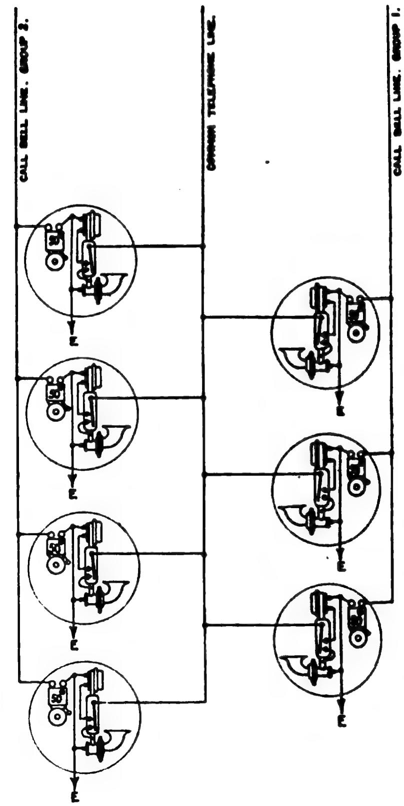

Call Bell Line. Group 2.
Common Telephone Line.
Call Bell Line. Group 1.
E

Fig. 10.

alarm-bell automatically starts and stops itself. It records every operation of the alarm relay. The record of a complete alarm-signal is shown thus:—

.... —— —— — — — —— .... —— —— — — — ——

(Code 231)

The annunciator, therefore, at once discloses the circuit on which the alarm has been given, and the code gives the identical point operated on that circuit.

(*b*) *Two or More Points on One Circuit operated at or about the Same Time.*—As there are a number of alarm-points on each circuit, and the point operated on that circuit is only recognized by its code signal, it is obvious that means must be provided to meet the contingency of one or more points being pulled at or about the same time, otherwise a confused erratic ring would result at the station were this state of affairs not anticipated.

When an alarm-point is operated, all boxes farther away from the station (as we have before seen) are electrically cut off, and no current can reach them until connection is again made at the extension circuit-breaking drum, when the alarm at the post operated nearer to the station has finished its call. If a box therefore is operated whilst one nearer to the station on that circuit is operating, the clockwork is held up by the catch on the electromagnet, and no further manipulation of the pull-handle will affect this. The point to notice is that the clockwork is held up almost immediately after the handle has been pulled, and with the code contact-fingers earthing on the first metallic segment of the code drum. When the nearer box has finished its signal, and the extension lines are again joined up, allowing battery current from the station to operate the electromagnet and thus liberate the clockwork, the signal of the box that was till then held up will automatically come into the station. Now supposing box 22 is operated, and whilst running another point (say box 15) nearer the station is pulled. Directly this is done, box 22 is cut off, its signal ceases, and the alarm from box 15 comes into the station. But the code of box 22, although it may be interrupted at any part of its signal, is not lost at the station. It will be remembered that the code-wheel revolves twice for each complete alarm. If box 22 is interrupted before the completion of its first code-signal, then it is held up at the end of the first revolution of the code-wheel; and when the lines are again joined through to it by the completion of the signals of a nearer box or boxes, then it automatically sends through and records its complete repeat intermittent, and code signal. If, on the other hand, box 22 is interrupted after the completion of its first code, the clockwork runs down and the remainder of the signal does not come in. This is immaterial, as the first intermittent and code signal has already been received and recorded.

(*c*) *Two or More Points on Different Circuits operated at or about the Same Time.*—When an annunciator flap is dropped at the station, this

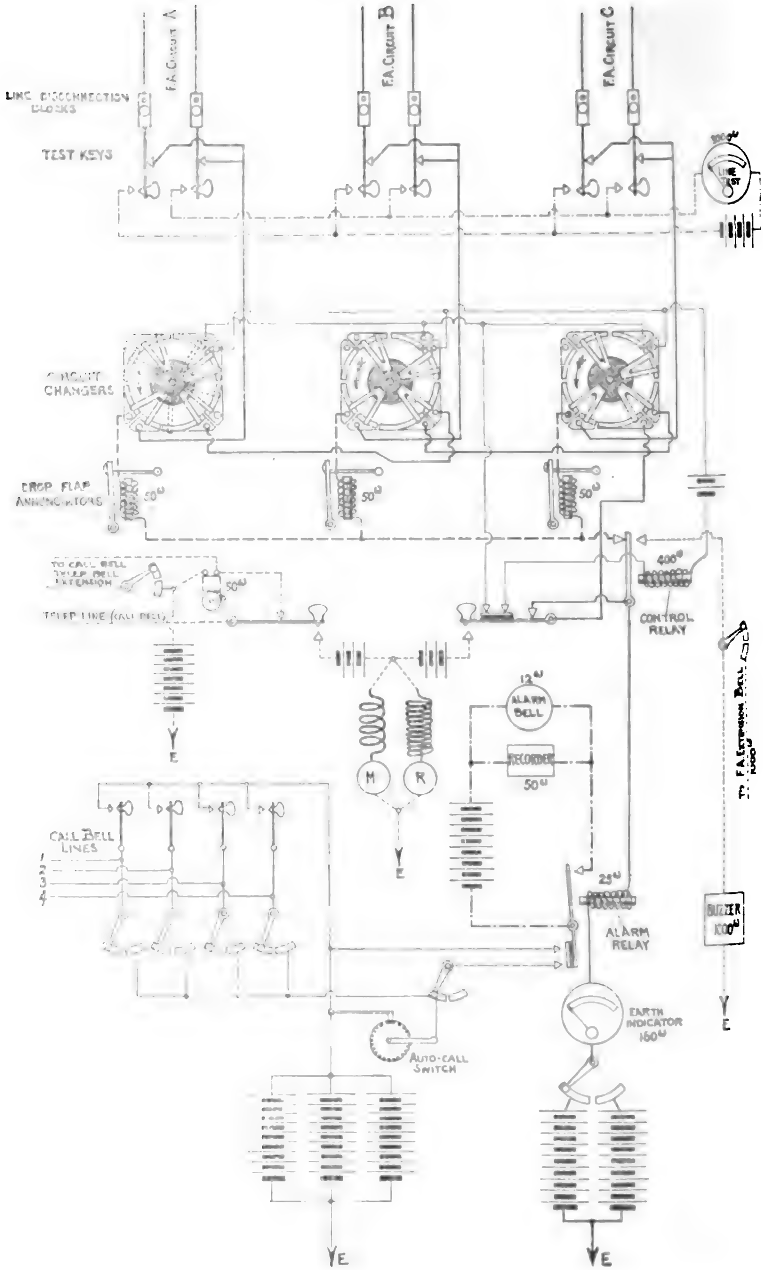

F.A. Circuit A
F.A. Circuit B
F.A. Circuit C
Test Keys
Circuit Changers
Control Relay
Alarm Bell
Recorder
M
R
Call Bell Lines
Alarm Relay
Earth Indicator
Auto-call Switch
Buzzer
E

Fig. 11.

closes a local circuit which energizes the control relay. The common lead from all the annunciator windings through which the line current first comes, is carried to the normal contact-maker of this control-relay armature. Therefore when an annunciator-flap is dropped, current is cut off from all other lines; and if another point on another circuit is operated, it cannot drop its annunciator-flap and send its signal until the flap that is down has been replaced. When this is done, any point on any other circuit which has meanwhile been operated, but held up, will automatically come in. There is another contingency to provide for under this heading. Supposing points on two or more different circuits were to have their handles pulled at the same identical moment. Then all of those circuits would drop their annunciator-flaps together. What is more probable to happen than this, is for points on two or more other different circuits to be operated at any time during the period that a flap on another circuit is down, and the signal coming in from the alarm first pulled. When the first flap is replaced, the other flaps on those circuits which are earthed will simultaneously drop. If no provision were made, the calls would come through from the different circuits under these conditions, intermingling and causing confusion. This, however, is simply remedied.

When the circuit-changer breaks the line current from the annunciator winding after dropping the flap, it switches the circuit on to each of the other circuit-changers in series, and finally on to the alarm relay. Thus with three circuits, A, B, and C, even if all drop their indicating flaps together, current is cut off from B and A, and only the signal on C will come in. When C is replaced, current is still cut off from A at B, and only the signal on B will come in. When B is replaced, the signal on A will come in. It will be noted that the point operated nearest to the station on a circuit takes precedence over other points which might be operated about the same time on that circuit, which is obviously a distinct advantage for the fire-brigade.

Faults.

(*d*) *An Earth developing or Extraneous Current on Line.*—This must necessarily show itself on the earth indicator. If it has a high resistance the deflection will be small, and will not affect the working results when an alarm is operated. The indication, however, is there, and steps may be taken at the earliest opportunity to locate the leak and restore insulation. If, however, the resistance of this earth decreases and goes on decreasing, it will arrive at a resistance when it will allow sufficient current to pass to actuate the annunciator, drop its flap, and ring the alarm-bell, thus giving an audible signal that the circuit has a bad fault on it. This ringing of the alarm-bell, however, will either be erratic or continuous according to the nature of the earth. It cannot possibly be mistaken for a fire-call which gives a regular intermittent ring followed by a code ring. There are two test keys to each circuit. By pressing separately, first one and then

the other of these keys over the flap which has dropped, the line which has the earth on it is discovered, as the bell stops ringing when it is disconnected from the line at fault. The line disconnecting-screw is then taken out, and the circuit is again in thorough working order on its remaining line. Extraneous current on the line may have the same effect as an earth.

(*e*) *A Line Broken and Earthed.*—The foregoing description for an earthed line also explains the state of affairs when a line is broken with the broken end earthing.

(*f*) *A Line Broken and not Earthing.*—This is the only big fault which does not automatically make itself known at the station on this open-circuit system. It can very easily be tested for, however, by means of the two testing keys provided on the station board for each circuit. By referring to the connections of a series of alarm-points on a circuit, it will be seen that a high resistance is connected across the two extension terminals of the last box. When the two keys of a circuit are simultaneously pressed at the station, a closed circuit is formed from a test battery, through one line, through every alarm-point across the contacts on their extension-line breaking drums, through the high resistance at the end of the line, back along the other line, through every alarm-point again by way of the other contacts on the extension-line-breaking drums, on to the station board, through the line test-indicator, and back to the test battery. This test need only take a couple of seconds for each circuit, and if the lines are intact the indicator pointer deflects to "lines in order," about three-quarters of the way across the scale. It should be noticed that with a line broken, even if it were undiscovered, the circuit is still quite in working order on its remaining line. Only in the very rare event of both lines becoming broken at once would this affect the working of the alarms, and even then those alarms on the station side of the breaks would ring-in if operated.

(*g*) *Two Lines in Contact on a Circuit.*—This would not affect the working of the alarms in the slightest, provided that there was no bad earth on the line, as of course an earth on one line would also earth the other under these conditions. The use of the test keys will discover, however, if any circuit is short-circuited, as when the two keys on a circuit are depressed, the current instead of going to the extreme end of the line and returning through the high resistance from there, will have a lower resistance circuit to complete, and consequently the test indicator will deflect past "lines in order" to "lines shorted." The one operation of these test keys, therefore, discloses at once the fact if the lines are in contact or if either is broken.

(*h*) *Lightning.*—Lightning striking the lines will very probably drop an annunciator-flap at the station and give a short ring on the bell. No further ring, however, follows. The annunciator having dropped, a buzzer is actuated until the flap is replaced.

(*i*) *Telephonic Communication.*—Speaking from any alarm-point to the station is provided for by means of a central battery at the station

and without the use of any induction coils or condensers at the posts. Where the telephones are kept in the post in preference to having a jack only there, the transmitter is fixed and the receiver normally hangs on an automatic switch-lever. This lever, when relieved of the weight of the receiver, puts the two lines on the extension side through the microphone and receiver in parallel, and thence to earth, thus dropping the flap of that circuit at the station and ringing the alarm-bell there continuously. The duty man in answering the call presses his speaking key, which breaks the alarm circuit, putting the lines through to the telephone battery, and thence dividing to the primary of the induction coil and microphone on one side, and the secondary of the induction coil and receiver on the other side, and on to earth, when oral communication is established.

The telephone at the alarm-box being connected to the extension lines is automatically cut off whilst an alarm is being sent in. Thus anyone snatching the receiver off its switch-hook directly the door opens would not affect the alarm signal in any way, as the microphone and receiver are not connected up to the station until the alarm has finished. If another alarm on that circuit should be operated whilst telephonic communication is being carried on, the duty man will get the distinct intermittent signal and code on his telephone receiver. If an alarm on another circuit is started, the signal from same drops its circuit flap and gives the duty man an intimation that an alarm-point has been operated.

A high-resistance buzzer operated by the alarm battery gives warning at the station so long as an annunciator-flap is down and no signal is coming in. An unmistakable audible warning is thus given to the duty man if he has neglected to replace a flap.

CALL-BELLS.

With non-resident brigades the men have to be summoned from their respective houses immediately an alarm is received at the station. The same thing holds good of course with resident firemen, as they have to be summoned from their quarters; but a description of the necessary arrangements for calling out a non-resident brigade will cover both purposes.

The whole of the men are not called out to a first alarm, the number depending upon the extent of the fire, and the circumstances of the case. The whole of the men are therefore split up into certain groups, say, A, B, C, D, and E, and these five groups are controlled by five corresponding circuits running out from the station board. Those groups that are required to turn out for a first call are switched on to the alarm-circuit by means of their group switches. Any signal now coming from an alarm-point will not only be heard and recorded at the station, but will be sent on to these call-bell lines by means of their circuit or circuits being closed by the lower contacts of the alarm-relay. These firemen will receive the unmistakable fire-alarm signal

consisting of a regular intermittent ring, and further (what is claimed as a distinct advantage on this system) each of those men will also get the code ring of the point operated. If this point should therefore happen to be nearer to his house than the fire station, a fireman can proceed direct to the alarm-point in question.

If, however, the alarm is received at the station by some other means than an alarm-point being operated, the alarm to the call-bells has to be given from the station, but in a manner which leaves the firemen no doubt that it is a genuine fire-call, and not a telephone ring, or a fault on line. This is arranged by installing an auto call-switch on the switchboard, and across the call-bell contacts of the alarm relay. This switch is merely a clockwork mechanism with a contact drum, which intermittently puts the call-bells through the battery to earth, and thus when running sends out the characteristic intermittent ring to the men. The pulling of the lever on this auto-switch winds up the clockwork, and releasing of the same allows the contact wheel to revolve with the afore-mentioned results.

In some cases it is thought advisable to install telephones at the firemen's houses. The arrangements shown in Figs. 10 and 11 provide for a common line to be run to all the firemen's houses. This is used only for speaking on, or calling up the station from any of the houses. This latter is automatically effected when the telephone is taken off its switch-hook, the circuit thus being earthed through the microphone and receiver, consequently ringing the telephone bell at the station. The pressing of the call-bell telephone-key will then put the station-telephone into circuit with the calling-up house. A man at any of the alarm-points can ask the station to put him into communication with any of the firemen's houses. The station would then call up the required house, and when he had obtained an answer would also depress the alarm telephone-key. Both telephone-keys thus being used together would then put the alarm-post into direct telephonic communication with the desired house.

The call-bells in the houses are used as the means of calling attention if the station wishes to speak to any of them. As, however, there is more than one bell on the circuit closed by its respective calling-up button, a prearranged simple code of signals is devised, say 1, 2, 3, and 4 long rings respectively, if four houses are on the circuit. Each house need then only answer to its own call.

A better arrangement, irrespective of expense, is to have each call-bell on a separate circuit from the station board, controlled by a two-way switch and ringing-up button. Any man who is away on leave, or from any other reason is not on the active list, need not then be called up with his group. Likewise any possible combination of the men can be called out at once. The telephones can of course work on the same lines as the call-bells, but a separate speaking-key or jack would be required for each circuit.

"Knight" Closed-circuit System.

This system is the same in principle as the "Knight" open circuit, but the method of connecting up the street fire-alarm points one to another is different, though the alarm-boxes themselves may be the

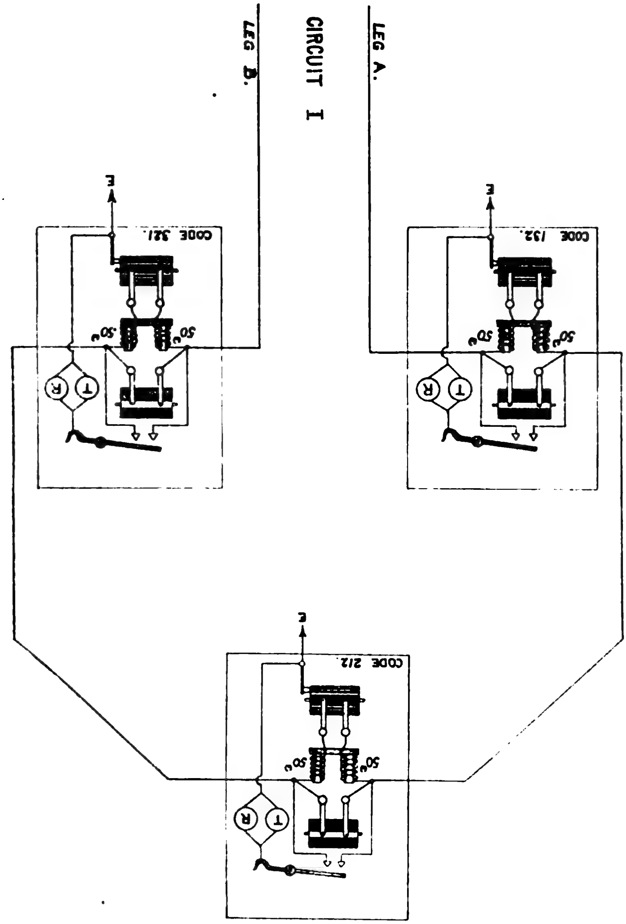
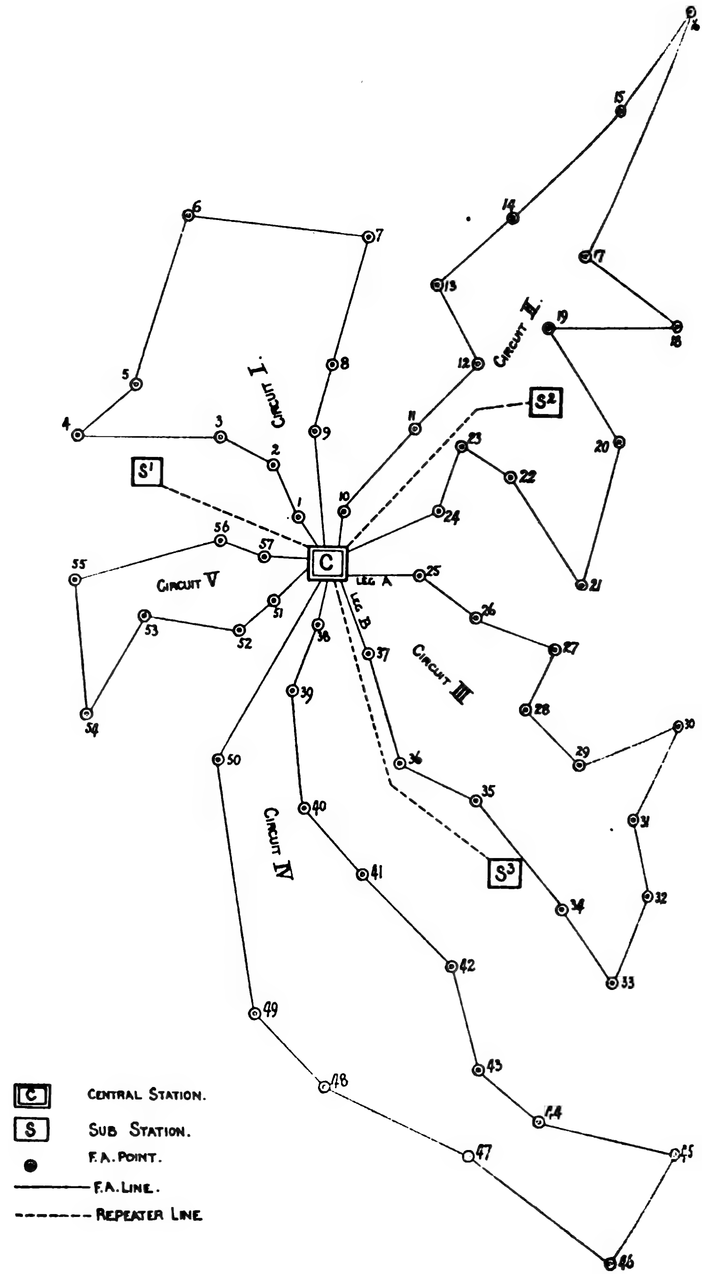


Fig. 12.

same in both systems. This system also automatically draws attention at the station if a line should break at any time. This is the only difference in working results that there is with the two systems.

Fig. 12 shows a circuit of alarm-points. These, it will be seen, are connected up in series on a loop metallic circuit. A current is constantly flowing round this loop, and any break in the circuit will affect



Circuit I.
Circuit II.
Circuit III
Circuit IV
Circuit V
Leg A
Leg B
C
S¹
S²
S³
Central Station.
Sub Station.
F.A. Point.
F.A. Line.
Repeater Line

FIG. 13.

certain instruments at the station in a manner to give an audible signal.

Figs. 13 and 14 give the lay-out for a number of points in a large town with three sub-stations. In Fig. 13 all the alarms ring into the central station, and the apparatus there will automatically repeat the district calls into the sub-station affected by them. Circuits 1 and 5 are repeated to sub-station S1; circuit 2 to sub-station S2, and circuits 3 and 4 to sub-station S3. On the other hand, in Fig. 14 the fire-alarms from the various points are localized, the alarm-points in one area ringing into one sub-station, and the alarms from another area into another sub-station. Here again all or part of the alarms running into one station may be automatically repeated to any other station if necessary. The latter method of alarms coming direct to sub-stations is unquestionably the best. It is likewise important that a repeating system should be able to distinguish and sort out certain signals for certain stations, so that directly a signal starts to come into a station that station knows that it has to turn out at once.

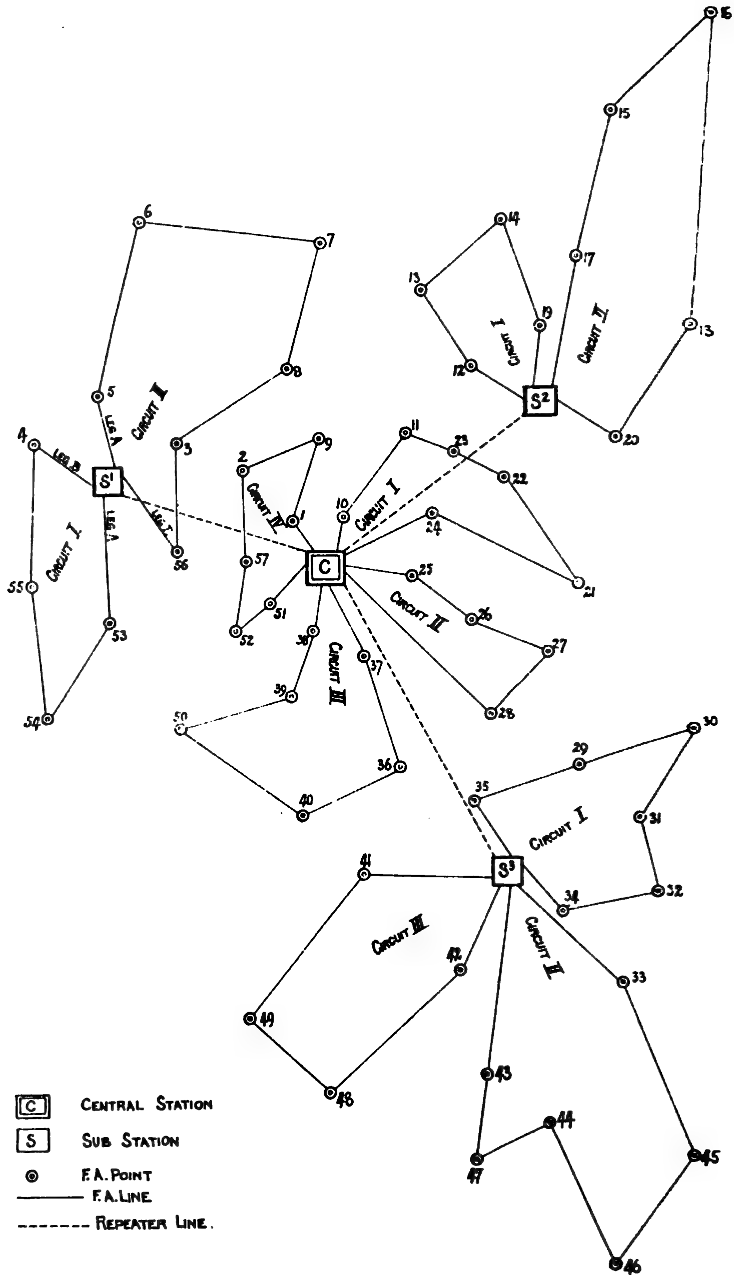
The station apparatus connections are shown in Fig. 15. The drop-annunciators, when energized sufficiently, fall and operate their respective circuit-changers. The fault indicators are held at one indication so long only as they are energized, and show a different indication if current is cut off from them. The fault switches are used when a fault proclaims itself on the line as hereafter described. The switching over of any fault switch, besides altering certain electrical connections, mechanically replaces all the fault indicators, or rather such of these as may be in the short-circuited position, and if current is flowing through this part of the circuit they will be held there electromagnetically.

Considering now the system as a whole, working under different conditions with which it may meet:—

(*a*) *Normal.*—The closed circuit, starting from the battery, goes through one coil of the alarm-relay, and branches out to all the disconnection-test indicators. Each disconnection-fault indicator then connects on to its respective A annunciator drop-flap, from there to its own circuit-changer and out to A line. Referring to Fig. 12, it will be seen that the circuit is closed through every alarm-point. The circuit enters the station again through leg B, across B circuit-changer through annunciator B and to the earth-fault indicator. This latter, however, is short-circuited by its manually-operated switch.

The whole of the B legs on all the circuits converge after passing their respective earth switches, and are joined across the B contacts of the control relay to the other coil of the alarm relay, thence to the other side of the battery, thus completing the circuit.

The fault indicators are wound to 2,000 ohms each, and as the A or earth-fault indicator is always normally in circuit, only a small current is then passing round each of the loops. This is not nearly sufficient to drop any of the annunciator-flaps or to actuate the alarm-relay. The whole system thus normally consists of closed metallic circuits, and no current is going to earth.



Central Station
Sub Station
F. A. Point
F. A. Line
Repeater Line.
Circuit I
Circuit II
Circuit III
Circuit IV
Leg A
Leg B

Fig. 14.

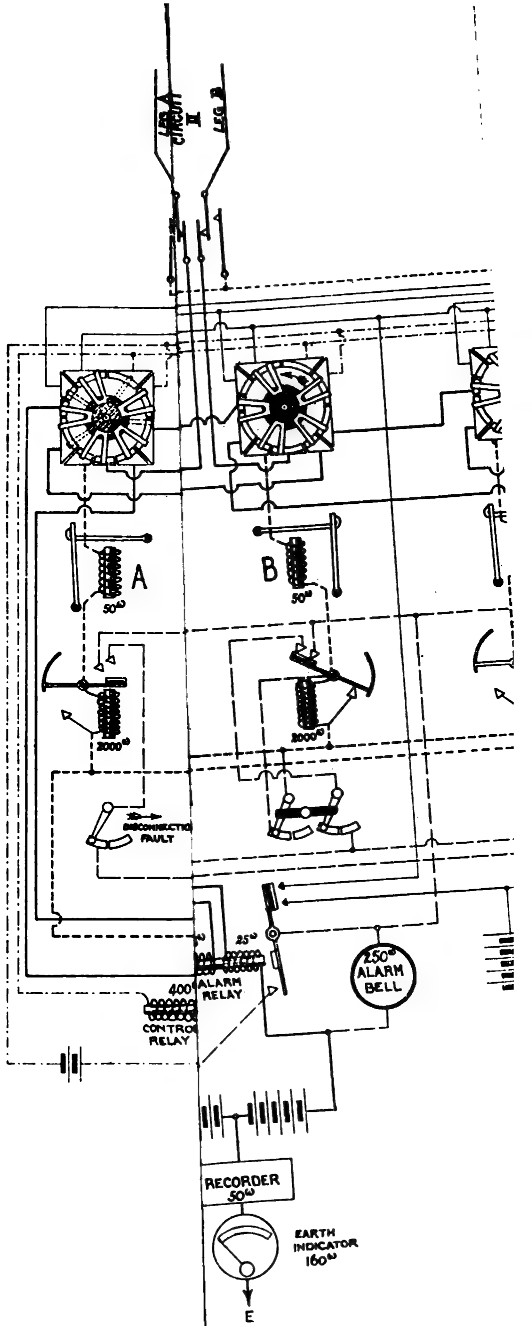

LEG A
CIRCUIT II
LEG B
A
50ω
B
50ω
2000ω
2000ω
DISCONNECTION FAULT
25ω
ALARM RELAY
250ω
ALARM BELL
400ω
CONTROL RELAY
RECORDER 50ω
EARTH INDICATOR 160ω
E

FIG. 15.

(*b*) *When a Point is operated.*—The circuit the point is on is momentarily broken at the extension-drum contacts, the disconnection-fault indicator is de-energized and short-circuits its own electromagnet winding, so that this high resistance is cut out of circuit. Immediately afterwards (the clockwork at the alarm-point operated continuing to revolve) the circuit is intermittently put to earth at the code-drum contacts. The full current will then flow down each leg of the circuit, dropping both its annunciator-flaps, through the alarm relay to the main battery. This battery is split in the middle, where both circuits converge and go to earth through the code-recorder and earth-indicator.

With the first full current, both annunciator-flaps drop and alter certain connections on their respective circuit-changes. No more current can now come in from B leg, as this line is switched on to A circuit-changer, which latter has now terminated B's line-connection there. The remainder of the intermittent code-signal and code can therefore only come into the station on leg A of the circuit, whence it now goes direct to the alarm-relay, A battery, through the recorder and earth-indicator as before, and then to earth.

The intermittent and code signal of the point operated are thus rung out on the alarm-bell by the closing of the alarm-relay contact, in synchronism with the earthing of the line by the code-drum in the street.

As may be seen from the diagrams of connections, the whole main battery is used for operating the alarm-bell when this relay-contact is closed, but only A half of the battery is used for operating the recorder. This latter instrument places on record every signal received, and the starting of its clockwork is utilized to break the fault-indicating circuit directly a signalling current is received, otherwise the alarm-bell would be ringing continuously as for a fault. Indeed, this is what actually happens when the complete alarm-signal has been received and recorded. When the recorder stops, the bell rings continuously until the fault indicator has been replaced. As the electromagnet of this is short-circuited until the annunciator-flaps are also replaced, the first-mentioned operation is not possible until the second has been performed.

(*c*) *Two or More Points on one Circuit operated at or about the Same Time.*—As previously explained, when an alarm-point is operated, its signal can only come in on A leg of the circuit, or rather this is so when the circuit is in proper or normal working order. All alarm-points farther away down the line from the station are cut off from the station by the extension-circuit-breaking drum of the alarm-point operated. If therefore another alarm-point farther away is operated whilst the first is sending in its signal, no current can reach it; and until it is again connected to the station battery by the nearer alarm-point finishing, it is held up by its electromagnet as explained in the open-circuit system.

This previous description will also explain what occurs when an

alarm-point is operated nearer to the station on the line than the one first started.

It will be noticed from Fig. 12 that the alarm-point held up is connected to the station every time that the code contact-drum of the nearer alarm-point connects its two contact-fingers together. Each time this occurs, however, the contact-fingers are simultaneously earthed, so that all the current from the station battery flows to earth at the nearer alarm-point, the held-up point being short-circuited and receiving no current. Thus any number of points operated on a circuit will automatically ring out and record themselves at the station one after the other.

(*d*) *Two or More Points on Different Circuits operated at or about the Same Time.*—Practically the same explanation applies as that given for the open-circuit system.

(*e*) *Repeating.*—Provision is made for repeating any or all of the signals received at a station to one or more other stations. This is accomplished by means of the circuit-changers and repeating contacts on the alarm-relay. The former select the circuits which are to send their signals on to the other station or stations, and the latter makes and breaks this repeating circuit in synchronism with the original alarm-signal. By judicious arrangement of the various alarm-points in a large town, all points which have to call out another station besides the one they first ring into, may be connected to one circuit. Thus there is no necessity with this system for any stations to receive an alarm-call or an automatically repeated call unless that station has to turn out to it. By this method, on the first strokes of the warning intermittent signal being received, the men can be assembled, and by the time the code signal has rung they are nearly ready to be off to the fire.

Faults.

(*f*) *An Earth Developing or Extraneous Current on Line.*—The same explanation given for the "Knight" open-circuit system applies so far as the results of an earth are concerned. The temporary remedy, however, is different with this closed-circuit system. The earth-switch is put to the fault position. This does three things: (1) It mechanically puts the B fault-indicator to show "earth"; (2) the short-circuit is taken off this indicator, the high resistance of its electromagnet is put into circuit, and as it forms part of the closed metallic circuit, the indicator will be held in this position irrespective of any current it may be receiving due to the faulty earth; (3) the fault-alarm circuit is switched on. The circuit now has a resistance of at least 2,000 ohms between either side of the battery and the earth fault, and even a "dead earth" will not pass sufficient current to operate the annunciator-flaps or alarm. Directly an alarm is operated, however, the line is momentarily broken and either A or B fault indicator is de-energized for a short period, thus

allowing it to short-circuit itself and cut out its resistance. Which of the two fault indicators is thus de-energized depends upon the alarm-point operated, and the locality of the earth. Obviously that indicator will remain unaffected which has nothing to break its circuit between itself and the earth-fault. Under these conditions the signal will therefore come in on that leg of the circuit which has the operated alarm between the station and the earth-fault. The alarm-point having been operated, the earth-fault is cut off from this leg by the extension-line contact-drum; and the high resistance at the station on this leg having been cut out, the subsequent intermittent earthing of the contact-fingers on the code-drum will send the signal through to the station.

(*g*) *A Line Broken and not Earthing.*—In this case A fault indicator drops away, giving a visual indication "broken circuit," and ringing a bell. It also cuts out the high resistance of its electromagnet. The A fault switch is put over, thus stopping the bell. Any alarm-point now being operated will come in on that leg of the circuit which has not the break between the point operated and the station.

(*h*) *A Line Broken and Earthed.*—If a line breaks and both of the broken ends become earthed, the result is of course the same as when a line is earthed without breaking. If, however, when the line breaks, only one of the broken ends becomes earthed, two cases arise: (1) leg A earthed; and (2) leg B earthed.

(1) A certain period of time, even if extremely short, must elapse after the break and before the broken end becomes earthed. Directly there is a break, the fault-indicator on leg A will fall away, indicating "line broken," closing the alarm-circuit and giving a continuous ring. The subsequent earthing of this leg will have no further effect.

(2) The A side will act as described in (1), but B side will also close the bell-circuit through the alarm relay-contact.

The temporary remedy for (1) is to change A switch over. This disconnects the alarm-bell from that fault-circuit, and any alarm that side of the break will come in on A side, whilst any alarm on the unearthed side B will come in on the B side.

When (2) occurs, the changing over of A switch will not stop the bell ringing. The other fault-switch, therefore, being put over, the effect is as described in (*f*). The system is now again in working order, and will receive any calls, and the faults can be remedied as soon as convenient.

(*i*) *Two Legs of a Circuit in Contact.*—The method of joining up the boxes in all closed-circuit systems, viz. by loop-lines, generally obviates the necessity of running the out or A line on the same route as the in or B line, so that unless the two lines do run close to each other there is no chance of them making contact.

With the system now being described, however, even if the two lines on a circuit become short-circuited, any point operated on that circuit under these conditions will ring into the station—provided, of course, that the lines are otherwise in order. But as any other fault will

proclaim itself, it is practically impossible for any of the alarm-points or lines to be in such a condition that they are not capable of sending an alarm in when a point is operated, without the station knowing of the fact and taking steps accordingly.

(*j*) *Telephonic Communication.*—This is provided in a very simple manner similar to that in the open-circuit system. The receiver being taken off the hook, or the hand set plugged into the fire-alarm jack, the loop is earthed and the bell rings at the station. A speaking-key is provided for each circuit at the station. The automatic telephone-switch at the station breaks the fault alarm-circuit.

THE NATURE OF DIELECTRIC FATIGUE.

By W. HOLTTUM, B.Eng., Student.

(*Paper first received* 21*st December*, 1912, *and in final form* 8*th March*, 1913.)

SUMMARY.

Method of testing.
Details of apparatus.
Test results and deductions therefrom.
General conclusions.

METHOD OF TESTING.

The following hypotheses of the nature of dielectric fatigue were made the basis of the method of testing adopted. A sample of insulating material, say a sheet, has initially a certain instantaneous strength—defined as the greatest difference of potential which it is able to withstand between its two sides for a very short period of time. It is reasonable to assume that the instantaneous strength will have, at any moment, some value between its initial value and zero, depending upon the treatment to which the sample has been subjected; and that one feature of a state of fatigue is that the instantaneous strength is below its initial value.

Suppose, then, that an alternating pressure is applied continuously to cause a pressure gradient through the sheet. If fatigue results, then it may be said that the instantaneous strength has been reduced; and one way of measuring the fatigue would be to measure the instantaneous strength. It is also reasonable to suppose that for a given sample there is a certain minimum pressure below which there is no fatigue. Also there is the obvious practical limitation to experiment that a sample cannot be stressed until its instantaneous strength is less than the crest value of the applied pressure; nevertheless, we may suppose that from sufficient data the curve showing the relation between the instantaneous strength and the time of application might be continued beyond the point of breakdown.

The purpose of the investigation was thus to determine the relation between the duration of application of the pressure and the instantaneous strength when identical samples were tested at constant pressure and

frequency. Alternating pressures were used for convenience and also because it is generally supposed that it is not so much the stress as the change of stress that causes the decrease in strength.

Previous attention which this subject has received * has consisted in applying an alternating pressure and measuring the time that elapses before breakdown, the results giving a series of curves (1) between the pressure and the length of time to breakdown at constant frequency, and (2) between the frequency and the time, at constant pressure. It is obvious that however many readings are taken it is impossible to find a relation between the time and the instantaneous strength.

The first necessity, therefore, was to decide upon some method of determining the instantaneous strength, or some quantity equally informative. It would obviously be impossible to measure the pressure which the sample would stand for an infinitely short time; an arbitrary

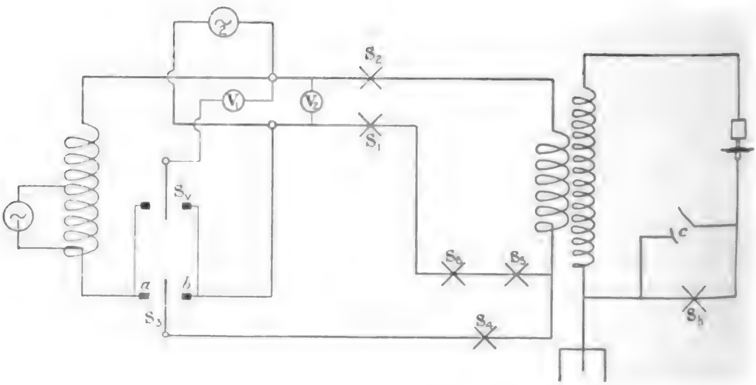


FIG. 1.

standard was therefore decided upon, viz. the pressure of a certain wave shape which would just cause breakdown if applied for $\frac{1}{10}$ sec. at 50 frequency, and the expression "instantaneous strength" is used in this paper with that meaning except where another frequency than 50 is under consideration.

The electrical pressure for carrying out the tests was obtained from a single-phase transformer having a ratio of 100 : 1 and giving a maximum pressure of 20,000 volts (R.M.S.).

Fig. 1 shows the principal connections, the two field regulators which were arranged close at hand being omitted from this diagram. V_2, S_1, and S_2 are fixed on a panel as part of the permanent transformer connections. Two motor-generators were used: (1) to apply the fatiguing pressure, and (2) to apply the $\frac{1}{10}$ sec. breakdown pressure. V_1 is a precision voltmeter for use on machine (1), and is connected

* Langsdorf, *Electrical World*, vol. 52, p. 942, 1908

through a two-way switch S_v so that the reading of the voltmeter V_2 can be periodically checked. The pressure was measured only on the low-tension side, the ratio of transformation having previously been ascertained to be the same for all induction densities inside the working range.

The method of testing was as follows :—

The switches being ready, *i.e.* S_1, S_2, S_5, and C open, and S_4, S_6, and S_A closed, and the rheostats having been set to give the desired pressure on each machine—the drop of pressure caused by switching on the transformer being allowed for—the sample is placed in position and the switches S_1 and S_2 are closed. The pressures are observed again and the rheostats adjusted if necessary. The switch S_3 is closed to contact *a* and the time noted on a stop-watch. Machine No. 1 is now connected to the transformer, and the sample is subjected to an alternating pressure V_1. At the end of the intended fatiguing period t_1, S_A is opened, and S_4 is opened and S_5 closed in quick succession; then approximately 0·3 sec. from the instant of opening S_A the contact C is made and held for $\frac{1}{10}$ sec., thus applying the pressure V_2 from machine No. 2. Switch S_6 is then pulled out, and S_1 and S_2 are opened at leisure, and the state of the sample—that is, whether broken or unbroken—is noted. The mode of operation of the switches will be described later.

To find the instantaneous strength of a sample after fatigue for t_1 sec. at the pressure V_1, a value of V_2 was chosen and a sample tested as described; if the sample broke another was tested with a lower value of V_2. A sample was generally discarded after test, whether broken or not, as the effect of the test on an unbroken sample being unknown the result of any further test on it would have therefore been unreliable. It was sometimes desirable, however, during the first few tests with each pair of simultaneous values of V_1 and t_1, when the required value of V_2 was not known even approximately, to repeat the tests on any unbroken samples, as an indication whether the value of V_2 chosen was near that required or not. The choice of the value of V_2 for each test was based on the previous results, until a value was found at which an approximately equal number of samples were found to be broken and unbroken after test. This was the required value of V_2. The number of samples necessary to find one such value varied from 7 to 22, the average being 14. It was generally assumed to be conclusive if out of 6 or 8 tested with the same value of V_2 one-half were broken and one-half were not. It should be possible to obtain in this way for any frequency and at each pressure a curve of instantaneous strength on a time base, the frequency of machine No. 2 being maintained constant at 50. Hence if 10 frequencies and 10 pressures were selected, 100 curves would be required for a complete set of results, and allowing, say, 8 points to each curve and 12 samples to each point, this would require the testing of $100 \times 8 \times 12 = 9{,}600$ samples. It was expected, however, that from a much smaller number a sufficient indication of the nature of the curves could be obtained.

All the testing was done under oil, the principal reason for this

being that during a test in air there is a considerable and rapid rise in temperature, easily noticeable to the touch after the pressure has been applied for a few seconds. This rise in temperature probably affects the strength, and though it might be regarded as one of the factors contributing to the phenomenon of fatigue, the object was to get information about fatigue other than that due to heating. Testing under oil effects this to a certain extent, by cooling and by preventing the brush discharge round the edges of the electrodes. It is open to the objection, however, that prolonged submersion in oil is likely to increase the instantaneous strength; but as the test pressure was applied immediately after placing the sample in position, the period of submersion before the end of the test was generally not more than half a minute. The materials tested consisted of ebonite $\frac{1}{2}$ mm. thick and presspahn 1 mm. thick. The ebonite was found to have its instantaneous strength increased by about 2 per cent after 5 minutes' soaking in the transformer oil used, whilst the strength of the presspahn increased about 5 per cent after soaking for 1 minute. The presspahn, however, was generally tested less than 10 seconds after immersion.

Details of Apparatus.

The switches S_4, S_5, S_6, S_k, and the contact C were automatically operated. The reasons for doing this were: (1) to ensure that the pressure V_2 was applied as soon as possible after the removal of the pressure V_1 in order to avoid possible recovery from fatigue, and also that the interval should be the same in each test; and (2) to arrange that the contact C should be maintained for the exact time required, and, what is more important, always for the same length of time. This was done by making S_4, S_5, and S_k special quick-acting switches, operated by releasing catches—S_6 does not require to be quick-acting but was automatically worked for convenience, its object being to put the transformer out of action as soon as the test was over. These switches were arranged side by side under a shaft, from which projected radial arms of such a length as to trip the switches. The shaft was provided with a drum round which a string suspending a weight was wound: and four balance weights served the purposes of balancing and supplying the moment of inertia. A view of the apparatus is given in Fig. 2.

One radial arm engaged with a catch, which when released left the shaft and its attachments free to rotate under the pull of the string. A brake was fitted to the under side of the drum to bring the rotating part to rest when the switches had all been operated. The apparatus was controlled from another table, 5 ft. away, by two wires, one to release the catch and one to put on the brake. The brake once on was kept in operation by a spring catch on the control table until released. After a test each switch was set by hand, and the string was unhooked from its peg on the drum, taken one turn round the latter, and hooked on again (the switches were in the way of the shaft being rotated backwards to wind the string on the drum).

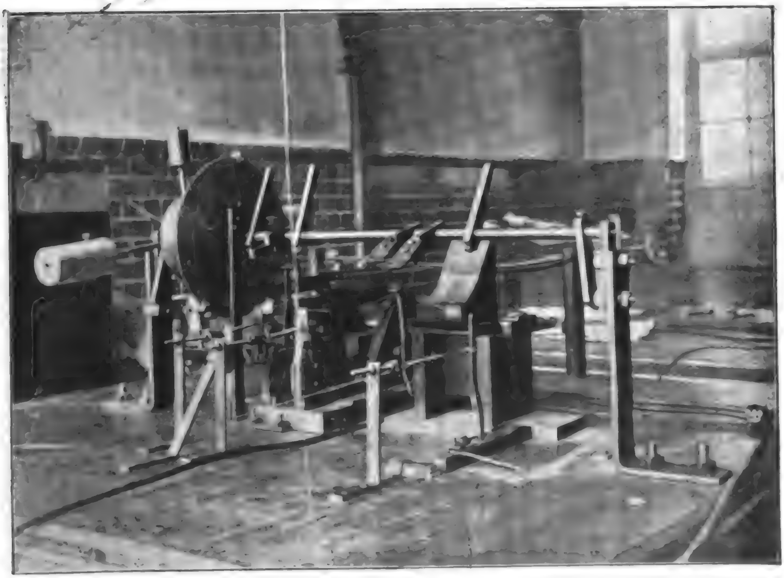


FIG. 2.

The principal features of interest are the design of the switches S_4, S_5, and S_6, and the contact C. Fig. 3 shows S_4, which operates as follows: The catch *a*, pivoted at the bottom, on being knocked back

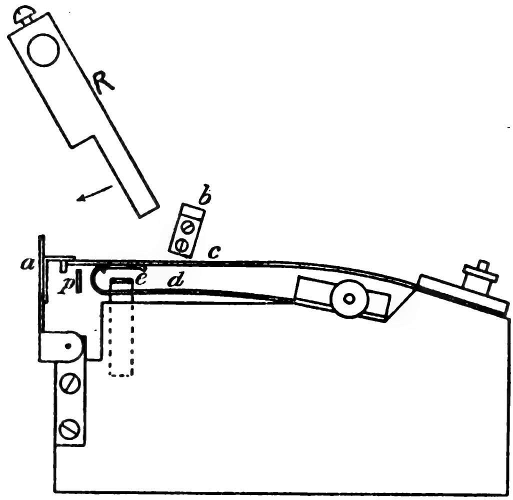


FIG. 3.

by the rotating lever R releases the upper spring *c*, which flies up and has its vibrations immediately stopped by the rubber-padded buffer *b*.

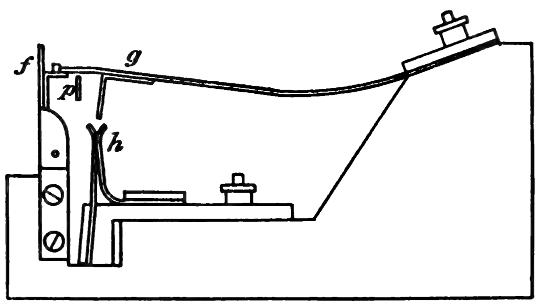


FIG. 4.

The lower spring *d* rises about $\frac{1}{8}$ in. as far as the stop *e*, the contact then being broken by the two springs separating.

Fig. 4 shows S_5. In this *f* is a similar catch to *a* in Fig. 3, except that it holds a spring up instead of down. When this is knocked back

the spring *g* descends, the lower projecting slip passing between the two springs *h* and closing the circuit. As these springs are made of hard brass strip $\frac{3}{8}$ in. × $\frac{1}{16}$ in., it is obvious from the proportions of the switch (the rotating lever above being $3\frac{1}{8}$ in. long from centre to end) that the actions of S_4 and S_5 when the catches *a* and *f* are released are practically instantaneous. S_6 is an ordinary knife switch placed between S_4 and S_5, which are close together. In order to prevent S_4 and S_5 being closed at the same time, thus paralleling the two alternators if the other switches were closed, a lever was pivoted on the support of S_6, the ends of it being shown at *p* in Figs. 3 and 4: hence S_5 could not close until S_4 had opened and allowed its end of the lever to rise.

Fig. 5 shows the switch S_h and the contact C. The latter consists of a stationary contact arc, and two brush arms on the shaft. One

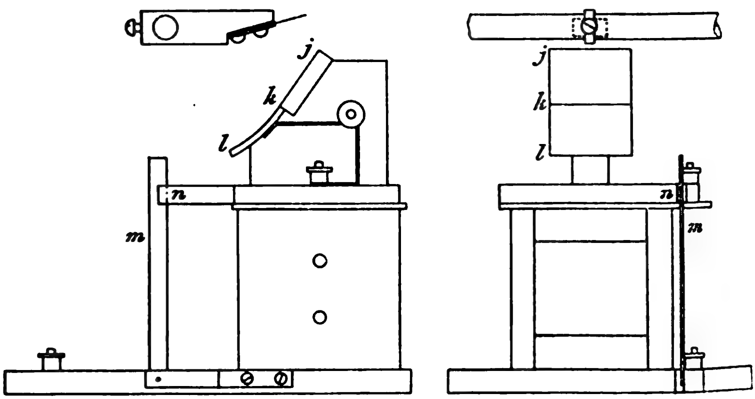


FIG. 5.

brush arm and brush are indicated; *j k* is an ebonite guide plate which the brush traverses first, and *k l* is the brass contact arc, these being supported on an ebonite stand. The high-tension circuit through C was completed by a fixed brush rubbing on the shaft, which was earthed as shown in Fig. 1. The effective contact arc could thus be made any size between once and twice the length of the fixed arc by altering the angle between the brush arms.

The brushes were of springy brass foil, $\frac{3}{4}$ in. square, projecting. They had each 5 longitudinal cuts in them, thus making six independent brushes barely $\frac{1}{8}$ in. wide, and ensuring a good contact. The switch S_h is operated by the arm *m* being knocked from between its holding spring contacts *n*, which are also attached to the ebonite stand. On being released *m* is instantly brought to the horizontal by a spring at its hinge at the bottom. A spiral spring was attached to each of the catches *a* and *f*, and to the arm *m*, tending to operate them, and was

adjusted to such a tension that the catches were on the point of being operated; in this way the force to be applied by the rotating part was reduced to a minimum. The method of calculating the angles at which the radial switch arms had to be set is explained below. In order to set these, a metal disc, 7 in. in diameter, was fixed to the side of the drum, and the desired angles were marked round this disc with brass indices. A pointer was fixed to the base of the machine, and each arm was set to operate its switch when its index came opposite the pointer.

The switchgear was arranged to act as desired in the following manner: It was found convenient that the first revolution should take 1·75 secs. Assuming uniform acceleration this means an acceleration of 4·1 radians per second per second. The arc of the high-tension contact was made 0·5 radian long, and thus any angle of contact from 0·5 to 1·0 radian could be obtained. An angle of 0·6 radian proved convenient. Starting from this the following table of action of the machine was calculated:—

Time from Release.	Angle turned through.	Operation.
Sec.	Deg.	
1·127	149·5	H.T. break
1·157	157·0	L.T. break
1·164	159·3	L.T. make
1·414	234·9	H.T. make
1·514	269·3	H.T. break

The switch arm for S_6 was set to operate S_6 when the high-tension contact was well opened.

The moment of inertia and frictional resistance to motion were then found, showing that a weight of 388 grammes was required to give the necessary acceleration. The moment of inertia, I, was 413,000 gramme-cm.^{2} and the friction, F, was equivalent to a pull of 40,000 dynes in the string.

For the comparison of the test results obtained, it is only necessary that the time for the high-tension contact and the lengths of the various intervals should be the same for every test, and whether they have the exact values intended, so long as they are reasonably close to those values, is really of no importance. Assuming a possible error of 10 per cent in the value of I and of 100 per cent in the value of F, the consequent variation in the time of holding on for the high-tension contact is only 8 per cent. The only cause that can produce a variation in this time during the tests is an alteration in the value of F. If this quantity were to double in value or to decrease to one-half, the variation

in the time of the high-tension contact would only be 5 per cent, and an alteration of 10 per cent in F would alter the time less than 1 per cent. In actual working there is included with F the resistance due to operating the switches and the high-tension contact. This is something like a force of 150,000 dynes acting through an angular distance of $\frac{1}{10}$ radian, which is equivalent to 3,200 dynes acting throughout the motion, so that the above considerations are sufficient to show that no trouble would arise from irregular working of the switchgear.

The electrodes were turned from brass plate $\frac{5}{16}$ in. thick, and had rounded edges of $\frac{1}{4}$ in. radius, the bottom one being $2\frac{1}{8}$ in. outside diameter and the top $1\frac{1}{2}$ in., so that the diameters of the areas in contact with the sample were $1\frac{5}{8}$ in. and 1 in. respectively. The upper disc carried a lead weight, making the pressure on the sample $4\frac{1}{4}$ lb.

It may be questioned whether the 0·25 sec. allowed between the closing of the low-tension and high-tension circuits is sufficient for the transformer to settle down to normal working. In view of the absence of all knowledge of how the instantaneous strength recovers after the removal of the fatiguing pressure, it was necessary to make this period as short as possible; but lack of time prevented the effects of different intervals being tried. The worst case of switching on occurs when the primary circuit is closed at an instant when the pressure is zero and the remanent magnetism is a maximum in the same direction to that in which the flux is required. The consequence of this would be that there is a rush of current which may affect the secondary potential difference in two ways: (1) By demagnetizing the alternator and causing a drop in the primary pressure, and (2) by introducing a considerable resistance drop in the primary and so reducing the electromotive force to be induced. Oscillograms were taken of the primary pressure and current at closing the primary circuit on both machines, and showed that when switching occurred at zero pressure the pressure wave became normal in less than one cycle. This takes no account of remanent magnetism, but it could not cause by itself any serious disturbance of the pressure wave.

This disposes of the first possibility; the second is settled by the following considerations. The current wave does not require to arrive at its normal value before the pressure wave practically coincides with its final form; all that is necessary is that the loss of pressure due to the magnetizing current shall not be considerable; this will be the case for the 10 cycles or so allowed. The consistency of the results is sufficiently conclusive that there can be no serious disturbance from this cause.

TEST RESULTS, AND DEDUCTIONS THEREFROM.

Ebonite* was chosen for the first tests because of its probable homogeneity and uniformity. It was found, however, that it cannot be obtained in large quantities with absolutely uniform thickness, and the $\frac{1}{2}$-mm. pieces ordered, 7 cm. square, varied from 0·48 to 0·58 mm. in

* The ebonite used was the quality N.N. of Messrs. H. Traun & Sons.

thickness. Hence the samples had to be sorted into their different thicknesses, 0·45 mm., 0·46 mm., 0·47 mm., etc., and tests made on one thickness at a time. This had the advantage that the instantaneous strengths for different thicknesses could be found. The samples were all tested with no other preparation than wiping with a dry soft duster.

Fatigue tests were carried out on pieces 0·48 mm. thick; and 33 samples were tested continuously to breakdown, with the wave shape from machine No. 1 in order to find a suitable fatiguing pressure. The period before breakdown was found to be longer the lower the pressure used, but it was also much more variable as found by Mr. E. H. Rayner.*

For convenience the symbols V_1 and V_2 will be used to signify R.M.S. kilovolts when the transformer is being supplied by machines Nos. 1 and 2 respectively. It was naturally supposed that the pressure giving the most uniform time to breakdown, provided such time was sufficiently long, would also yield the most consistent fatigue results; and so a fatiguing pressure of 13·4 V_1 at 50 frequency was first selected, this causing breakdown in about 17 secs. on an average.

TABLE I.

Duration of Fatigue in seconds.	V_2 kilovolts (R.M.S.).	V_2 (Crest) kilovolts.	Kilovolts per mm. (Crest).	No. of samples tested.
0	14·6	23·7	49·3	19
1	14·5	23·5	48·9	13
5	14·4	23·3	48·5	11
10	14·3	23·1	48·2	11

Although good agreement was obtained between all the tests at 1 sec. and 5 sec., the 10-sec. tests were far from consistent, 5 of the 11 samples breaking down before the 10-sec. period had elapsed, and 3 having a residual strength of over 14·3 V_2. Further tests at 15 sec. were made with still more uneven results, 11 out of 19 samples breaking in less than the 15 sec., and 5 of the remaining 8 having a residual strength of over 14·2 V_2. The highest pressure which could be applied indefinitely without breakdown was 12·6 V_1. Hence the maximum amount of weakening possible was $\frac{16{\cdot}2 - 12{\cdot}6}{16{\cdot}2}$, or 22·2 per cent, 16·2 V_1 being the initial instantaneous strength. The above results and others not recorded here show that when the weakening, or fatigue, becomes greater than about 1·7 per cent it is too uncertain to measure with any precision.

* *Journal of the Institution of Electrical Engineers*, vol. 49, p. 11, 1912.

The fact that some samples break before the fatigue time has elapsed while others have still a residual strength within 1 or 2 per cent of the initial instantaneous strength shows one of two things (or possibly a combination of both): (1) The instantaneous strength recovers very rapidly after the removal of the fatiguing pressure, *i.e.* in the 0·29-sec. interval before the application of the second pressure; or (2) what seems more probable, the instantaneous strength decreases very rapidly immediately before breakdown; that is, the curve of instantaneous strength would appear to be of the form shown in Fig. 6. Fatigue tests could then be made to about the point *a*, but after that no certain information could be obtained, because of the variableness

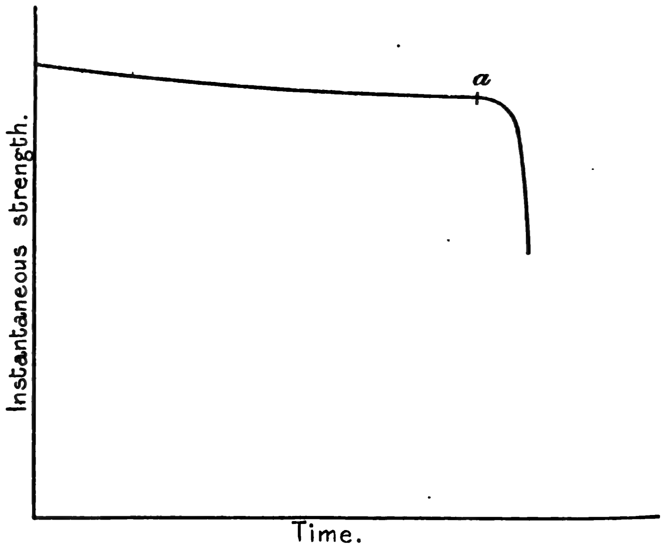


FIG. 6.

of the time that elapsed before breakdown for the different samples. It was expected that the decrease of instantaneous strength when the point *a* was reached would have been a much greater proportion of its initial value. Had that been the case, and had it been sufficiently uniform to be accurately measurable, tests would have been made to find the rate of recovery of strength after fatiguing. Similar tests were made on presspahn, but the results were much less consistent than the above, so much so that no deductions could be made from them.

Two samples of ebonite 0·5 mm. thick were tested for longer periods at the highest pressure which did not give early breakdown, to see if any fatigue was noticeable. In the first a pressure of 13 V_1 was applied for $4\frac{1}{4}$ hours, and then the pressure was gradually raised to 15·2 V_1 in 8 sec., the sample breaking at that pressure.

The instantaneous strength with machine No. 1 should be about 16·6 for this thickness. Considering how slowly the pressure was raised, this does not indicate any fatigue previous to the raising of the pressure. The other sample had a pressure of 13·1 V_1 applied for 4¼ hours, instantaneous pressures of 14·5, 15, and 15·5 V_2 being subsequently applied by the switch device; the sample, however, remained unbroken. The pressure was then raised gradually to 17·5 V_1, at which point breakdown occurred. This appears to be an abnormal sample.

The switchgear was then utilized to find the initial instantaneous strength of various thicknesses of ebonite and presspahn at various frequencies. These results showed a much greater degree of consistency, and since the breakdown is caused by about 5 alterations, more or less according to the frequency, they may be regarded as fatigue tests, though not giving information on the lines originally intended. An examination of the detailed tests from which the results were deduced leads to the conclusion that the limit of error in the figures given is not more than 0·1 kilovolt. The following figures were obtained for ebonite, and are arranged in the order of frequency:—

TABLE II.

Thickness in mm.	Frequency.	V_1 R.M.S.	V_1 Kilovolts per mm. (Crest).	V_2 R.M.S.	V_2 Kilovolts per mm. (Crest).
0·48	50	16·2	49·2	—	—
0·56	50	17·6	45·8	—	—
0·48	60	15·8	48·0	—	—
0·49	34	—	—	15·7	51·9
0·54	34	—	—	16·6	49·6
0·46	40	—	—	14·9	52·5
0·50	40	—	—	15·6	50·4
0·57	40	—	—	16·5	46·7
0·47	45	—	—	14·9	51·2
0·51	45	—	—	15·8	50·0
0·57	45	—	—	16·4	46·6
0·48	50	—	—	14·6	49·3
0·56	50	—	—	16·0	46·1
0·45	60	—	—	14·6	52·3
0·49	60	—	—	15·6	51·3
0·55	60	—	—	16·3	47·8

The average number of samples per test was 13. The temperature of the room did not vary more than 3 deg. C. throughout the tests, and three of the above results were checked at a later date and found correct, so that they are all comparable.

The following are the principal points to be noticed in connection with Table II :—

1. Three thicknesses were tested for each of the frequencies 40, 45,

and 60. Taking the equation $T^x = KV$, where T is the thickness, K a constant, and V the instantaneous strength, 0·46, 0·62, and 0·55 are calculated as the mean values of x for the above frequencies respectively. The mean of these values is about 0·5, the figure generally accepted.

2. On considering the variation of the breakdown pressure with frequency, it appears that the lowest pressure is required at 50 frequency. The natural assumption, of course, is that the pressure required for breakdown would be greater the lower the frequency, but this appears to hold only below 50 cycles. That the instantaneous strength above this frequency is greater requires more strict investigation before acceptance.

3. The comparison of the figures for V_1 and V_2 in Table II is interesting in view of the wave shapes of the two machines. Machine No. 1 is a 1-h.p. generator with a rotating drum armature, and a perfectly smooth wave shape, amplitude factor 1·456. Machine No. 2 is a 10-kw. generator with a rotating field and a very uneven wave shape, amplitude factor 1·615. The load of the transformer running light was found to be insufficient to alter the value of the amplitude factor; the degree of saturation of the field did not alter it, probably because the poles had no projecting shoes, and at the highest pressures required they were not saturated; it was likewise unaffected by frequency. The figure 1·456 would be less likely to alter in view of the nature of machine No. 1. The part of Table II which is of interest in this connection is reproduced in Table III:—

TABLE III.

Thickness in mm.	Frequency.	V_1		V_2	
		R.M.S.	Kilovolts per mm. (Crest).	R.M.S.	Kilovolts per mm. (Crest).
0·48	50	16·2	49·2	14·6	49·3
0·56	50	17·6	45·8	16·0	46·1
0·48	60	15·8	48·0	—	—
0·49	60	—	—	15·6	51·3
0·55	60	—	—	16·3	47·8

It will be seen that the crest values of V_1 and V_2 agree fairly closely for each thickness at 50 frequency, and not quite so well at 60. When samples were tested continuously to destruction, a crackling sound generally commenced some little time before breakdown, accompanied by a visible discharge at the sample, and this usually

continued uniformly until breakdown. The following average figures were found :—

TABLE IV.

Thickness in mm.	Frequency.	Kilovolts per mm. (Crest).	Total Time in sec.	Duration of Crackling in sec.	No. of Samples.
0·48	40·0	40·6	93·0	11·80	9
0·48	50·0	40·6	11·1	10·30	12
0·52	50·0	38·6	28·7	11·17	6
0·48	66·7	40·6	12·1	6·43	7

We see that the figures for the time of crackling are much more regular in their relation to each other (*i.e.* diminishing slightly as the frequency increases) than the total time to breakdown. There was also less difference in the duration of crackling for the individual samples than in the total time.

The most likely explanation of the phenomenon seems to be that it is due to the expulsion of the oil from the surface of the sample over a small area at the edge of the smaller electrode, this being the place where the sample ultimately broke. It seems possible that the crackling sound may indicate the absorption of power by the sample under test. It is shown by Mr. Rayner in the paper already mentioned that as the point of breakdown is approached the amount of power absorbed increases.

The following figures were obtained from the tests on presspahn :—

TABLE V.

Thickness in mm.	Frequency.	V_2 R.M.S.	V_2 Kilovolts per mm. (Crest).	Oil Temp. deg. C.
1·04	40	18·4	28·6	15·90
0·98	50	16·8	27·7	15·90, 15·65 †
1·03	50	18·6	29·2	15·65
1·04	50	18·6	28·9	15·90
1·06	50	17·5*	26·7	15·10
1·08	50	17·6	26·3	15·65
1·00	60	18·1*	29·2	—
1·03	60	17·9	28·1	15·90
1·04	60	18·2	28·3	—

* Doubtful readings. † Test made at each temperature.

The average number of samples per test was 16. These figures show less consistency than those for ebonite, but it was possible to find quite definite values for the instantaneous strengths except in two cases.

GENERAL CONCLUSIONS.

1. That fatigue due to electric stress alone is almost negligible for ebonite and presspahn until the point of breakdown is approached, the greatest amount measured for ebonite being 2 per cent of the initial instantaneous strength (as defined above).

2. That the least pressure which will cause breakdown if applied continuously is about 80 per cent of the lowest measured value to which the instantaneous strength can be reduced by fatigue.

The conclusion of greatest practical importance that can be drawn from these tests is that the dielectric strength of ebonite for sudden stresses lasting for a fraction of a second is only some 28 per cent greater than it is for long-continued stress, and that any ebonite used for insulation purposes should be designed for the greatest pressure which may occur as though that pressure were continuous.

The work here described was carried out at the Laboratories of Applied Electricity, Liverpool University, under the supervision of Professor E. W. Marchant, to whom the author wishes to express his thanks for valuable advice throughout the work. He also wishes to thank Mr. F. Mercer, B.Eng., for assistance in making some of the tests, and for doing the oscillograph work.

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